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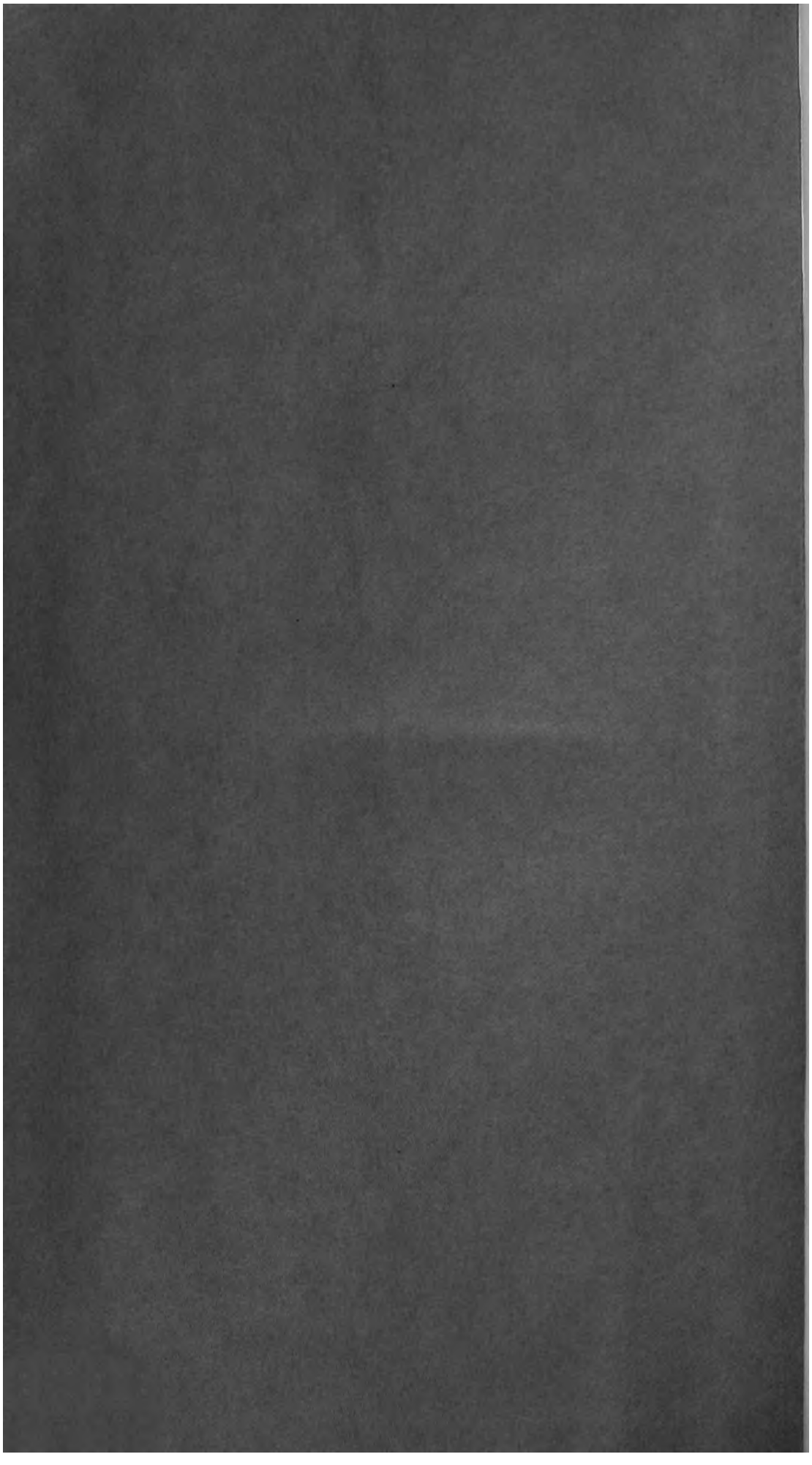
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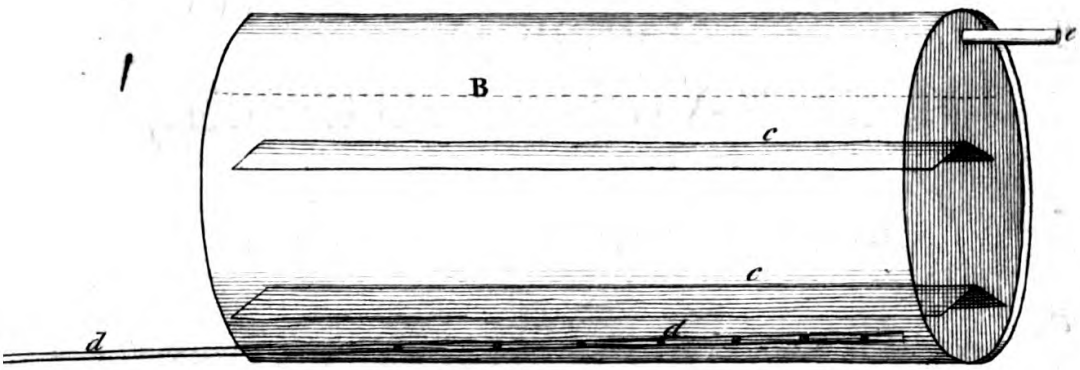
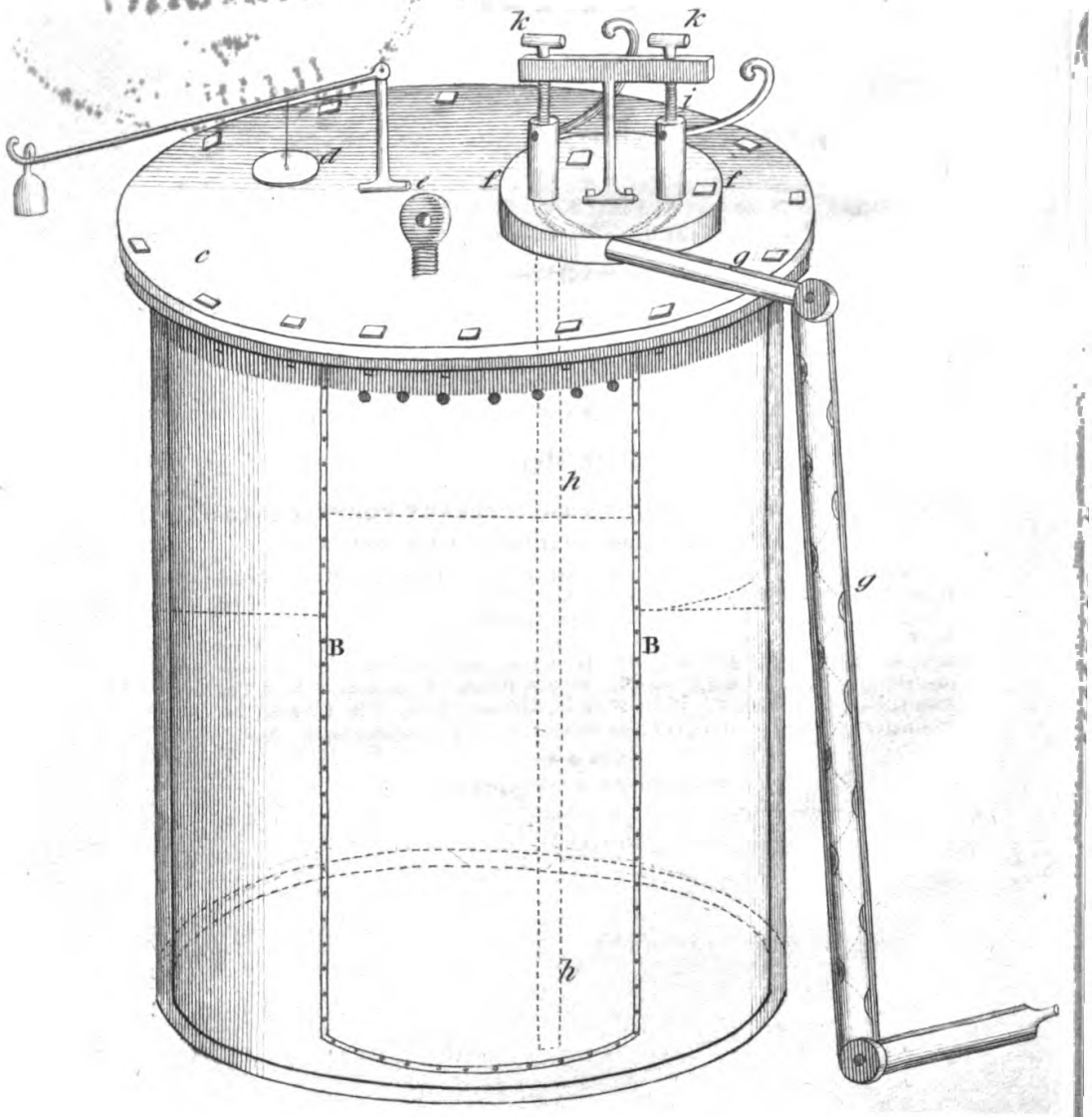


Fig.2.



THE

AMERICAN

JOURNAL OF SCIENCE,

MORE ESPECIALLY OF

MINERALOGY, GEOLOGY,

AND THE

OTHER BRANCHES OF NATURAL HISTORY;

INCLUDING ALSO

AGRICULTURE

AND THE

ORNAMENTAL AS WELL AS USEFUL

ARTS.

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Professor of Chemistry, Mineralogy, &c. in Yale College; Author of Travels in England, Scotland, and Holland, &c.; and Member of various Literary and Scientific Societies.

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Geology

ADVERTISEMENT.

IN the following plan of this Work, we trust it will be understood, that we do not pledge ourselves that all the subjects mentioned shall be touched upon *in every Number.* This is plainly impossible, unless every article should be very short and imperfect. All that the Public are entitled to expect is, that in the progress of the Journal, the various subjects mentioned may occupy such an extent as our communications and resources shall permit.

We have been honoured by such a list of names of gentlemen who are willing to be considered as contributors to this Journal,

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that the publication of it would afford us no ordinary gratification, did we not feel that it is more decorous to allow their names to appear with their communications, without laying them under a previous pledge to the Public.

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PLAN OF THE WORK.

THIS Journal is intended to embrace the circle of THE PHYSICAL SCIENCES, with their application to THE ARTS, and to every useful purpose.

It is designed as a deposit for *original American communications;* it will contain also occasional selections from Foreign Journals, and notices of the progress of Science in other countries. Within its plan are embraced

NATURAL HISTORY, in its three great departments of MINERALOGY, BOTANY, and ZOOLOGY.

CHEMISTRY and NATURAL PHILOSOPHY, and their various branches: and MATHEMATICS, pure and mixed.

It will be a leading object to illustrate AMERICAN NATURAL HISTORY, and especially our MINERALOGY and GEOLOGY.

The APPLICATIONS of these sciences are obviously as numerous as *physical arts*, and *physical wants;* for no one of these arts or wants can be named which is not connected with them.

While SCIENCE will be cherished *for its own sake*, and with a due respect for its own *inherent* dignity; it will also be employed as the *handmaid to the Arts.* Its numerous applications to AGRICULTURE, the earliest and most important of them; to MANUFACTURES, both mechanical and

chemical; and, to DOMESTIC ECONOMY, will be carefully sought out, and faithfully made.

It is within the design of this Journal to receive communications likewise on MUSIC, SCULPTURE ENGRAVING, PAINTING, and generally on the fine and liberal, as well as useful arts;

On Military and Civil Engineering, and the art of Navigation;

Notices, Reviews, and Analyses of new scientific works; accounts of Inventions, and Specifications of Patents;

Biographical and Obituary Notices of scientific men; essays on COMPARATIVE ANATOMY and PHYSIOLOGY, and generally on such other branches of medicine as depend on scientific principles;

Meteorological Registers, and Reports of Agricultural Experiments: and interesting Miscellaneous Articles, not perhaps exactly included under either of the above heads.

Communications are respectfully solicited from men of science, and *from men versed in the practical arts.*

Learned Societies are invited to make this Journal, occasionally, the vehicle of their communications to the Public.

The Editor will not hold himself responsible for the sentiments and opinions advanced by his correspondents: he will consider it as an allowed liberty to make slight *verbal alterations*, where errors may be presumed to have arisen from inadvertency.

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THE

AMERICAN

JOURNAL OF SCIENCE, &c.

INTRODUCTORY REMARKS.

THE age in which we live is not less distinguished by a vigorous and successful cultivation of physical science, than by its numerous and important applications to the practical arts, and to the common purposes of life.

In every enlightened country, men illustrious for talent, worth, and knowledge, are ardently engaged in enlarging the boundaries of natural science; and the history of their labours and discoveries is communicated to the world chiefly through the medium of Scientific Journals. The utility of such Journals has thus become generally evident; they are the heralds of science; they proclaim its toils and its achievements; they demonstrate its intimate connexion as well with the comfort, as with the intellectual and moral improvement of our species; and they often procure for it enviable honours and substantial rewards.

In England the interests of science have been, for a series of years, greatly promoted by the excellent Journals of Tilloch and Nicholson; and for the loss of the latter, the scientific world has been fully compensated by Dr. Thomson's Annals of

Philosophy, and by the Journal of Science and the Arts, both published in London.

In France, the Annales de Chimie et de Physique, the Journal des Mines, the Journal de Physique, &c. have long enjoyed a high and deserved reputation. Indeed, there are few countries in Europe which do not produce some similar publication; not to mention the transactions of learned societies and numerous medical Journals.

From these scources *our* country reaps, and will long continue to reap, an abundant harvest of information: and if the light of science, as well as of day, springs from the east, we will welcome the rays of both; nor should national pride induce us to reject so rich an offering.

But can we do nothing in return? In a general diffusion of useful information through the various classes of society, in activity of intellect, and fertility of resource and invention, characterizing a highly intelligent population, we have no reason to shrink from a comparison with any country. But the devoted cultivators of *science*, in the United States, are comparatively few; they are, however, rapidly increasing in number. Among them are persons distinguished for their capacity and attainments, and notwithstanding the local feelings nourished by our state sovereignties, and the rival claims of several of our larger cities, there is evidently a predisposition towards a concentration of effort, from which we may hope for the happiest results, with regard to the advancement of both the science and the reputation of our country.

Is it not, therefore, desirable to furnish some rallying point, some object sufficiently interesting to be compassed by common efforts, and thus to become the basis of an enduring, common interest? To produce these efforts, and to excite this interest, nothing, perhaps, bids fairer than a Scientific Journal. Hitherto nearly all our exertions, of this kind, have been made by medical gentlemen, and directed primarily to medical objects. We are neither ignorant nor forgetful of the merits of our various Medical Journals, nor of the zeal with which, as far as consistent with their main object, they have fostered the physical sciences. We are aware, also, that Journals have

been established, *professedly* deriving their materials principally from foreign sources; that our various literary Magazines and Reviews have given, and continue to give, some notices of physical and mathematical subjects, and that some of them seem even partial to these branches of knowledge: that various limited efforts have been made, and are still making, to publish occasional or periodical papers, devoted to mathematical or physical subjects, and that even our newspapers sometimes contain scientific intelligence. We are aware, also, that some of our academies and societies of natural history, either in Journals of their own, or through the medium of existing magazines, communicate to the public the efforts of their members in various branches of natural science.

But all these facts go only to prove the strong tendency which exists in this country towards the cultivation of physical science, and the inadequacy of the existing means for its effectual promulgation.

Although our limits do not permit us, however much inclined, to be more particular in commemorating the labours and in honouring the performances (often marked by much ability) of our predecessors and cotemporaries, there is one effort which we are not willing to pass by without a more particular notice; and we are persuaded that no apology is necessary for naming the Journal of the late Dr. Bruce, of New-York, devoted principally to mineralogy and geology.

No future historian of American science will fail to commemorate this work as our earliest *purely scientific* Journal, supported by *original American communications.*

Both in this country and in Europe, it was received in a very flattering manner; it excited, *at home*, great zeal and effort in support of the sciences which it fostered, and, *abroad*, it was hailed as the harbinger of our future exertions. The editor was honoured with letters on the subject of his Journal, and with applications for it from most of the countries in Europe; but its friends had to regret that, although conducted in a manner perfectly to their satisfaction, it appeared only at distant intervals, and, after the lapse of several years, never proceeded beyond the fourth number.

The hopes of its revival have now, unhappily, become completely extinct, by the lamented death of Dr. Bruce.*

This gentleman, with an accomplished education, with extensive acquirements in science, and great zeal for promoting it in his own country; advantageously and extensively known in Europe, and furnished with a correct and discriminating mind, and a chaste, scientific taste, was so well qualified for the task which he had undertaken, that no one can attempt to resume those scientific labours which he has now *for ever* relinquished, without realizing that he undertakes an arduous enterprise, and lays himself under a heavy responsibility. American science has much to lament in the death of Dr. Bruce.

No one, it is presumed, will doubt that a Journal devoted to science, and embracing a sphere sufficiently extensive to allure to its support the principal scientific men of our country, is greatly needed; if cordially supported, it will be successful, and if successful, it will be a great public benefit.

Even a failure, in so good a cause, (unless it should arise from incapacity or unfaithfulness,) cannot be regarded as dishonourable. It may prove only that the attempt was *premature*, and that our country is not yet ripe for such an undertaking; for *without the efficient support of talent, knowledge*, and *money, it cannot long proceed.* No editor can hope to carry forward such a work without the active aid of scientific and practical men; but, at the same time, the public have a right

* I trust the public will pardon me for stating, that various scientific friends, despairing of the revival of the Journal of Dr. Bruce, had, for some time, pressed me to undertake the editing of a Journal of Science. Considerations of personal friendship prevented me from listening to such proposals till the decline of Dr. Bruce's health, attended by the most alarming symptoms, rendered it very obvious that his Journal would not be revived. Towards the close of last November, in a personal interview, I communicated to him the design of the present work, at the same time offering to waive it, provided he considered it as probable that his own Journal would be resumed. Of this, however, he gave no encouragement; but, on the contrary, expressed his warm approbation of my undertaking, authorized me to consider him as a contributor, and to make public use of his name as a patron. It was not till after this that the annunciation of this work took place; and it is certain that had not all hope of the resumption of Dr. Bruce's Journal been completely cut off, *this* would not have appeared.

to expect that he will not be sparing of his own labour, and that his work shall be generally marked by the impress of his own hand. To this extent the editor cheerfully acknowledges his obligations to the public; and it will be his endeavour faithfully to redeem his pledge.

Most of the periodical works of our country have been short-lived. *This*, also, *may* perish in its infancy; and if any degree of confidence is cherished, that it will attain a maturer age, it is derived from the obvious and intrinsic importance of the undertaking; from its being built upon permanent and momentous national interests; from the evidence of a decided approbation of the design, on the part of men of the first eminence, obtained in the progress of an extensive correspondence; from assurances of support, in the way of contributions, from men of ability in many parts of the union; and from the existence of *such a crisis* in the affairs of this country and of the world, as appears peculiarly auspicious to the success of every wise and good undertaking.

As regards the subjects of this work, it is in our power to do much in the department of the natural history of this country. Our Zoology has been more fully investigated than our mineralogy and botany; but neither department is in danger of being exhausted. The interesting travels of Lewis and Clark have recently brought to our knowledge several plants and animals before unknown. Foreign naturalists frequently explore our territory; and, for the most part, convey to Europe the fruits of their researches, while but a small part of our own productions is examined and described by Americans: certainly, this is little to our credit, and still less to our advantage. Honourable exceptions to the truth of this remark are furnished by the exertions of some gentlemen in our principal cities, and in various other parts of the Union.*

Our botany, it is true, has been extensively and successfully investigated; but this field is still *rich*, and rewards every new

* The efforts of Stephen Elliott, Esq. of South Carolina, in regard to the botany of the Southern States, are particularly worthy of imitation and praise.

research with some interesting discovery. Our mineralogy, however, is a treasure but just opened. That both science and art may expect much advantage from this source, is sufficiently evinced by the success which has crowned the active efforts of a few ardent cultivators of this science: several new species of minerals have been added to it in this country; great numbers of American localities discovered, and interesting additions made to our materials, for the useful and ornamental arts. The science of mineralogy is now illustrated by courses of lectures, and by several good cabinets in the different states. Among the cabinets, the splendid collection of Colonel GIBBS, now in Yale College, (a munificent DEPOSIT for the benefit of his country,) *stands pre-eminent:* it would be considered as a very noble cabinet in any part of Europe: and its introduction into the United States, and its *gratuitous* dedication to the promotion of science, are equally advantageous to the community, and honourable to its patriotic and enlightened proprietor. Mineralogy is most intimately connected with our arts, and especially with our agriculture.

Such are the disguises worn by many most useful mineral substances, that an unskilful observer is liable to pass a thing by, as worthless, which, if better informed, he would seize with avidity; and, still more frequently, a worthless substance, clothed perhaps in a brilliant and attractive exterior, excites hopes altogether delusive, and induces expense, without a possibility of remuneration. A diffusion of correct knowledge on this subject is the only adequate remedy for either evil.

Our geology, also, presents a most interesting field of inquiry. A grand outline has recently been drawn by Mr. Maclure, with a masterly hand, and with a vast extent of personal observation and labour: but to fill up the detail, both observation and labour still more extensive are demanded; nor can the object be effected, till more good geologists are formed, and distributed over our extensive territory.

To account for the formation and changes of our globe, by excursions of the imagination, often splendid and imposing, but usually visionary, and almost always baseless, was, till within half a century, the business of geological speculations; but

this research has now assumed a more sober character; the science of geology has been reared upon numerous and accurate observations of *facts;* and standing thus upon the basis of induction, it is entitled to a rank among those sciences which Lord Bacon's Philosophy has contributed to create. Geological researches are now prosecuted, by actually exploring the structure and arrangement of districts, countries, and continents. The obliquity of the strata of most rocks, causing their edges to project in many places above the surface; their exposure in other instances, on the sides or tops of hills and mountains; or, in consequence of the intersection of their strata, by roads, canals, and river-courses, or by the wearing of the ocean; or their direct perforation, by the shafts of mines; all these causes, and others, afford extensive means of reading the interior structure of the globe.

The outlines of American geology appear to be particularly grand, simple, and instructive; and a knowledge of the important facts, and general principles of this science, is of vast practical use, as regards the interests of agriculture, and the research for useful minerals. Geological and mineralogical descriptions, and maps of particular states and districts, are very much needed in the United States; and to excite a spirit to furnish them will form one leading object of this journal.

The science of natural philosophy, with its powerful auxiliary, mathematics, and the science of chemistry, the twin sister of natural philosophy, are of incalculable importance to this country. A volume would not suffice to trace their applications, and to enumerate the instances of their utility.

As one which may be allowed to stand, *instar omnium*, we may mention the steam engine; that legitimate child of physical and chemical science—at once more powerful than the united force of the strongest and largest animals, and more manageable than the smallest and gentlest; raising from the bowels of the earth the massy treasures of its mines, drawing up rivers from their channels, and pouring them, in streams of life, into the bosom of cities; and, above all, propelling against the currents, the winds, and the waves of the ocean, those stupendous vessels, which combine speed with certainty,

and establish upon the bosom of the deep the luxuries and accommodations of the land.

The successful execution of this magnificent design was first witnessed upon the waters of the Hudson, but is now imitated in almost every civilized country; and it remains to be seen whether they will emulate us by transporting, by the same means, and against the same obstacles, the most formidable trains of artillery.

The mechanical inventions of this country are numerous; many of them are ingenious, and some are highly important. In no way can a knowledge of them be so readily and extensively diffused as in a scientific journal. To this object, therefore, a part of our labours (should there be a call for it,) will be devoted, and every necessary aid will be given by plates and descriptions.

Science and art mutually assist each other; the arts furnish facts and materials to science, and science illuminates the path of the arts.

The science of mathematics, both pure and mixed, can never cease to be interesting and important to man, as long as the relations of quantity shall exist, as long as ships shall traverse the ocean, as long as man shall measure the surface or heights of the earth on which he lives, or calculate the distances and examine the relations of the planets and stars; and as long as the *iron reign of war* shall demand the discharge of projectiles, or the construction of complicated defences.

In a word, the whole circle of physical science is directly applicable to human wants, and constantly holds out a light to the practical arts; it thus polishes and benefits society, and every where demonstrates both supreme intelligence, and harmony and beneficence of design in THE CREATOR.

Art. I. *Essay on Musical Temperament.**

By Professor Fisher, of Yale College.

It is well known to those who have attended to the subject of musical ratios, that a fixed scale of eight degrees to the octave, which shall render all its concords perfect, is impossible. It has been demonstrated by Dr. Smith, from an investigation of all the positions which the major, the minor, and the half-tone can assume, that the most perfect scales possible, of which there are two equally so, differing only in the position of the major and the minor tone above the key note, must have one Vth and one 3d too flat, and consequently the supplementary 4th and VIth too sharp, by a comma. In vocal music, and in that of perfect instruments, this defect in the scale is not perceived, because a small change may be made in the key, whenever the occurrence of either of those naturally imperfect intervals renders such a change necessary to perfect harmony. But in instruments with fixed scales, such as the guitar, the piano-forte, and the organ, if we begin with tuning as many concords as possible perfect, the resulting chords above-mentioned will be necessarily false in an offensive degree. Hence it is an important problem in practical harmonics, to distribute these imperfections in the scale among the different chords, in such a manner as to occasion the least possible injury to harmony.

But this is not the only nor the principal difficulty which the tuner of imperfect instruments has to encounter. In order that these instruments may form a proper accompaniment for the voice, and be used in conjunction with perfect instruments, it is necessary that music should be capable of being executed on them, in all the different keys in common use; and especially that they should be capable of those occasional modulations which often occur in the course of the same piece. Now only five additional sounds to the octave are usually inserted for this purpose, between those of the natural scale, which, of course, furnish it with only three sharps and two flats. Hence,

* From the MS. papers of the Connecticut Academy, now published by permission.

when a greater number of flats or sharps is introduced, the music can be executed only by striking, in the former case, the sharp of the note next below ; and, in the latter, the flat of the note next above. But as the diatonic semitone is more than half the major, and much more than half the minor tone, if the additional sounds in the common artificial scale be made perfect for one of the above employments, they must be extremely harsh for the other. Hence arises the necessity of adjusting the position of these five inserted sounds so that they may make tolerable harmony, whichever way employed. A change in these will require corresponding changes in the position of the several degrees of the natural scale ; so that it is highly probable that the best scheme of temperament will leave no concord, either of the natural or artificial scale, absolutely perfect.

In adjusting the imperfections of the scale, the three following considerations have been usually taken into view.

I. One object to be aimed at is, to make the sum of the temperaments of all the concords the least possible. Since experience teaches us that the harshness of a given concord increases with its temperament, it is obvious that of two systems which agree in other respects, the best is that in which the sum of the temperaments is least.

II. When other things are equal, the best adjustment of the imperfections of the scale is that which diminishes the harmoniousness of all the different concords proportionally. The succession of a worse to a better harmony, is justly regarded by several of the best writers on this subject, as one of the principal causes of offence to the ear, in instruments imperfectly tuned.

III. When different chords of the same kind are of unequally frequent occurrence, there is an advantage, *cæteris paribus*, in giving the greatest temperament to that which occurs most seldom. This important consideration has indeed been neglected by Dr. Smith, in the systems which he recommends, both for his changeable and the common fixed scale ; as it is, also, by the numerous advocates of the system of equal semitones. But many authors on temperament, and most instrument-makers, pay a vague regard to it. Their aim has been,

although in a loose and conjectural manner, to make the prominent chords of the simplest keys the nearest to perfection, whilst a greater temperament is thrown upon those which occur only in the more complex keys. Thus Dr. Young, in the Philos. Trans. for 1800, recommends a scheme which increases the temperament of the IIIds, on the key note of the successive keys, as we modulate by fifths from C, nearly in arithmetical progression. Earl Stanhope assigns as a reason for the small temperament which is given to several of the IIIds in his system, that they are on the tonic of the simpler keys. The irregularities in Mr. Hawkes's scheme may be traced to the same cause. And, with the instrument-makers, it is a favourite maxim to lay the wolf, as they term it, where it will be most seldom heard.

But if the above consideration deserves any weight at all, it deserves to be accurately investigated. Not only ought the relative frequency of different chords to be ascertained with the greatest accuracy, of which the nature of the subject is susceptible, but the degree of weight which this consideration ought to have, when compared with the two others above-mentioned, should be determined: for it is plain that neither of them ought to be ever left out of view.

Accordingly, the principal design of the following propositions will be to investigate the actual frequency of occurrence of different chords in practice; and from this and the two other above-mentioned considerations united, to deduce the best system of temperament for a scale, containing any given number of sounds to the octave, and particularly for the common Douzeave, or scale of twelve degrees.

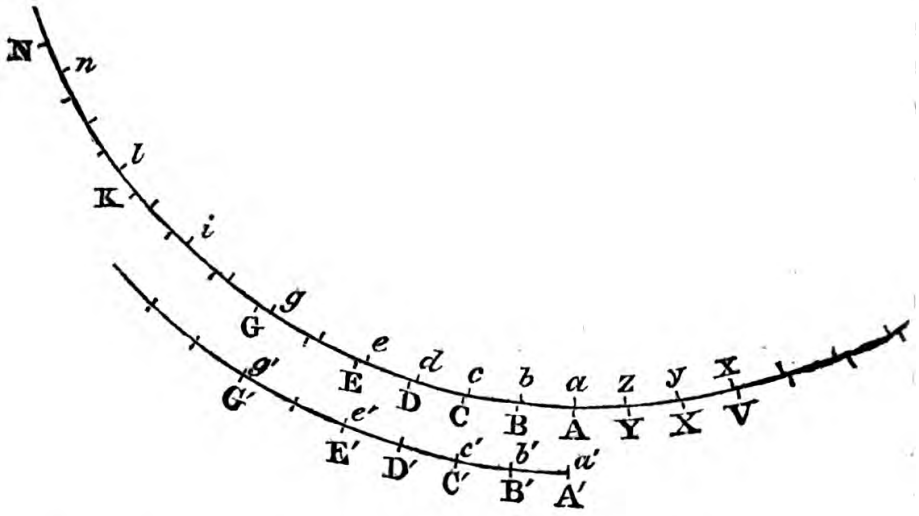
Proposition I.

All consonances may be regarded, without any sensible error in practice, as equally harmonious in their kinds, when equally tempered; and when unequally tempered, within certain limits, as having their harmoniousness diminished in the direct ratio of their temperaments.

As different consonances, when perfect, are not pleasing to the ear in an equal degree, some approaching nearer to the

nature of discords than others, so a set of tempered consonances, *cæteris paribus*, will be best constituted when their harmoniousness is diminished *proportionally*. Suppose, for example, that the agreeable effects of the Vth, IIId, and 3d, when perfect, are as any unequal numbers, a, b, and c; the best arrangement of a tempered scale, other things being equal, would be, not that in which the agreeable effect of the Vth was reduced to an absolute level with that of the IIId, or 3d, but when they were so tempered that their agreeable effects on the ear might be expressed by $\frac{m}{n}a$, $\frac{m}{n}b$, and $\frac{m}{n}c$.

That different consonances, in this sense, are equally harmonious in their kinds, when equally tempered, or, at least, sufficiently so for every practical purpose, may be illustrated in the following manner :



Let the lines AB, *ab*, represent the times of vibration of two tempered unisons. Whatever be the ratio of AB to *ab*, whether rational or irrational, it is obvious that the successive vibrations will alternately recede from and approach each other, till they very nearly coincide; and, that during one of these periods, the longer vibration, AB, has gained *one* of the shorter. Let the points, A, B, &c. represent the middle of the

successive times of vibration of the lower; and *a*, *b*, &c. those of the higher of the tempered unisons. Let the arch AGN .. VA be a part of a circle, representing one period of their pulses, and let the points A, *a*, be the middle points of the times of those vibrations which approach the nearest to a coincidence. It is obvious that the dislocations *b*B, *c*C, &c. of the successive pulses, increase in a ratio which is very nearly that of their distances from A, or *a*. Now if the pulses exactly coincided, the unisons would be perfect; and the same would be equally true, if the pulses of the one bisected, or divided in any other constant ratio, those of the other; as clearly appears from observation. It is, therefore, not the absolute magnitude, as asserted by Dr. Smith, but the *variableness* of the successive dislocations, B*b*, C*c*, &c. which renders the imperfect unisons discordant; and the magnitude of the successive increments of these dislocations is the measure of the degree of discordance heard in the unisons.

If now the time of vibration in each is doubled, AC, *ac*, &c. will represent the times of vibration of imperfect unisons an octave below, and the successive dislocations will be C*c*, E*e*, &c. only half as frequent as before. But the unisons AE, *ae*, will be equally harmonious with AB, *ab;* because, although the successive dislocations are less frequent than before, yet the coincidences C′*c*′, E′*e*′ of the corresponding perfect unisons are less frequent in the same ratio.

Suppose, in the second place, that the time of vibration is doubled, in only one of the unisons, *ab;* and that the times become AB and *ac*, or those of imperfect octaves. These will also be equally harmonious in their kind with the unisons AB, *ab*. For, although the dislocations C*c*, E*e*, &c. are but half as numerous as before, the coincidences of the corresponding perfect octaves will be but half as numerous. The dislocations which remain are the same as those of the imperfent unisons; and if some of the dislocations are struck out, and the increments of successive ones thus increased, no greater change is made in the nature of the imperfect than of the perfect consonance.

If, thirdly, we omit two-thirds of the pulses of the lower unison, retaining the octave *ac* of the last case, we shall have AD, *ac*, the times of vibration of imperfect Vths, to which, and to all other concords, the same reasoning may be applied as above. It may be briefly exhibited thus; since the intermission of the coincidences C′*c*′, E′*e*′ of the perfect unisons, an octave below A′B′, does not render the Vth A′D′G *a*′*c*′*e*′*g*′ less perfect than the unison A′*c*′ *a*′*c*′, each being perfect in its kind; so neither does the intermission of the corresponding dislocations C*c*, E*e*, of the tempered unisons, in the imperfect Vth, ADG, *aceg*, render it less harmonious in its kind than the tempered unison AB, *ab*, from which it is derived in exactly the same manner that the perfect Vth is derived from the perfect unison.

The consonances thus derived, as has been shown by Dr. Smith, will have the same periods, and consequently the same beats, with the imperfect unisons. It is obvious, likewise, that they will all be equally tempered. Let *m* AB, and *n ab*, be a general expression for the times of vibration of any such consonance. The tempering ratio of an imperfect consonance is always found by dividing the ratio of the vibrations of the imperfect by that of the corresponding perfect consonance. But $\frac{m\,\mathrm{AB}}{n\,ab} \div \frac{m}{n} = \frac{\mathrm{AB}}{ab}$; which is evidently the tempering ratio of the imperfect unisons.

Hence, so far as any reasoning, founded on the abstract nature of coexisting pulses can be relied on, (for, in a case of this kind, rigid demonstration can scarcely be expected,) we are led to conclude that the harmoniousness of different consonances is proportionally diminished when they are equally tempered.

The remaining part of the proposition, viz. that consonances differently tempered have their harmoniousness diminished, or their harshness increased, in the direct ratio of their temperaments, will be evident, when we consider that the temperament of any consonance is the sole cause of its harshness, and that the effect ought to be proportioned to its ade-

quate cause. We may add, that the rapidity of the beats, in a given consonance, increases very nearly in the ratio of the temperament; and universal experience shows, that increasing the rapidity of the beats of the same consonance, increases its harshness. This is on the supposition that the consonance is not varied so much as to interfere with any other whose ratio is equally simple.

Cor. We may hence infer, that in every system of temperament which preserves the octaves perfect, each consonance is equally harmonious, in its kind, with its complement to the octave, and its compounds with octaves. For the tempering ratio of the complement of any concord to the octave, is the same with that of the concord itself, differing only in its sign, which does not sensibly affect the harmony or the rate of beating; while the tempering ratio of the compounds with octaves is not only the same, but with the same sign.

Scholium 1.

There is no point in harmonics, concerning which theorists have been more divided in opinion than in regard to the true measure of equal harmony, in consonances of different kinds. Euler maintains, that the more simple a consonance is, the *less* temperament it will bear; and this seems to have ever been the general opinion of practical musicians.* Dr. Smith, on the contrary, asserts, and has attempted to demonstrate, that the simpler will bear a much greater temperament than the more complex consonances. The foregoing proposition has, at least, the merit of taking the middle ground between these discordant opinions. If admitted, it will greatly simplify the whole subject, and will reduce the labour of rendering all the concords in three octaves as equally harmonious as possible, which occupies so large a portion of Dr. Smith's volume, to a single short proposition. Dr. Smith's measure of equal harmony, viz. equal numbers of short cycles in the intervals between the successive beats, seems designed, not to render the different consonances proportionally harmonious, but to reduce

* See Kollmann's Harmony, p. 13, &c.

the simpler to an absolute level, in point of agreeableness, with the more complex; which, as has been shown, is not the object to be aimed at in adjusting their comparative temperaments. But, in truth, his measure is far more favourable to the complex consonances than equal harmony, even in this sense, would require; and, in a great number of instances, leads to the grossest absurdities. Two consonances, according to him, are equally harmonious, when their temperaments are inversely as the products of the least numbers expressing their perfect ratio. If so, the VIII + 3d, whose ratio is $\frac{5}{12}$, when tempered $\frac{1}{20}$ of a comma, and the unison, whose ratio is $\frac{1}{1}$, when tempered 3 commas, are equally harmonious. But all who have the least experience in tempered consonances will pronounce, at once, that the former could scarcely be distinguished by the nicest ear from the corresponding perfect concord, while the latter would be a most offensive discord. One instance more shall suffice. The temperaments to render the VIII + Vth, and the VIII + 6th equally harmonious, are laid down in his tables to be as 80 : 3. We will now suppose an instrument perfectly tuned in Dr. Smith's manner, and furnished with all the additional sounds which constitute his changeable scale. In this system, the IIIds, and consequently the VIII + 6ths, are tempered $\frac{1}{9}$ of a comma; which, so far from being offensive, will be positively agreeable to the ear. This cannot be doubted by those who admit that the VIII + 6ths in the common imperfect scales, when tempered at a medium nearly seven times as much, make tolerable harmony. Yet, according to the theory which we are opposing, the VIII + Vth will be equally harmonious when tempered nearly a minor semitone. Now let any one, even with the common instruments, whenever an VIII + Vth occurs, strike the semitone next above or below: for example, instead of playing C, *g*, let him play C, *g*♯; instead of A, *e*, let him play A, *e*♭, &c. and compare the harmony of these with that of the VIII + 6ths, if he wants any farther evidence that Dr. Smith's measure of equal harmony is without foundation.

It may be thought, that even the measure of equal harmony laid down in the proposition, is more favourable to the com-

plex consonances than the conclusions of experience will warrant. But when it is asserted by practical musicians, that the octave will bear less tempering than the Vth, the Vth less than the IIId, &c., they doubtless intend to estimate the temperament by the rate of beating, and to imply, that when different consonances to the same base are made to beat equally fast, the simpler are more offensive than the more complex consonances. This is entirely consistent with the proposition; for when equally tempered, the more complex consonances will beat more rapidly than the more simple; if on the same base, very nearly in the ratio of their major terms. (Smith's Har. Prop. XI. Cor. 4.) If, for example, an octave, a Vth, and a IIId on the same base were made to beat with a rapidity which is as the numbers 2, 3, and 5, no unprejudiced ear would probably pronounce the octave less harmonious in its kind than the IIId.

To those, on the other hand, who may incline to a measure of equal harmony between that laid down in the proposition and that of Dr. Smith, on account of the rapidity of the beats of the more complex consonances, it may be sufficient to reply, that if the beats of a more complex consonance are more rapid than those of a simpler one, when both are equally tempered, those of the latter, cæteris paribus, are more *distinct.* It is the distinctness of the undulations, in tempered consonances, which is one of the principal causes of offence to the ear.

Scholium 2.

It will be proper to explain, in this place, the notation of musical intervals, which will be adopted in the following pages. It is well known that musical intervals are as the logarithms of their corresponding ratios. If, therefore, the octave be represented by .30103, the log. of 2, the value of the Vth will be expressed by .17509; that of the major tone by .05115; that of the comma by .00540, &c. But in order to avoid the prefixed ciphers, in calculations where so small intervals as the temperaments of different concords are concerned, we will

multiply each of these values by 100,000, which will give a set of integral values having the same ratio. The octave will now become 30103, the comma 540, &c. ; and, in general, when temperaments are hereafter expressed by numbers, they are to be considered as so many 540ths of a comma. Had more logarithmic places been taken, the intervals would have been expressed with greater accuracy ; but it was supposed that the additional accuracy would not compensate for the increased labour of computation which it would occasion. This notation has been adopted by Dr. Robinson, in the article Temperament, (Encyc. Brit. Supplement ;) and for every practical purpose, is as much superior to that proposed by Mr. Farey, in parts of the Schisma, lesser fraction and minute,* as all decimal measures necessarily are, to those which consist of different denominations.

Proposition II.

In adjusting the imperfections of the scale, so as to render all the consonances as equally harmonious as possible, only the simple consonances, such as the Vth, IIId, and 3d, with their complements to and compounds with the octave, can be regarded.

It has been generally assigned as the reason for neglecting the consonances, usually termed discords, in ascertaining the best scheme of temperament, that they are of less frequent occurrence than the concords. This, however, if it were the only reason, would lead us, not to neglect them entirely, but merely to give them a less degree of influence than the concords, in proportion as they are less used.

A consideration which seems not to have been often noticed, renders it impossible to pay them any regard in harmonical computations. All such computations must proceed on the supposition that within the limits to which the temperaments of the different consonances extend, they become harsher as their temperaments are increased. It is evident that any con-

* Tilloch's Phil. Mag. Vol. XXVIII. p. 140.

sonance may be tempered so much as to become better by having its temperament increased, in consequence of its approaching as near to some other perfect ratio, the terms of which are equally small ; or perhaps much nearer some perfect ratio whose terms are not proportionally larger. For example, after we have sharpened the Vth more than 3 commas, it becomes more harmonious, as approaching much nearer to the perfect ratio $\frac{5}{6}$. In this, however, and the other concords, the value of the nearest perfect ratios in small numbers, varies so much from the ratios of these concords, and the consequent limits within which the last part of Prop. I. holds true, are so wide that there is no hazard in making it a basis of calculation. And if there be a few exceptions to this, in some systems, in which the temperaments of a few of the concords become so large as to approach nearer to some other perfect ratio, whose terms are nearly as small as those of the perfect concord, although they might become more harmonious, by having their temperament increased, yet their effect in *melody* would be still more impaired; so that the concords may all be considered as subjected to the same rule of calculation.

But the limits within which the second part of Prop. I. holds true, with regard to the more complex consonances, are much more limited. We cannot, for instance, sharpen the 7th, whose ratio is 9 : 16 more than $\frac{1}{2}$ a comma, without rendering it more harmonious, as approaching nearer another perfect ratio which is simpler ; that of 5 : 9. Yet the difference between these two 7ths is so trifling that they have never received distinct names ; and, indeed, their effect on the ear in melody would not be sensibly different.

Again, the 5th, whose perfect ratio has been generally laid down as 45 : 64, but which is in reality 25 : 36,* cannot be sharpened more than $\frac{1}{3}$ of a comma, before it becomes more

* The propriety of making 25 : 36 the true ratio of the 5th will be manifest, when it is considered that this is the value of that interval as sounded by voices and perfect instruments; when the 3ds which compose it are made perfect. This interval, as found in the scale which has the fewest tempered concords possible referred to at the beginning of this essay, ought to be regarded as the true 5th, flattened by a comma, in the same manner as one of its component 3ds will be allowed by all to be flattened.

harmonious by having its temperament increased, as approaching nearer the simpler ratio 7 : 10. At the same time, the effect of this interval in melody would not be sensibly varied. The limits, within which the harmoniousness of the IVth is inversely as its temperament, are still narrower.

Hence it appears that no inference can be drawn from the temperaments of such consonances as the 7th, 5th, IVth, &c. respecting their real harmoniousness. The other perfect ratios which have nearly the same value with those of these chords, and which are in equally simple terms, are so numerous that by increasing their temperament they alternately become more and less harmonious; and in a manner so irregular, that to attempt to subject them to calculation, with the concords, would be in vain. Even when unaltered, they may be considered either as greater temperaments of more simple, or less temperaments of more complex ratios. Suppose the 5th, for example, to be flattened $\frac{1}{5}$ of a comma: shall it be considered as deriving its character from the perfect ratio 25 : 36, and be regarded as flattened 108; or shall it be referred to the perfect ratio 7 : 10, and considered as sharpened 239? No one can tell.—On the whole, it is manifest that no consonances more complex than those included in the proposition, can be regarded in adjusting the temperaments of the scale.

Proposition III.

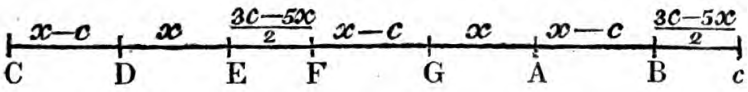
The best scale of sounds, which renders the harmony of all the concords as nearly equal as possible, is that in which the Vths are flattened $\frac{2}{7}$, and the IIIds and 3ds, each $\frac{1}{7}$ of a comma.

The octave must be kept perfect, for reasons which have satisfied all theoretical and practical harmonists, how widely soever their opinions have differed in other respects. Admitting equal temperament to be the measure of equal harmony, the complements of the Vth, IIId, and 3d, to the octave, and their compounds with octaves will be equally harmoni-

ous in their kinds with these concords respectively ; according to the corollary of Prop I.

Hence we have only to find those temperaments of the Vths, IIIds, and 3ds, in the compass of one octave, which will render them all, as nearly as possible, equally harmonious. The temperaments of the different concords of the same name ought evidently to be rendered equal ; since, otherwise, their harmony cannot be equal. This can be effected only by rendering the major and minor tones equal, and preserving the equality of the two semitones. If this is done, the temperament of all the IIIds will be equal, since they will each be the sum of two equal tones. For a similar reason the 3ds, and consequently the Vths, formed by the addition of IIIds, and 3ds, will be equally tempered.

In order to reduce the octave to five equal and variable tones, and two equal and variable semitones, we will suppose the intervals of the untempered octave to be represented by



the parts CD, DE, &c. of the line C*c*. Denoting the comma by *c*, we will suppose the tone DE, which is naturally minor, to be increased by any variable quantity, x ; then, by the foregoing observations, the other minor tone, GA, must be increased by the same quantity. As the major tones must be rendered equal to the minor, their increment will be $x - c$. As the octave is to be perfect, the variation of the two semitones must be the same with that of the five tones, with the contrary sign ; and as they are to be equally varied, the decrement of each will be $\frac{5x - 3c}{2}$; or what amounts to the same thing, the increment of each will be $\frac{3c - 5x}{2}$.

The several concords of the same name in this octave are now affected with equal and variable temperaments. The common increment of the IIIds will be $2x - c$; that of the 3ds $\frac{1}{3}.\overline{c - 3x}$; and consequently that of the Vths $\frac{1}{2}.\overline{x - c}$.

In adjusting these variable temperaments, so as to render the harmony of the concords of *different* kinds, as nearly equal as possible, we immediately discover that, as the Vth is composed of the IIId and 3d, the temperaments of the three cannot all be equal. When the temperaments of the IIId and 3d have the same sign, that of the Vths must be equal to their sum; and, when they have contrary signs, to their difference. Hence the temperament of one of these three concords is necessarily equal to the sum of that of the other two. This being fixed, the temperaments, and consequently, (by Prop. I.) the discordance of the different consonances is the most equably divided possible, when the two smaller temperaments, whose sum is equal to the greater, are made equal to each other. The problem contains three cases.

1. When the temperaments of the IIId and 3d have the same sign, they ought to be equal to each other. Making $2x - c = \frac{1}{2}.\overline{c - 3x}$, we obtain $x = \frac{3}{7}c$, which, substituted in the general expressions for the temperaments of the Vth, IIId, and 3d, makes their increments equal to $-\frac{2}{7}c$, $-\frac{1}{7}c$, $-\frac{1}{7}c$, respectively.

2. Let the temperaments of the IIId and 3d have contrary signs: and first, let that of the IIIds be the greater. Then the former ought to be double of the latter, in order that the temperament of the Vths and and 3ds may be equal. Hence we have $2x - c = -2.\frac{1}{2}.\overline{c - 3x}$; whence x is found $= o$; and by substitution as before, the required temperament of the IIId $= -c$; of the Vth $-\frac{1}{2}c$, and of the 3d $\frac{1}{2}c$.

3. Let the temperaments of the IIId and 3d have contrary signs, as before; and let that of the 3d be the greater. Making $\frac{1}{2}.\overline{c - 3x} = -\overline{2.2x - c}$, we obtain $x = \frac{3}{5}c$; which gives, by substitution, the temperaments of the 3d, Vth, and IIId $-\frac{2}{5}c$, $-\frac{1}{5}c$, and $\frac{1}{5}c$, respectively.

Each of these results makes the harmony of all the consonances as nearly equal as possible; but as the sum of the temperaments in the first case is much the least, it follows that the temperaments stated in the proposition constitute the best

scheme of intervals for the natural scale, in which the harmony of all the different consonances is rendered as nearly equal as possible.

Cor. 1. In the same manner it may be shown that these temperaments are the best, among those which approach as nearly as possible to equal harmony, for the *artificial* scale; provided that it is furnished with distinct sounds for all the sharps and flats in common use. By inserting a sound between F and G, making the interval F♯ G equal to either of the semitones found above, the intervals, reckoned from G as a key note, will be exactly the same in respect to their temperaments, as the corresponding ones reckoned from C. The same thing holds, whatever be the number of flats and sharps. It is supposed, however, that the flat of a note is never used for the sharp of that next below, or the contrary; and hence this scheme of temperament would only be adapted to an instrument, furnished with all the degrees of the enharmonic scale; or, at least, with as many as are in common use.

Cor. 2. This scale will differ but little in practice from the one deduced, with so much labour, by Dr. Smith, from his criterion of equal harmony; which flattens the Vths $\frac{5}{18}$, the *III*ds $\frac{1}{9}$, and the 3ds $\frac{1}{6}$ of a comma. The several differences are only $\frac{1}{126}$, $\frac{2}{63}$, and $\frac{1}{42}$ of a comma. Hence, as his measure of equal harmony differs so widely from that of Proposition I. we may infer that the consideration of equalizing the harmony of the concords of different names can have very little practical influence on the temperaments of the scale. Should it, therefore, be maintained that the criterion laid down in Prop. I. is not mathematically accurate; yet, as it must be allowed, in the most unfavourable view, to correspond far better with the decisions of experience than that of Doctor Smith, the chance is, that, at the lowest estimate, the temperaments deduced from it approach much more nearly to correctness. Hence it is manifest that equal temperament may be made, *without any sensible error in practice*, the criterion of equal harmony.

Scholium 3.

Although the foregoing would be the best division of the musical scale, if our sole object were to render the harmony of its concords as nearly equal as possible, yet the two other considerations, stated at the beginning of the essay, must by no means be neglected, as has been done by Dr. Smith. It seems to be universally admitted, that the sum of the temperaments may be increased to a certain extent, in order to equalize the harmony of the concords; otherwise the natural scale of major and minor tones, which makes the sum of the temperaments of the Vths, IIIds, and 3ds but 2 commas, ought to be left unaltered. Yet how far this principle ought to be carried, may be a matter of doubt. If we make the IIIds perfect, and flatten the Vths and 3ds each $\frac{1}{4}$ *c*, according to the old system of mean tones, we shall have the smallest aggregate of temperaments which admits of the different concords of the same name being rendered equally imperfect; but this amounts to $2\frac{1}{2}$ commas. Thus far, however, it seems evidently proper to proceed. If we go still farther, and endeavour to equalize the harmony of the concords of *different* names, it may be questioned whether nearly as much is not lost as gained; for the aggregate temperaments are increased, in Dr. Smith's scale, to $2\frac{2}{3}$ *c*, and in that of the above proposition to $2\frac{5}{7}$ *c*. The system of mean tones, although more unequal in its harmony when but two notes are struck at once, yet when the chords are played full, as they generally are on the organ, never offends the ear by a transition from a better to a worse harmony. For every *triad* is equally harmonious; being composed of a perfect IIId, and a Vth and 3d, tempered each $\frac{1}{4}$ *c*, or of their complements to, or compounds with octaves, which, in their kinds, are equally harmonious.

Again, if different chords, in practice, vary in the frequency of their occurrence, this will be a sufficient reason for deviating from the system of equal temperament. Suppose, for example, that a given sum of temperament is to be divided between two Vths, one of which occurs in playing ten times as often as

the other : there can be no doubt that the greater part of the temperament ought to be thrown upon the latter. Hence it becomes an important problem to ascertain, with some degree of precision, the relative frequency with which different consonances occur in practice. Before proceeding to a direct investigation of this problem, it may be observed, in general, that such a difference manifestly exists. In a given key, it cannot have escaped the most superficial observer, that the most frequent combination of sounds is the common chord on the tonic ; that the next after this is that on the dominant, and the third, that on the subdominant. Perhaps scarcely a piece of music can be found, in which this order of frequency does not hold true. It is equally true that some signatures occur oftener than others. That of one sharp will be found to be more used, in the major mode, than any other ; and, in general, the more simple keys will be found of more frequent occurrence than those which have more flats or sharps. These differences are not the result of accident. The tonic, dominant, and subdominant, are obviously the most prominent notes in the scale, and must always be the fundamental bases of more chords than either of the others ; while the greater ease of playing on the simpler keys will always be a reason with composers for setting a larger part of their music on these, than on the more difficult keys. It is observable, that the greater part of musical compositions, whether of the major or minor mode, is reducible to two kinds : that in which the base chiefly moves between the tonic and its octave, and that in which the base moves between the dominant and subdominant of the key. The former class, in the major mode, are almost universally set on the key of one sharp ; the latter, generally on the natural key, or that of two sharps. In the minor mode, the former class have usually the signature of two flats, or the natural key ; the latter, that of one flat. Hence the three former keys will comprise the greater part of the music in the major mode, and the three latter, of that in the minor mode, in every promiscuous collection. But if we were even to suppose each of the chords in the same key, and each of the signatures, of equally frequent occurrence, some chords would occur much

oftener, as forming an essential part of the harmony of *more keys* than others. The Vth DA, for example, forms one of the essential chords of six different keys; while the Vth G♯D♯ forms a part only of the single key of four sharps.

Proposition IV.

To find a set of numbers, expressing the ratio of the probable number of times that each of the different consonances in the scale will occur, in any set of musical compositions.

This can be done only by investigating their actual frequency of occurrence in a collection of pieces for the instrument to be tuned, sufficiently extensive and diversified to serve as a specimen of music for the same instrument in general. This may appear, at first view, an endless task; and it would be really such, were we to take music promiscuously, and count all the consonances which the base makes with the higher parts, and the higher parts with each other. But it appears, from Prop. I. Cor. that all the positions and inversions of a chord, when the octaves are kept perfect, are equally harmonious with the chord itself. The Vth, for example, which makes one of the consonances in a common harmonic triad, is equally harmonious in its kind, with the V+VIII, which takes its place in the 3d position of this triad, and with the 4th in its second inversion. Hence, instead of counting single consonances, we have only to count chords; and this is done with the greatest ease, by means of the figures of the thorough base. The labour will be still farther abridged by reducing the derivative chords, such as the 6, the $\frac{6}{4}$, &c. to their proper roots, as they are taken down. But even after these reductions, the labour of numbering the different chords in a sufficiently extensive set of compositions, to establish, with any degree of certainty, the relative frequency of the different signatures, would be very irksome. A method, however, presents itself, which renders it sufficient to examine the chords in such a set of pieces only as will give their chance of occurrence in *two* keys—a major, and its relative minor.

It will be evident to all who are much conversant with musical compositions, that the *internal structure* of all pieces in

the same mode, whatever be their signature, is much the same. There is scarcely more difference, for example, in the relative frequency of different chords in the natural key, and in that of two sharps, or two flats, than there is in different pieces on the same key. If the Vth CG on the tonic has to the Vth EB on the mediant in the natural key, any given ratio of frequency $m : n$, the relative frequency of the Vth DA on the tonic, and the Vth F♯ C♯ on the mediant in the key of two sharps, will not sensibly differ from that of $m : n$. Hence, if we examine a sufficient number of pieces to establish the relative frequency of the different consonances in one major and its relative minor key, and, by a much more extensive investigation, ascertain the relative frequency of occurrence of the different signatures, it is evident, that by multiplying this last series of numbers into the first, and adding those products which belong to chords terminated by the same letters, we shall have a series of numbers expressing the chance of occurrence in favour of each of the consonances of the scale, when *all* the keys are taken into view.

It was judged that 200 scores, taken promiscuously from all the varieties of music for the organ,* would afford a set of numbers expressing, with sufficient accuracy, the chance that a given consonance will occur in a single major, and its relative minor key. Accordingly 200 scores were examined, 150 in the major, and 50 in the minor mode, (as it will appear hereafter that this is nearly the ratio of their frequency) of the various species of music for the organ, comprising a proper share both of the simpler and of the more rapid and chromatic movements. As the selecting and reducing to their proper keys all the occasional modulations which occur in the same

* The *propriety* of this limitation will be manifest, when we consider that in organ music, the chords are generally played more full, and are more protracted, than in music for other keyed instruments. It is harmony which constitutes its *character*, in a higher degree than in music for other instruments. Hence the harmony of the organ ought not to be impaired by including in our computations any music not adapted to it. If a similar examination of music for the piano-forte would afford a set of results essentially different from those of this proposition, this is no proof that it ought to have any concern in a system of temperament designed *primarily for the organ*, but merely that the same temperament cannot be equally adapted to different instruments. If, as is probable, such an examination would give essentially the same results, to introduce them would be superfluous.

piece would render the labour of ascertaining the relative frequency of different signatures very tedious, it was thought best to consider all those modulations which are too transient to be indicated by a new signature, as belonging to the same key. This will account for the occurrence of the chords in the following table, which are affected by flats and sharps.

The minim, or the crotchet, was taken for unity, according to the rapidity of the movement. Bases of greater or less length had their proper values assigned them; although mere notes of passage, which bore no proper harmony, were generally disregarded. The scores were taken promiscuously from all the different keys; and were reduced, when taken down, to the same tonic; the propriety of which will evidently appear from the foregoing remarks. The following table contains the result of the investigation.

TABLE I.

Bases.	Common Chords.		Flat Fifths.		7ths.		9-sevenths.	
	Major mode.	Minor mode.	Major.	Minor.	Major.	Minor.	Major.	Minor.
B III	5	8	—	—	7	—	—	—
B	3	—	163	55	11	17	2	—
B♭	4	4	—	—	—	—	—	—
A VII	—	—	—	—	—	—	3	—
A III	19	8	—	—	7	2	—	—
A	166	588	2	1	26	5	2	—
G ♯	—	—	3	38	—	—	—	—
G 3	18	15	—	—	—	—	—	—
G	965	93	—	—	178	15	3	—
F ♯	—	—	46	4	11	2	—	—
F	352	60	—	—	11	12	7	3
E III	26	271	—	—	1	25	—	—
E	32	25	5	1	8	—	1	4
D♯ III	—	—	2	1	—	—	—	—
D♯	—	—	—	4	—	—	—	—
D III	29	4	—	—	49	7	—	—
D	120	129	—	—	55	18	6	1
C♯	—	—	2	4	1	—	—	—
C 3	2	—	—	—	—	—	—	—
C	1769	275	—	—	5	1	4	1

The following anomalous chords were found in the major mode, and are subjoined, to make the list complete :

8 ※5ths on C, and 1 on D.

5 $\frac{5}{4}$ths on D, 2 on E, and 1 on G.

The left hand column of the foregoing table contains the fundamental bases of the several chords. When any number is annexed to the letter denoting the fundamental, it denotes the quality of some other note belonging to the chord. E III, for example, denotes that the various chords on E, which stand against it, have their third sharped ; G 3, that the third, which is naturally major, is to be taken minor, &c. Of the two columns in each of the four remaining pairs, the left contains the number of chords belonging to each root, of the kind specified at the top, which were found in 150 scores in the major mode ; and the right, the corresponding results of the examination of 50 scores in the minor mode. The diminished triad, which is used in harmonical progression like the other triads, has its lowest note considered as its fundamental. The diminished 7th, in the few instances in which it occurred, was considered as the first inversion of the $\frac{9}{7}$th, agreeably to the French classification, and was accordingly reduced to that head.

From this table, the number of times that each consonance of two notes would actually occur, were the 200 scores played, is easily computed. We will suppose three notes, besides octaves, to be played to each chord. The octaves played it is unnecessary to take into the computation, as it would only multiply the number of consonances whose temperament is the same, in the same ratio, and would have no effect on the *ratio* of the numbers expressing the frequency of the different consonances. In the chord of the 7th, which naturally consists of four notes, we will suppose, for the sake of uniformity, that one is omitted ; and as the 7th ought always to be struck, we will suppose the Vth and IIId of the base to be omitted, each half the number of times in which this chord occurs. Considered as composed of three distinct notes, neither of which is an octave of either of the others, each chord will contain three distinct consonances. The common chord on C, for example, will contain the Vth CG, the IIId CE, and the 3d EG. The

$\frac{8}{7}$ on C will contain the VII CB, the IX, or (which must have the same temperament) the IId CD, and the 3d BD. Reducing all these consonances to their proper places, and adding those of the same name which have the same degree for their base, we obtain the following results :

TABLE II.

Bases.	Vths, 4ths, and Octaves.		IIIds, 6ths, and Octaves.		3ds, VIths, and Octaves.	
	Major.	Minor.	Major.	Minor.	Major.	Minor.
B	8	8	10	8	1141	214
B♭	3	6	22	19	——	——
A	195	607	22	10	626	663
G♯	——	——	——	——	32	310
G	1088	116	1090	125	22	23
F♯	——	——	——	——	78	10
F	395	78	486	301	——	——
E	59	308	40	284	1828	308
E♭	——	——	2	——	——	——
D♯	——	——	——	——	7	9
D	197	156	60	7	403	213
C♯	——	——	——	——	26	12
C	1807	278	1959	870	4	1

Bases.	5ths, IVths, and Octaves.		7ths, IIds, and Octaves		VIIths, 2ds, and Octaves.	
	Major.	Minor.	Major.	Minor.	Major.	Minor.
B	256	265	25	17	——	——
B♭	——	——	——	——	——	——
A	2	1	34	7	3	——
G♯	10	53	——	——	——	——
G	——	——	188	20	——	——
F♯	74	7	1	2	——	——
F	——	——	——	——	17	16
E	10	1	20	27	——	——
E♭	——	——	——	——	——	——
D♯	7	5	——	——	——	——
D	——	——	123	27	——	——
C♯	9	10	1	——	——	——
C	——	——	5	1	10	1

Besides the following chromatic intervals :

Major mode.	8 extreme sharp 5ths on	C
	1 ———	D
	1 extreme flat 7th ——	G♯
Minor mode.	4 extreme sharp 6ths on	F
	4 extreme flat 7ths on	C♯
	3 ———	G♯

It was thought best to exhibit a complete table of all the consonances which occurred in the 200 scores examined; although (Prop. II.) only the concords in the upper half of the table can be regarded in forming a system of temperament. For the more frequent consonances, this table may be regarded as founded on a sufficiently extensive induction to be tolerably accurate. For the more unfrequent chords, and especially for those which arise from unusual modulations, it expresses the chance of occurrence with very little accuracy; and it is doubtless the fact that a more extensive investigation would include some chords not found at all in this list. But it must be recollected, on the other hand, that the influence of these unusual chords on the resulting system of temperament would be insensible, could their chance of occurrence be determined with the greatest accuracy.

But none of the numbers in the foregoing table by any means expresses the chance that a given interval will occur, considering *all* the keys in which it is found. For example, the Vth CG on the tonic of the natural key, in music written on this key, is the one of most frequent occurrence, its chance being expressed by 1807; but in the key of two flats, it becomes the Vth on the supertonic, and its chance of occurrence is only as 197. Hence the problem can be completed only by finding a set of numbers which shall express, with some degree of accuracy, the relative frequency of different signatures.

An examination of 1600 scores, comprising four entire collections of music for the organ and voice, by the best European composers, besides many miscellaneous pieces, afforded the results in the following table :

TABLE III.

Signatures.	Major Mode.	Minor Mode.
4𝄪*s*	 42	 2
3𝄪*s*	 95	 6
2𝄪*s*	 200	 13
1𝄪	 322	 72
♮	 176	 121
1♭	 180	 97
2♭*s*	 70	 77
3♭*s*	 116	 8
4♭*s*	 0	 3
Ratio of their sums 1201 :		399

The chance of occurrence for any chord varies as the frequency of the key to which it belongs, and as the number belonging to the place which it holds, as referred to the tonic, in Table II, jointly. Hence the chance of its occurrence in all the keys in which it is found, is as the sum of the products of the numbers in Table III., each into such a number of Table II. as corresponds to its place in that key. To give a specimen of the manner in which this calculation is to be conducted, the numbers belonging to the major mode in the three first divisions of Table II, are first to be multiplied throughout by 176, which expresses the relative frequency of the major mode of the natural key. They are then to be multiplied throughout by 322, which expresses the frequency of the key of one sharp. But the first product, which expresses the frequency of the Vth on the tonic, now becomes GD, and must be added, not to the first, but to the fifth, in the last row of products. The product into 59, expressing the frequency of the Vth on the mediant, becomes BF𝄪, an interval not found among the essential chords of the natural key. In general, the products of the numbers in Table III, into those in Table II. are to be considered as belonging, not to the letters against which these multipliers stand, but to those which have the same position

with regard to their successive tonics, as these have with regard to C. Whenever an interval occurs, affected with a new flat or sharp, it is to be considered as the commencement of a new succession of products. The IIId C♯E♯, for example, does not occur at all till we come to the key of two sharps, and even then only in occasional modulations, corresponding to the IIId on B in the natural key, whose multiplier is 10. In the key of 3 sharps it becomes another accidental chord, answering to the IIId on E in the key of C, and consequently has 40 for its multiplier. It is only in the key of 6 sharps, that it becomes a constituent chord of the key; when if that key were ever used, it would correspond to the IIId GB on the dominant of the natural key.

After all the products have been taken and reduced to their proper places, in the manner exemplified above, a similar operation must be repeated with the numbers in the second column of Table III. and those in the second columns in the three first divisions of Table II.

The necessity of keeping the major, and its relative minor key, distinct, will be evident, when we consider that the several keys in the minor mode do not follow the same law of frequency as in the major; as is manifest from the observations in Schol. Prop. III. and as clearly appears from an inspection of Table III.

But in order to discover the relative frequency of the different chords on *every* account, the results of the two foregoing operations must be united. Now, as the numbers in the two columns of Table II. at a medium, are as 3 : 1. and those in Table III. are in the same ratio, although the factors are to each other in only the simple ratio of the relative frequency of the two modes, yet their products will, at a medium, be in *the duplicate* ratio of that frequency. Hence, to render the two sets of results homologous, so that those which correspond to the same interval may be properly added, to express the general chance of occurrence for that interval in all the major and minor keys in which it is found, this duplicate ratio must be reduced to a simple one, either by dividing the first, or by multiplying the last series of results, by 3. We will do the

latter, as it will give the ratios in the largest, and, of course, the most accurate terms. Then adding those results in each which belong to the same interval, and cutting off the three right hand figures, (expressing in the nearest small fractions those results which are under 1000) which will leave a set of ratios abundantly accurate for every purpose; the numbers constituting the final solution of the problem will stand as follows:

TABLE IV.

Bases.	Vths and 4ths.	IIIds and 6ths.	3ds and VIths.	Bases.	Vths and 4ths.	IIIds and 6ths.	3ds and VIths.
F𝄪	67	29	1072	B𝄪	——	——	4
F	639	924	66	B	221	135	1161
E𝄪	——	——	12	B♭	418	654	5
E	548	323	1151	A𝄪	——	——	29
E♭	265	363	½	A	870	568	1085
D𝄪	⅓	½	144	A♭	52	78	⅕
D	1166	943	569	G𝄪	5	4	365
D♭	1	6	——	G	1207	1197	567
C𝄪	25	12	581	F𝄪𝄪	——	——	¼
C	816	1131	180	G♭	——	½	——

NOTE. In this table, as well as the last, the Vths, IIIds, and 3ds are to be taken *above*, and the 4ths, 6ths, and VIths, their complements to the octave, *below* the corresponding degrees in the first column. And, in general, whenever the Vths, IIIds, and 3ds are hereafter treated as different classes of concords, each will be understood to include its complement to the octave and its compounds with octaves.

Scholium.

The foregoing table exhibits, with sufficient accuracy, the ratio of the whole number of times which the different chords would occur, were the 1600 scores, whose signatures were examined, actually played in succession, on the keys to which they are set, and with an instrument having distinct sounds for

all the flats and sharps. Had the examination been more extensive, the results might be relied on with greater assurance as accurate ; but the general similarity, not only in the structure of different musical compositions, but in the comparative frequency of the different keys in different authors; is so great, that a more extensive examination was thought to be of little practical importance.

(*To be continued.*)

ART. II. *Review of an elementary Treatise on Mineralogy and Geology, being an introduction to the study of these sciences, and designed for the use of pupils; for persons attending lectures on these subjects; and as a companion for travellers in the United States of America—Illustrated by six plates.* By PARKER CLEAVELAND, *Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy in Bowdoin College, Member of the American Academy, and Corresponding Member of the Linnæan Society of New England.*

> ——————— itum est in viscera terræ :
> Quasque recondiderat, Stygiisque admoverat umbris,
> Effodiuntur opes ——————— OVID.

Boston, published by Cummings and Hilliard, No. 1, *Cornhill. Printed by Hilliard & Metcalf, at the University Press, Cambridge, New England.* 1816.

THIS work has been for some time before the public, and it has been more or less the subject of remark in our various journals. It is, however, so appropriate to the leading objects of *this* Journal, that we cannot consider ourselves as performing labours of supererogation while we consider the necessity, plan, and execution of the treatise of Professor Cleaveland.

An extensive cultivation of the physical sciences is peculiar to an advanced state of society, and evinces, in the country

where they flourish, a highly improved state of the arts, and a great degree of intelligence in the community. To this state of things we are now fast approximating. The ardent curiosity regarding these subjects, already enkindled in the public mind, the very respectable attainments in science which we have already made, and our rapidly augmenting means of information in books, instruments, collections, and teachers, afford ground for the happiest anticipations.

Those sciences which require no means for their investigation beyond books, teachers, and study—those which demand no physical demonstrations, no instruments of research, no material specimens: we mean those sciences which relate only to the intellectual and moral character of man, were early fostered, and, in a good degree, matured in this country. Hence, in theology, in ethics, in jurisprudence, and in civil policy, our advances were much earlier, and more worthy of respect, than in the sciences relating to material things. In some of these, it is true, we have made very considerable advances, especially in natural philosophy and the mathematics, and their applications to the arts; and this has been true, in some good degree, for very nearly a century. Natural history has been the most tardy in its growth, and no branch of it was, till within a few years, involved in such darkness as mineralogy. Notwithstanding the laudable efforts of a few gentlemen to excite some taste for these subjects, so little had been effected in forming collections, in kindling curiosity, and diffusing information, that only fifteen years since, it was a matter of extreme difficulty to obtain, *among ourselves*, even *the names* of the most common stones and minerals; and one might inquire earnestly, and long, before he could find any one to identify even *quartz*, *feldspar*, or *hornblende*, among the simple minerals; or *granite*, *porphyry*, or *trap*, among the rocks. *We speak from experience*, and well remember with what impatient, but almost despairing curiosity, we eyed the bleak, naked ridges, which impended over the valleys and plains that were the scenes of our youthful excursions. In vain did we doubt whether the glittering spangles of mica, and the still more alluring brilliancy

of pyrites, gave assurance of the existence of the precious metals in those substances; or whether the cutting of glass by the garnet, and by quartz, proved that these minerals were the diamond; but if they were not precious metals, and if they were not diamonds, we in vain inquired of our companions, and even of our teachers, what they were.

We do not forget that Dr. Adam Seybert, in Philadelphia; Dr. Samuel L. Mitchill, in New-York; and Dr. Benjamin Waterhouse, in Harvard University, began at an earlier period to enlighten the public on this subject; they began to form collections; Harvard received a select cabinet from France and England; and Mr. Smith, of Philadelphia, (although, returning from Europe fraught with scientific acquisitions, he perished tragically near his native shores,) left his collection to enrich the Museum of the American Philosophical Society.

Still, however, although individuals were enlightened, no serious impression was produced on the public mind; a few lights were indeed held out, but they were lights twinkling in an almost impervious gloom.

The return of the late Benjamin D. Perkins, and of the late Dr. A. Bruce, from Europe, in 1802 and 3, with their collections, then the most complete and beautiful that this country had ever seen; the return of Colonel Gibbs, in 1805, with his extensive and magnificent cabinet; his consequent excursions and researches into our mineralogy; the commencement, about this time, of courses of lectures on mineralogy, in several of our colleges, and of collections by them and by many individuals; the return of Mr. Maclure, in 1807; his Herculean labour in surveying the United States geologically, by personal examination; and the institution of the American Journal of Mineralogy, by Dr. Bruce, in 1810;—these are among the most prominent events, which, in the course of a few years, have totally changed the face of this science in the United States.

During the last ten years, it has been cultivated with great ardour, and with great success: many interesting discoveries in American mineralogy have been made; and this science,

with its sister science, Geology, is fast arresting the public attention. In such a state of things, books relating to mineralogy would of course be eagerly sought for.

No work, anterior to Kirwan, could be consulted by the student with much advantage, on account of the wonderful progress, which, within forty or fifty years, has been made in mineralogy. Even Kirwan, who performed a most important service to the science, was become, in some considerable degree, imperfect and obsolete; the German treatises, the fruitful fountains from which the science had flowed over Europe, were not translated; neither were those of the French; and this was the more to be regretted, because they had mellowed down the harshness and enriched the sterility of the German method of description, besides adding many interesting discoveries of their own. It is true we possessed the truly valuable treatise of Professor Jameson, the most complete in our language. But the expense of the work made it unattainable by most of our students, and the undeviating strictness with which the highly respectable author has adhered to the German mode of description, gave it an aspect somewhat repulsive to the minds of novices, who consulted no other book. We are, however, well aware of the value of this work, especially in the improved edition. It must, without doubt, be in the hands of every one who would be master of the science; but it is much better adapted to the purposes of proficients than of beginners.

The mineralogical articles dispersed through Aikin's Dictionary are exceedingly valuable; but, from the high price of the work, they are inaccessible to most persons.

The most recent of the French systems, that by Brongniart, seemed to combine nearly all the requisites that could be desired in an elementary treatise; and a translation of it would probably, ere this, have been given to the American public, had we not been led to expect the work of Professor Cleaveland, which, it was anticipated, would at least possess one important advantage over the work of Brongniart, and every other; it would exhibit, more or less extensively, *American*

localities, and give the leading features of our natural mineral associations.

Thus it appears* that the work of Professor Cleaveland was eminently needed; the science, at large, needed it; and to American mineralogists it was nearly indispensable. It appeared too at a very opportune moment. Had it come a few years sooner, in might not have found many readers. Now it is sustained by the prevailing curiosity, and diffused state of information regarding mineralogy; and, in turn, no cause could operate more effectually to cherish this curiosity, and to diffuse this information still more widely, than this book. Professor Cleaveland is therefore entitled to our thanks for undertaking this task; and, in this age of book-making, it is no small negative praise if an author be acquitted of *unnecessarily* adding to the already onerous mass of books.

With respect to the PLAN of this work, Professor Cleaveland has, with good judgment, availed himself of the excellencies of both the German and French schools.

Mr. Werner, of Fribourg, in some sense not only the founder of the modern German school of mineralogy, but almost of the science itself, is entitled to our lasting gratitude for his system of external characters, first published in 1774. In this admirable treatise he has combined precision and copiousness, so that exact ideas are attached to every part of the descriptive language, and every character is meant to be defined.

It is intended that a full description of a mineral upon this plan shall entirely exhaust the subject, and that although many properties may be found in common among different minerals, still every picture shall contain *peculiar* features, not to be found in any other. It would certainly appear, at first view, that this method must be perfect, and leave nothing farther to be desired. It has, however, been found in practice, that the full descriptions of the Wernerian writers are heavy and dry; they are redundant also, from the frequent repetition

* The smaller works of Phillips and Aikin were not then published; had they been, they could not have superseded Cleaveland; the same may be said of the respectable work of Professor Kidd, of Oxford University.

of similar properties ; and from not giving due prominence to those which are peculiar, and therefore distinctive, they frequently fail to leave a distinct impression of any thing on the mind, and thus, in the midst of what is called by the writers of this school a full *oryctognostic picture*, a student is sometimes absolutely bewildered.

Some of the modern French writers, availing themselves of Mr. Werner's very able delineation of the external characters of minerals, have selected such as are most important, most striking, distinctive, and interesting ; and drawing a spirited and bold sketch, have left the minuter parts untouched : such a picture, although less perfect, often presents a stronger likeness, and more effectually arrests the attention.

This is the method of description which has been, as we think, *happily* adopted, to a great extent by Mr. Cleaveland.

Mr. Werner, availing himself of the similarities in the external appearance of minerals, has (excepting the metals) *arranged* them also upon this plan, without regard to their constitution ; that is, *to their real nature*, or, at least, making this wholly subservient to the other : this has caused him, in some instances, to bring together things which are totally unlike in their nature, and, in other instances, to separate those which were entirely similar. Whatever may be said in favour of such a course, considered as a provisional one, while chemical analysis was in its infancy, the mind can never rest satisfied with any arrangement which contradicts the real nature of things ; in a word, the composition of minerals is the only correct foundation for their classification. This classification has been adopted by several of the ablest modern French writers.

" It is believed," (says Professor Cleaveland, Preface, p. 7.) " that the more valuable parts of the two systems may be incorporated, or, in other words, that the peculiar descriptive language of the one may, in a certain degree, be united to the accurate and scientific arrangement of the other.

" This union of descriptive language and scientific arrangement has been effected with good success, by BRONGNIART, in his System of Mineralogy—an elementary work, which seems better adapted both to interest and instruct, than any which

has hitherto appeared. The author of this volume has, therefore, adopted the *general* plan of Brongniart, the more important parts of whose work are, of course, incorporated with this."

A happier model could not, in our opinion, be chosen; and we conceive that Professor Cleaveland is perfectly consistent, and perfectly perspicuous, when, adopting the chemical composition of minerals as the only proper foundation of arrangement, and, of course, rejecting the principle of Mr. Werner, which arranges them upon their external properties, he still adopts his *descriptive* language as far as it answers his purpose. For to elect a principle of arrangement, and to classify all the members of a system so as to give each its appropriate place, is obviously quite a different thing from describing each member, after its place in a system is ascertained. In doing the latter, characters may be drawn from any source which affords them.

In his "Introduction to the Study of Mineralogy," the author has given a view at once copious, condensed, and perspicuous, of all that is necessary to be learned previously to the study of particular minerals. He begins with definitions and general principles, which are laid down with clearness.

By way of engaging the attention to the study of this department of nature, he remarks:

"From a superficial view of minerals in their natural depositories, at or near the surface of the earth, it would hardly be expected that they could constitute the object of a distinct branch of science. Nothing appears farther removed from the influence of established principles and regular arrangement, than the mineral kingdom when observed in a cursory manner. But a closer inspection and more comprehensive view of the subject will convince us, that this portion of the works of nature is by no means destitute of the impress of the Deity. Indications of the same wisdom, power, and benevolence, which appear in the animal and vegetable kingdoms, are also clearly discernible in the mineral."

"It may also be remarked," continues the author, "that several arts and manufactures depend on mineralogy for their

existence; and that improvements and discoveries in the latter cannot fail of extending their beneficial effects to the aforementioned employments. In fine, the study of mineralogy, whether it be viewed as tending to increase individual wealth, to improve and multiply arts and manufactures, and thus promote the public good; or as affording a pleasant subject for scientific research, recommends itself to the attention of the citizen and scholar."

This introductory view of the importance and interest of the science cannot be charged with the fault of exaggeration, since it is most evident that neither civilization, refinement in arts, nor comfort, can exist where the properties of mineral substances are but imperfectly understood.

As regards this country, the argument admits of much amplification. The more our mineral treasures are explored, the more abundantly do they repay the research; and we trust that the period is not far distant, when we shall no longer ignorantly tread under our feet minerals of great curiosity and value, and import from other countries, at a great expense, what we, in many instances, possess abundantly at home.*

But to return to the plan of the author's work. Few persons, unacquainted with the science of mineralogy, would suspect that mere brute matter could exhibit many strong marks, capable of discrimination.

It may, however, be confidently affirmed, that there is no mineral which, if carefully studied, may not be distinguished by characters sufficiently decisive from every other mineral; an account of these characters ought, therefore, to precede every system of mineralogy. Professor Cleaveland has, with entire propriety, included them under the heads of crystal-

* A vast region in the interior of New-York and Pennsylvania is now fertilized by inexhaustible beds of sulphat of lime, (plaster of Paris,) which, till a very few years since, were not even known to exist.

Near New-Haven immense beds of green marble were discovered in 1811, during a mineralogical excursion: this beautiful material, closely resembling the *verd antique*, is now, on the spot, wrought into tables, fireplaces, and many other ornamental forms; and although the farmers had made fences of it for 150 years, no one suspected what it was till the study of mineralogy, in Yale College, brought it to light.

lography, physical and external characters, and chemical characters.

He has given a clear view of the Abbé Haüy's curious discoveries regarding the six primitive figures or solids which form the bases of all crystals—the three integrant particles or molecules which constitute the primitive forms, and of the theory by which it is shown how the immensely numerous and diversified secondary or actual forms arise out of these few elementary figures.

This is certainly one of the most singular and acute discoveries of our age. It is true, there is a difference of opinion among mineralogists as to the practical use of crystallography in the discrimination of minerals. Some dwell upon it with excessive minuteness, and others seem restless and impatient of its details. The truth seems to be, that those who understand it, derive from it (wherever it is applicable) the most satisfactory aid; and it requires only a moderate knowledge of geometry to understand its principal outlines. On the other hand, it is no doubt possible, in most instances, to dispense with its aid, and to discriminate minerals by their other properties.

Of the external and physical characters of Mr. Werner, Mr. Cleaveland has given a clear account, combining into the same view the fine discriminations of the French authors, particularly regarding refraction, phosphorescence, specific gravity, electricity, chatoyement, and magnetism. The same may be said of the chemical characters. We do not know a more satisfactory and able view of the characters of minerals than Professor Cleaveland has exhibited.

We would however ask, whether, in enumerating the kinds of lustre, the term *adamantine* should not be explained, as it is not understood by people in general, while the terms denoting the other kinds are *generally* intelligible; whether in the enumeration of imitative forms, *lenticular* and *acicular* should not rather be referred to the laws of crystallization; whether *reniform* and *mamillary* are synonymous; whether *sandstone*, as being a mere aggregate of *fragments*, is a good instance of the *granular* fracture; whether in its natural state (at least the

common ore of nickel) is *ever* magnetic, till *purified*, and whether cobalt is *ever* magnetic unless *impure*."

Professor Cleaveland's remarks on *fracture* are uncommonly discriminating and instructive, and would lead a learner to a just comprehension of this important point in the characters of minerals.

The section relating to the *chemical characters* is concise, and professedly proceeds upon the principle of selection. It might perhaps have been, to some extent, advantageously enlarged; although, it is true, the author refers us to the particular minerals for individual instances; still it might have been well to have illustrated the general principles by a few well-chosen instances, *e. g.* how, by the blowpipe, *galena* is distinguished from *sulphuret of antimony; carbonat of lead* from *sulphat of barytes*, or *carbonat of lime; garnet* from *titanium; plaster of Paris* from *soapstone*, &c.; and, among trials in the moist way, how by nitric acid and ammonia, *iron pyrites* is distinguished from *copper pyrites;* and how, by acids, *sulphat of lime* is known from *carbonat of lime*. As the acids are used principally for trials on the effervescence of carbonats, most of which form with sulphuric acid, insoluble compounds, we should doubt whether sulphuric acid is so advantageously employed as the nitric or muriatic, in such cases, on account of the clogging of the effervescence by the thick magena, produced by a recently precipitated and insoluble sulphat.

According to our experience, the nitric or muriatic acid, diluted with two or three parts of water, is most eligible.

With respect to the blowpipe: it is *a convenience* to have a mouth-piece of wood, or ivory, joined to a tube of metal, as Mr. Cleaveland recommends; and some authors direct to have the tube attached to a hollow ball, for the sake of condensing the moisture of the breath; but every thing which adds to the expense and complication of the instrument will tend to discourage its use; we have never found any difficulty in performing every important experiment with the common goldsmith's brass blowpipe; and are confident, that, after the learner has acquired the art, or *knack*, of propelling a continued stream of air from his mouth, by means of the muscles of the lips and

cheeks, while his respiration proceeds without embarrassment through the nostrils, he will need no other instrument than the common blowpipe. Indeed it is a truly admirable instrument, instantly giving us the effect of very powerful furnaces, the heat being entirely under command, the subject of operation and all the changes in full view, and the expense and bulk of the instrument being such that every one may possess it, and carry it about his person.

The chapter on the principles of arrangement is worthy of all praise. This difficult subject is here discussed with such clearness, comprehensiveness, and candour, as prove the author to be completely master of his subject; and we are persuaded, that, on this topic, no author can be studied with more advantage. We forbear to extract, because the whole should be attentively perused in connexion, and scarcely admits of abridgement. We entirely agree with Professor Cleaveland, as we have already said, that the chemical composition of minerals is the only just foundation of their arrangement; that next in importance is the crystalline structure, including a knowledge of the primitive form, and integrant molecule; and last and least important, *in fixing the arrangement*, are the external characters: these last should be only provisionally employed, where the two first are not ascertained, or the second is not applicable. When the arrangement is once made, we *may*, however, and we commonly *shall*, in *describing* minerals, pursue precisely the reverse order; the external characters will usually be mentioned first, the crystalline characters next, and the chemical last of all. In description, the external characters are often the most valuable; if judiciously selected and arranged, they will always prove of the most essential service, and can rarely be entirely dispensed with.

With regard to the NOMENCLATURE of minerals, we feelingly unite with Professor Cleaveland in deploring the oppressive redundancy of synonymes. Few minerals have only one name, and usually they have several. With Count Bournon we agree, that the discoverer of a mineral has the exclusive right of naming it, and that the name once given should not be changed without the most cogent reasons. What then shall

we say of the ABBE HAÜY, of whom, whether we speak of his genius, his learning, his acuteness, his discoveries, his candour, and love of truth, or his universally amiable and venerable character, we can never think without sentiments of the highest respect and admiration? More than any modern writer he has added to the list of synonymes, often exchanging a very good name, derived perhaps from the locality or discoverer of a mineral, for one professedly significant, but connected with its subject by a chain of thought so slight, that considerable knowledge of Greek etymology, and still more explanation, is necessary to comprehend the connexion; and thus, after all, it amounts, with respect to most readers, only to the exchange of one arbitrary name for another. What advantage, for instance, has *grammatite*, alluding to a line often obscure, and still oftener wholly invisible, over the good old name *tremolite*, which always reminds us of an interesting locality; how is *pyroxene* better than *augite*, *amphibole* than *hornblende*, *amphigene* than *leucite*, or *disthene* than *sappar*. Some of the Abbé Haüy's names are, however, very happily chosen, especially where new discriminations were to be established, or errors corrected, or even a redundant crop of synonymes to be superseded by a better name. *Epidote* is an instance of the latter, and the new divisions of the old *zeolite family* into four species, *mesotype*, *stilbite*, *analcime*, and *chabasie*, afford a happy instance of the former. It were much to be wished, that by the common consent of mineralogists, one nomenclature should be universally adopted: for its uniformity is of much more importance than its nature.

In expressing our approbation of the principles of arrangement adopted by Professor Cleaveland, we have of course espoused those of his TABULAR VIEW, which is perhaps as nearly as the state of science will admit, erected upon a chemical basis, like that of Brongniart, to which it bears a close resemblance. Some of the subordinate parts, we could have wished had been arranged in a manner somewhat different. In the genus lime, it appears to us better to describe the species carbonat first; because, being very abundant, and its characters clear, it forms a convenient point of departure and

standard of comparison, in describing the other species which have lime for their basis, and some of which are comparatively rare. The same remark we would make upon quartz, and its concomitant, pure silicious stones. There appears to us a high advantage in making these minerals clearly known first, before we proceed to those which are much more rare, and especially which are much harder, and possess the characters of gems. For example, if a learner has become acquainted with quartz, chalcedony, flint, opal, chrysoprase, and jasper, he will much more easily comprehend the superior hardness, &c. and different composition of topaz, sapphire, spinelleruby, chrysoberyl, and zircon, which we should much prefer to see occupying a later, than the first place in a tabular arrangement; and, although topaz, by containing fluoric acid, appears to be in some measure assimilated to saline minerals, it is in its characters so very diverse from the earthy salts, that we have fair reason to conclude that the fluoric acid does not stamp the character; and, as it bears so close a resemblance to the ruby and sapphire, which evidently derive their principal characters from the argillaceous earth, we perhaps ought to infer that this (the topaz,) does so too. Indeed Professor Cleaveland has sufficiently implied his own opinion, by giving these minerals a juxtaposition in his table, although the same reasons which induced the placing of the topaz next to the earthy salts, could not have justified the placing of the sapphire there. On these points we are not, however, strenuous; they are of more importance if the work be used as a text-book for lectures, than as a private companion. With respect to the *completeness* of Professor Cleaveland's tabular view, we have carefully compared it with the third edition of Jameson's mineralogy; and although a few new species, or sub-species, and varieties have been added in this last edition, they are in general of so little importance, that Professor Cleaveland's work cannot be considered as materially deficient; and the few cases in which it is so, are much more than made up by his entirely new and instructive views of American mineralogy, to which no parallel is to be found in any other

book, and which give it peculiar interest to the American, and even to the European, reader.

In another edition, (which we cannot doubt will speedily be called for,) he will of course add whatever is omitted in this, and we should be gratified to see a good article on the subject of the ærolites or stones which have fallen from the atmosphere. This subject is one, in our view, of high interest; and although *in strictness* it may not claim a place in a tabular view of minerals, (we must confess, however, that we see no important obstacle to its being treated of under the head of native iron,) there can be no objection to its being placed in an appendix. The fall of stones from the atmosphere is the most curious and mysterious fact in natural history.

It may seem perhaps too trivial to remark, that the annexation of numbers, referring to the pages, would be a serious addition to the utility of the tabular view. Very few inadvertencies have been observed—the following may be mentioned: *Amenia*, in the State of New-York, is printed (by a typographical error we presume) Armenia; and *Menechan*, where the menechanite is found, is mentioned as occurring in Scotland, but it is in Cornwall.

Authors seem agreed that the black-lead ore is an altered carbonat, but they seem not to have been so well agreed as to the nature of the blue-lead ore. In the cabinet of Colonel Gibbs, there are specimens which appear satisfactorily to illustrate both these subjects. The black-lead is by the blowpipe alone reducible to metallic lead; there is one specimen in the cabinet referred to, which is blackened on what appears to have been the under side, and seemingly by the contact of sulphuretted hydrogen gas; that which was probably the upper part remains unaltered, and is beautiful white carbonat of lead; this appearance is the more striking, because the piece is large and full of interstices, by which the gas appears to have passed through. The blue ore is in large six-sided prisms of a dark blue or almost black colour; where the prisms are broken across, they present an unequal appearance; sometimes they are *invested;* and sometimes slightly, and at other times deeply, *penetrated* by sulphuret of lead, hav-

ing the usual brilliant foliated fracture. The part which looks like sulphuret of lead is easily reducible by the blowpipe, but not the whole crystal, as authors appear to imply; for if that part of the crystal which does not present the appearance of galena is heated by the blowpipe flame, it is not reduced, but congeals into the garnet dodecahedron, with its colour unaltered: these crystals are therefore phosphat of lead, and they appear to be either an original mixture of phosphat and sulphuret of lead, or the phosphat has somehow in part given up its phosphoric acid, and assumed in its stead sulphur, perhaps from the decomposition of sulphuretted hydrogen.

Professor Cleaveland will, of course, add new localities, even foreign ones, where they are interesting, and domestic ones, where they are well authenticated. Among the former, we trust he will mention the lake of sulphuric acid contained in the crater of Mount Idienne, in the Province of Bagnia Vangui, in the eastern part of Java, and also the river of sulphuric acid which flows from it and kills animals, scorches vegetation, and corrodes the stones.* Among American localities, we beg leave to mention violet fluor spar, abundant and very handsome, near Shawnee Town, on the Ohio, in the Illinois Territory, and galena, of which this fluor is the gangue;—sulphat of magnesia, perfectly crystallized, in masses composed of delicate white prisms, in a cave in the Indiana Territory, not very remote from Louisville, in Kentucky; it is said to be so abundant that the inhabitants carry it away by the wagon load;—pulverulent carbonat of magnesia, apparently pure, found by Mr. Pierce at Hoboken, in serpentine, where the hydrate of magnesia was found;—chabasie, agates, chalcedony, amethyst, and analcime, at Deerfield, by Mr. E. Hitchcock;—agates in abundance at East-Haven, near New-Haven, in secondary greenstone, like the above-named minerals at Deerfield;—saline springs, covered with petroleum, and emitting large volumes of inflammable gases, numerous in New-Connecticut, south of Lake Erie;—magnetical pyrites, abundant in the bismuth vein, at Trumbull, Connecticut;—very

* See Tilloch's Phil. Mag. Vol. XLII. p. 182.

brilliant fine-grained micaceous iron, in large masses near Bellows' Falls; yellow foliated blende, in Berlin, Connecticut, and near Hamilton College—the latter discovered by Professor Noyes; it is in veins in compact limestone;—red oxid of titanium, often geniculated, at Leyden, in Massachusetts, discovered by Mr. E. Hitchcock;—red oxid of titanium, in very large crystals and geniculated, imbedded in micaceous schistus, at Oxford, 20 miles north from New-Haven;—silicious petrifactions of wood, abundant in the island of Antigua, recently brought by Mr. Pelatiah Perit, of New-York;—sulphuret of molybdena, at Pettipaug, and at East-Haddam, Connecticut;—prehnite abundant and beautiful, in secondary greenstone, at Woodbury, 24 miles north of New-Haven, discovered by Mr. Elijah Baldwin;—black oxid of manganese, in great abundance, and of an excellent quality, near Bennington, Vermont, and plumose mica, in a very fine graphic granite, in a hill two miles north of Watertown, Connecticut.

The introduction to the STUDY OF GEOLOGY, deserves a more extended series of remarks than it would now be proper to make, after so full a consideration of the previous parts of the work.

Professor Jameson's elaborate exposition of the Wernerian system, is too full, and too much devoted to a particular system, for beginners: the sketches of geology contained in the systems of Chemistry by Murray and Thomson, and in Phillips's mineralogy, are two limited, although useful: the excellent account of the Wernerian system, contained in an Appendix to Brochant's Mineralogy, has, we believe, never been translated; and we need not say that Professor Playfair's illustrations of the Huttonian Theory, De Luc's Geology, and Cuvier's Geology, are not well adapted to the purposes of a beginner; neither is Delametherie's, nor has it been translated. An introduction to geology was, therefore, hardly less needed than one to mineralogy. Professor Cleaveland has performed this difficult duty with great ability, and has brought this interesting branch of science fairly within the reach of our students.

Although adhering substantially to the Wernerian arrangement of rocks, he has, so to speak, blended Werner's three

classes of primitive, transition, and secondary rocks, into one class; and where the same rock occurs in all the three classes, or in two of them, he mentions it in giving the history of the particular rock. This method simplifies the subject very much to the apprehensions of a learner. A rigid Wernerian would probably revolt at it, but the distinctions of Mr. Werner may still be pointed out, and, we should think, ought to be, at least by all teachers.

In Mr. Cleaveland's account of the trap rocks, we should almost imagine that some typographical error had crept into the following paragraph:

"But in modern geological inquiries, the word trap is usually employed to designate a *simple mineral*, composed of hornblende nearly or quite pure, and also those aggregates in which *hornblende* predominates. Hence, the *presence* of hornblende, as a predominating ingredient, characterizes those MINERALS to which most geologists apply the name *trap*."

Now, it is not accordant with our apprehensions that trap is ever at the present time employed to designate a *simple mineral*, nor has Professor Cleaveland himself used it in his tabular view, or in his description of simple minerals. In our view, it is the *classical* word of modern geology, to designate that description of rocks in which hornblende predominates, and perhaps a few others of minor importance usally associated with them. It is true, a rock composed of pure hornblende may be called trap, but it is not true, *vice versa*, that this rock, considered in its character of a simple mineral, is called trap. If our views are correct, the section which is headed *trap* or *hornblende*, should be *trap* or *hornblende rocks*, and greenstone should come in as a subdivision, and not form a distinct section. With these alterations, and with the substitution of rock in the *first*, and rocks in the *second* instance, in the paragraph above quoted, instead of *mineral* and *minerals*, we apprehend the view of this family of rocks would be much more clear, and a degree of confusion, which learners now experience from the paragraph, would be prevented. If we are wrong, we are sure Professor Cleaveland will pardon us; if right, his candour will readily admit the correction.

As to the manner in which the work of Professor Cleaveland is executed, the remarks which we already made, have in a good degree anticipated this head.

We cannot, however, dismiss the subject without adding that, in our opinion, this work does honour to our country, and will greatly promote the knowledge of mineralogy and geology, besides aiding in the great work of disseminating a taste for science generally. Our views of the plan we have already detailed. The manner of execution is masterly. Discrimination, perspicuity, judicious selection of characters and facts, and a style chaste, manly, and comprehensive, are among the characteristics of Professor Cleaveland's performance. It has brought within the reach of the American student the excellencies of Kirwan, Jameson, Haüy, Brochant, Brongniart, and Werner; and we are not ashamed to have this work compared with their productions. In our opinion Professor Cleaveland's work ought to be introduced into all our schools of mineralogy, and to be the travelling companion of every American mineralogist.

We trust that all cultivators of mineralogy and geology in this country, will willingly aid Professor Cleaveland in enlarging his list of American localities for a second edition; and we hope that he will repay them, at a future day, by giving us a distinct treatise on geology, with as particular a delineation as possible of the geological relations of the great North American formations. Mr. Maclure has, with great ability, sketched the outline; but much labour is still needed in filling up the detail.

Art. III. *New Locality of Fluor Spar, or Fluat of Lime and of Galena, or Sulphuret of Lead.*

Mr. JOSEPH BALDWIN, formerly of Connecticut, now residing near Shawnee Town, in the Illinois Territory, has given us some interesting specimens of fluor spar. They are found not far from Shawnee Town, on the banks of the Ohio;

and a few miles below where the Wabash joins the Ohio. The fluor forms the gangue of a lead vein, and we have pieces in which the lead and fluor are intimately blended. The lead ore is the common galena, or sulphuret, with a broad, foliated, or laminated fracture, and a high degree of metallic splendour. We reduced it to the metallic state, and it yielded a large product of very soft lead. On dissolving it in nitric acid, and applying the muriatic acid till precipitation ceased, the precipitate formed was *all redissolved* by boiling water; nor, when submitted to cupellation did the lead leave any thing upon the cupel. We, therefore, conclude that it contained no appreciable quantity of silver. It is said to be very abundant at Shawnee Town.

The fluor spar is very beautiful. Its colours, chiefly, very deep purple and violet; but still highly translucent; one specimen was entirely limpid. Both kinds, when thrown in coarse powder, on a red-hot shovel, in a dark place, phosphoresced, and the violet specimens in a very striking manner. Of the violet kind, we have a specimen nearly as large as a man's fist, which is perfectly pure and sound, and appears to have been a single crystal; the natural faces and angles were unfortunately obliterated by grinding on a common grindstone. We have others which are decidedly crystals of perfect regularity; cubes, and passages between the cube and octahedron. In some of the specimens, the disposition of colours, and the transmission of light is such as to show very clearly that the octahedron lies in the centre, as the nucleus or primitive form.

The size and beauty of the specimens, and the abundance of this mineral near Shawnee Town, (provided there is no mistake in the case) clearly entitle this to be considered as the most interesting American locality of this beautiful mineral. Measures have been taken to investigate the subject more fully, and to obtain a supply of specimens.

Quartz crystals appear to abound at the same place, besides various other minerals.

ART. IV. *Carbonate of Magnesia, and very uncommon Amianthus, discovered near New-York.—Extract of a Letter from Mr. James Pierce to the Editor.*

New-York, May 18, 1818.

DEAR SIR,

I FORWARD you specimens of straw and rose-coloured amianthus I recently met with on Staten-Island, which I detached, in strips, from a rock; it not appearing, as is usual, in veins. It breaks up like flax, and may be spun and wove without the aid of moisture; and in respect to tenacity, flexibility, and length of fibre, it may be considered the best found in this country, and perhaps equal to any hitherto discovered. Staten-Island exhibits many minerals worthy of examination. I subjoin, as requested, the following geological description, &c.

Hoboken, where I discovered native carbonate of magnesia, is situated opposite the city of New-York, on the western or New-Jersey bank of the Hudson. It is a primitive, insulated elevation, with a nucleus of serpentine; the ground gradually descends in every direction except on the river side, where mural precipices of serpentine rock are observed, extending about 100 rods parallel with the water, and elevated from 60 to 100 feet above its level. The carbonate of magnesia I found in horizontal veins of nearly two inches in breadth, and of unknown depth, in a midway region of this serpentine ledge; I extracted a considerable quantity with a spoon. When first taken out it was soft, white, and very slightly adhesive, from a little moisture; but, when dry, fell to powder without friction. The nature of the mineral I immediately conjectured, and treated it with diluted sulphuric acid, in which it entirely dissolved with effervescence, forming a bitter fluid, and leaving no sediment. Upon evaporation, well-defined crystals of Epsom salts were formed. It differs little from the manufactured carbonate of magnesia of the shops; but is rather a super than a sub-carbonate. It has been analyzed by

Professor Mitchill, who found it exclusively composed of magnesia and carbonic acid. Carbonates of magnesia, hitherto discovered, have been, I believe, found impure, and in a state of rock, requiring chemical process to render them serviceable; this is, perhaps, fit for immediate use. When I first mentioned the discovery to mineralogists, they were incredulous, supposing it did not natively exist in this state, but I convinced them by uniting it with sulphuric acid.

REMARKS.

The specimen of amianthus, referred to in Mr. Pierce's communication, is uncommon. The fibres measure from 12 to 15 inches in length, and are as soft and flexible as fine human hair.

It will be remembered, that in the rocks at Hoboken, Dr. Bruce discovered the hydrate of magnesia, or magnesia combined with nothing but water, in the proportion of about 70 per cent. of magnesia. This discovery gave a new and interesting species to mineralogy; it is now admitted in the systematical works on mineralogy.

Mr. Pierce's discovery is not less interesting; and we presume he will be deemed correct in the opinion, that pure native carbonate of magnesia has not been discovered before. The serpentine of Hoboken, then, is memorable for affording these two new species.

Art. V. *Native Copper.*

In Bruce's Journal, (Vol. I. p. 149.) mention is made of a remarkable piece of native copper, found near New-Haven many years ago, and weighing about 90lbs.

We have now to add, (and the fact is, indeed, mentioned in Cleaveland's Mineralogy,) that another piece has been recently found half a mile west of the Hartford turnpike road, opposite the town of Wallingford, and twelve miles from New-Haven. It was turned up in ploughing to repair a

road. The country is of the secondary trap formation, and the rocks, at the particular place, are the old red sandstone of Werner, which here occupies the plains, and runs under the trap. The piece weighs almost six pounds ; it is fine virgin copper, with rudiments of large octahedral crystals of native copper upon its surface, which is more or less incrusted with green carbonate of copper and ruby oxid, very much resembling that of Cornwall : the ruby oxid is particularly remarkable in the cavities of the piece.

As it was found within three or four miles of the place where the large piece of ninety pounds weight was discovered, and as copper is known to exist in many places in these hills, the facts should be kept in view, and may lead to something of importance.

Art. VI. *Petrified Wood from Antigua.*

THE mineralogy and geology of the West-India islands has been, as yet, but little explored. The scientific world has, however, been favoured with some interesting articles from the pen of Dr. Nugent ; and we are informed that he has described also the geology of the island of Antigua. We have recently become acquainted with one interesting production of this island, and without waiting for Dr. Nugent's account, (which we believe has not yet reached this country) we shall lay it before our readers.

We are under obligations to Mr. Pelatiah Perit, of New-York, for a collection of specimens of silicious petrifactions of wood from Antigua. Their characters are indubitable ; the distinct ligneous layers corresponding with the annual growth, the medullary prolongations, the knots formed by branches, the cracks and the bark, are all distinctly visible. Some of the pieces are ponderous portions of large trees.

As to the mineralizing matter, it is evidently silicious, and the specimens are principally the holzstein of Werner ; crystals of quartz are apparent in the cavities ; some parts are

agatized, and veins of chalcedony occasionally pervade the fissures: they are not impressible by steel, and give fire with it. According to the information of Mr. Perit, they are scattered over the surface of the Island of Antigua, with a profusion hardly less than that which Horneman observed of the same mineral during his travels over the eastern part of the great African desert.

It is much to be wished that our numerous intelligent navigators and travelling merchants would, in imitation of this and of a similar example, mentioned below, bestow some share of their attention on the natural productions of the countries which they visit. In this way they might, on their return, render very essential services to the science of their own country.

Art. VII. *Porcelain and Porcelain Clays.*

THROUGH the kind offices of a friend, we have been furnished, from one of the great porcelain manufactories in the vicinity of Paris, with a series of specimens, to illustrate the elegant art of fabricating porcelain. The specimens begin with the raw materials, and exhibit them in all their principal stages of advancement up to the perfect vessel, including the materials for the glazing, and the colours for the painting, and the application of both. At the request of the manufacturer, through whose liberality we were indulged with this gratification, we transmitted to Paris various specimens of American porcelain clays. This gentleman has caused them to be subjected to trials in the porcelain furnaces, and he finds that some of them are equal to the French porcelain clays, and some superior. As our specimens were all labelled with the names of the places, in this country, from which they were obtained, we hope soon to learn where to look for porcelain clays, equal or superior to those celebrated ones from which the superb French porcelain is manufactured.

As this subject is one of much practical importance to the rising arts of this country, and as much interest has been excited in Paris concerning our porcelain clays, we should feel greatly obliged by the transmission to us of any specimens of American porcelain clays, with memoranda of the place, the quantity, the depth at which obtained, the difficulty of obtaining, and, generally, all the peculiar circumstances. We will take care that their value shall be acertained, if they appear promising, and a proper return shall be made to the proprietor.

To those of our readers who may not be familiar with this subject, we would however take the liberty to remark, that porcelain clays generally arise from the decomposition of granite, and particularly of that kind which is denominated graphic granite, and which abounds with feldspar. It is, therefore, in the primitive countries that we are chiefly to expect them—such as New-England, and part of the high country of the middle and southern states.

It should be observed, that if a clay, otherwise apparently good, burns red, it contains iron, and is unfit for porcelain; although it may serve well enough for more common and coarse earthen ware.

Art. VIII. *Native Sulphur from Java.*

THROUGH the kindness of Mr. I. Huntington, recently returned from Java, we have received from that Island some fine specimens of native sulphur. They are very pure, of an orange yellow, slightly shaded with white, and occasionally with red; some of the cavities are lined with delicate crystals. What gives them particular interest is, that they are believed to be from that "large, and now nearly extinct, volcano, about sixty miles from the town of Batavia, at the bottom of which (of the crater) lie large quantities of native sulphur, even many hundred tons." It is in the crater of this volcano that the famous lake of sulphuric acid exists, and from which it flows down the mountain, and through the country

below, a river of the same acid. (See Tilloch's Phil. Mag. Vol. XLII. p. 182.) It is a most curious phenomenon, and we believe entirely without a parallel. Another river, called the White River, unites with this some miles below its origin : this river, which is so called from the turbidness of its waters, is salutary to men and animals ; fishes live in it, and vegetation is nourished by its waters ; but after the junction it becomes clear ; the acid dissolving the earthy particles which discoloured it, and it now becomes fatal to living beings : kills the fish, destroys the vegetation, and corrodes the stones in its channel. This remarkable river flows from Mount Idienne, in the province of Bagnia Vangni, in the eastern part of Java.

ART. IX. *Productions of Wier's Cave, in Virginia.*

WE are indebted to the Reverend Elias Cornelius, and to Mr. John H. Kain, for a collection of the calcareous incrustations of Wier's Cave, in Virginia.

The stalactites, and stalagmites, and various incrustations, are of uncommon size and beauty. Some of the stalactites have a delicate whiteness, and a brilliancy arising from their crystallized structure, which, with the regularity of their forms, give them a fair title to rank with those of the famous caverns in the Peak of Derbyshire, in the island of Antiparos, &c.

In these stalactites, the structure is most remarkably distinct, both in the fibrous and concentric lamellar form. In this collection were observed many forms of the crystallized hard carbonates of lime, of Count Bournon.

For a description of the cavern from which these specimens came, we refer to the succeeding memoir, by Mr. Kain

ART. X. *Remarks on the Mineralogy and Geology of the the Northwestern part of the State of Virginia, and the Eastern part of the State of Tennessee.* By Mr. JOHN H. KAIN, of Tennessee.

THE most prominent as well as the most beautiful feature of this country, is that succession of mountain and valley, ridge and vale, which we meet with in traversing its surface. The grand range of Alleghany mountains enters Virginia about the 39th degree of north latitude; and, pursuing a southwestern course, spreads out upon the east end of Tennessee, and terminates near the southern boundary line of that state, in the Alabama territory; and about the 34th parallel of north latitude. In this view are included the Blue Mountains, the North Mountains, the Alleghany, (properly so called) the Cumberland, Clinch, Iron, and Smoky mountains, together with a variety of smaller mountains, spurs, and ridges, all running parallel to each other, from the northeast to the southwest; and all, I believe I may say, covered with forests, and presenting to the eye of the naturalist a most interesting field for speculation and improvement.

With a few exceptions, the geologist meets with none of those remarkable appearances which indicate the changes and convulsions which have been wrought by time, the great enemy of nature. Occasionally we are presented with a view of a sublime precipice, formed by a section which a river appears to have made for itself through an opposing mountain; and the large masses of ruins, which lie scattered around such a place, seem, to the imagination of the solitary traveller, the historical records of commotions, awful even in retrospect. Most commonly, however, the mountains seem to have lain for ages in undisturbed repose; and the streams of water, when they have crossed them, have sought an easy passage through the ravines, which do not so often divide a mountain, or ridge, at right angles, as wind between the ends of two opposing spurs, which pass each other, gradually declining into the

champaign country at their mutual base. Through this whole extent of country we rarely meet with any remarkable falls of water; the obvious reason of which is, that the rocks are so soft that they are easily worn down to the level of the beds of rivers. But shoals, or shallows, are frequent, and are formed by beds of rounded sandstone, spread out into a broad base, over which the water often rushes with no small violence and noise.

The mountains are generally, though not always, sterile, and produce nothing but forest trees; but the valleys are, with hardly an exception, rich, and productive of every variety of "grass and herb yielding seed, and fruit-tree yielding fruit." Nor are they less favoured in the mineral kingdom; possessing the greatest abundance of all the most useful and necessary minerals, of which we shall now proceed to speak in order.

All the country included under the boundaries mentioned above, with the exception of some primitive ranges of mountains on the southeastern side, is apparently *transition.* This, it will be seen by a reference to Mr. Maclure's excellent map, will extend the boundary of his transition class considerably farther northwest, and make it include Cumberland Mountain and all East Tennessee. This would be evident from comparing the northwestern part of Virginia, which Mr. Maclure has included in his transition tract with all East Tennessee. Every mineralogist must observe the identity of the minerals of the two countries as well as that of their stratification and general formation. The limestone in the valleys, and the sandstone on the mountains, lie in strata which make an angle of from 25 to 45 degrees with the horizon. The limestone bears the impressions of shells, but rarely, if ever, of vegetables, and contains beds of hornstone, but not of flint, or what can properly be called flint.

The rock which lies in the lowest valleys, and often rises into pretty high hills, and is seen forming bluffs on the banks of the rivers, is *limestone:* it is of a dark blue, approaching to a gray, as it is exposed to the air, and often appearing quite white. Its fracture is compact in one direction; in another it

is more or less slaty in its structure. It is interspersed with veins of the crystallized carbonate of lime, more or less perfect, and of a pure but opaque white. Another variety of this limestone, not so abundant, is that which is white and red, having the white and red spots intimately mingled. Its structure is similar to the other kind.

Lying in beds of this limestone, parallel to, and imbedded in, its strata, is a stone, which, from its globular form, its hardness, and its colour, has been usually mistaken for flint. On comparing it with the flint of chalk-beds, we find it much less translucent, its colour darker, and its hues duller; and its rough and irregular fracture, compared with the easy, smooth, and conchoidal cleavage of the true flint, decides it to be hornstone. It is found also forming considerable distinct beds on the hills; and is seen in detached pieces, and irregular pebbles, covering many of the ridges.

Alternating with the beds of limestone, and possessing the same formation, is a soft *clay slate*. *Soapstone* is found in it.

As soon as we ascend the mountains, we meet with a slaty sand-stone of various compactness, as it possesses more or less iron, often forming an excellent iron ore. A variety of this iron ore has been lately turned to a good use, in the manufacture of a red paint, near Knoxville, Tennessee. Different varieties of this sandstone possess different qualities. It is converted by the inhabitants into millstones, grindstones, and whetstones. Interspersed among the sandstone of the mountains we often find very beautiful and interesting specimens of hornstones, assuming a resemblance to all the silicious stones, from the chalcedony to the jasper. In this extensive range of mountains, many other minerals exist, of which we shall treat more particularly hereafter. The limestone, slate, and sandstone, as far as the writer's knowledge extends, so to speak, *form the country;* the limestone and clay slate dipping under the sandstone. Gypsum, coal, sulphate of barytes, &c. are found in these, and we shall now speak of their localities.

Gypsum.—This valuable mineral production exists in Washington County, Virginia, 20 miles north of Abingdon, in the vicinity of Saltville. It is similar, in every respect, to the

plaster of Nova Scotia, and devoted by the farmers of that part of Virginia, and Tennessee, to similar purposes.

Coal is said to exist in immense quantities in the Cumberland Mountain. A bed of it is wrought near Knoxville, Tennessee. It is of an excellent quality; but wood is so abundant that it is used only in forges.

Sulphate of Barytes.—This mineral is found in Bottetourt County, Virginia, near Fincastle; and in Sevier County, Tennessee.

Hard Carbonates of Lime.—Stalactitical concretions abound in all the caves so often described as existing in this country. Those of Virginia are more perfectly crystallized than those of Tennessee. Under the head of *hard carbonates* should be mentioned an extensive bed or vein in Montgomery County in the State of Virginia, near the seat of Colonel Hancock. It appears to have been formed in a chasm, in the common limestone of the country, by a calcareous deposition which resembles, exactly, in all its characters, the calcareous concretions which are found forming in the caves of the country. The whole bed may, in fact, be regarded as a cave which has been filled up in the progress of time, by this curious process. Its width is various, from two feet to ten, or more, extending along the side of a very steep ridge, for at least 50 yards, and it is said to be continued seven miles farther.

The silicious carbonate of lime may be worth distinguishing from the common limestone. It is found in a bed near Colonel Hancock's, and was supposed to be gypsum. It phosphoresces beautifully; it is white, and confusedly crystalline in its structure, and much harder than the common limestone. Indeed the limestone generally, on the east of the Alleghany, is somewhat harder than that on the west.

Lead.—There are several localities of this mineral. A mine of it is wrought near New River, 15 miles from Wythe, Virginia. Another locality of the ore of lead is said to have been discovered in Granger County, Tennessee, on land belonging to General Cocke. It exists also, very near the surface, on the plantation of the Rev. Mr. Craighead, near Nashville; which, however, is out of our boundary.

Other metallic ores are said to have been found among these mountains, and particularly those of gold and silver; but the accounts are vague and uncertain, and not to be credited.

The numerous *Caves* of this country present attractions to every, the least curious, traveller; and, in an eminent degree, to the mineralogist. They are crevices, or large chasms, probably worn in the rocks by the passage of water This will, at first view, perhaps appear a bold assertion; but if it be recollected that they occur only in limestone, which is a soft rock, and (under certain circumstances,) soluble in water; that the rocks bear every mark of having been worn by water; and that streams of water are always found in them, it will not appear an improbable hypothesis. It is by no means difficult to believe that a stream, after having worn such a chasm as a cave presents, in the solid rock, may have found another channel; and, forsaking the old, have left room for nature to display some of her most beautiful works. A description of one of these caves will be a description of all; and we shall select *Wier's Cave*, in Rockingham County, Virginia, as it is the most curious of any with which we are acquainted.

The entrance of the cave is narrow and difficult. When the cave was first discovered, the passage into it was impeded by stalactites, which had formed perpendicular columns across it; but these are now removed. As we advance, our course is at first horizontal, but we soon descend fifteen or twenty feet by a ladder, and find ourselves in a large echoing cavern. Stalactites of a silvery whiteness are suspended from above, and pillars of stalagmites are rising around us. Ledges of rocks form our floor, and the uneven walls are incrusted over with a beautiful brown spar, which is sometimes suspended from the canopy in thin, shining, and translucent sheets. In passing on over the rugged rock of our pathway, our attention is divided between a care for our safety, and an admiration of the surrounding wonders.

Proceeding on through a narrower crevice in the rocks, we are soon introduced into other apartments, differing in shape and size from the first, but resembling it in the irregularity of

its walls, floor, and covering, and in the calcareous incrustations and concretions, which, assuming a thousand fantastic shapes, and displaying a sparkling lustre, the more vivid as the light is stronger, give to this whole grotto the power of charming every beholder.

The cave is a mile and a half in extent, and extremely irregular in its course and shape. Its perpendicular height varies from three to forty feet, and its breadth from two to thirty. Its dividing branches are numerous, forming a great variety of apartments. The blue limestone appears frequently enough to satisfy us that it is the groundwork of the whole; but it is almost every where covered with incrustations of the hard carbonates. These hang from the arched vault above in clusters, and often reach the ground, forming massive columns. Stalagmites again rise from the floor like so many statues; the irregular sides of the ledges of rocks are often incrusted over with white crystals of the carbonate of lime, and have the appearance of banks of salt: at times we seem to walk on diamond pavements; again our footway is of rounded pebbles, and seems the bed of a river which had deserted its channel. Often we pass small streams of water; and the water is continually dripping from the ends of the stalactites, the echoing sound of which, when it drops, forms the only interruption to the profound silence which reigns throughout the cavern.

To give an idea of the diversified shapes which these concretions assume in the progress of their formation, (and they are constantly forming,) would be impossible. Suffice it to say, that there is scarcely any thing on earth to which they may not be supposed to form a resemblance; and yet, in fact, they are unlike any thing but themselves.

It is generally known that the earth in these caves contains the nitrates of lime, and potash, and other salts. The numerous caves which have been found in the Cumberland mountains and other parts of Tennessee, have been very productive of the nitrate of potash. In the investigation of the causes which have given origin to these salts, it may be recollected, that wild animals burrow in these caves; that when pursued by the hunter, they make them the places of their retreat, and

probably die there ; that the aborigines have made them a place of burial ; and that the streams of water which flow through them in wet weather, carry with them not only great quantities of leaves but many other vegetable productions.

The *natural bridge* is celebrated as one of the greatest curiosities of the world. Viewed by a geologist, it would probably be considered as a cave (so to speak) *unroofed* in all but one place. It seems improbable that if the ravine had been made by a convulsion, which had split and separated the rock to the distance of fifty or sixty feet, any part of it, and particularly so large a mass as that which forms the bridge, should have been left, without exhibiting any marks of violence. The rock is limestone. It is known that this rock wears away rapidly under the attrition of water ; and the supposition does not appear improbable, that, in the lapse of ages, so large a creek as that which flows below the bridge, may have worn as deep a ravine as that which now strikes us with so much surprise. In short, may not a cave have been originally formed where the ravine is now, and the pending portion of it have fallen in at every place except that which now forms this celebrated natural curiosity ?

Mineral Springs.—The mineral springs of this region are numerous and diversified. Chalybeate springs are promiscuously scattered over the whole of it ; and springs impregnated with sulphuretted hydrogen are quite common. Salt springs and licks are found more in the western than the eastern range of mountains. That which was first wrought by William King, is well known. The salt here is associated with gypsum. In the same range of mountains, farther to the southwest, there are now several other salt-works, and also one to the west, on Goose Creek, in Kentucky, which has been very productive.

The Warm Springs.—These springs are situated in a country which presents many attractions to the travelling geologist ; and much light, it is hoped, will yet be thrown on the geology of our country, by a more minute and accurate examination of it than has yet been made.

The warm springs ooze through the sand on the south bank of the French Broad river, in the mountains which divide the

state of Tennessee from her parent state, about the 36th parallel of latitude. The temperature of the water is about 95° of Fahrenheit.

On the opposite side of the river from the springs is a geological curiosity. A limestone rock is seen dipping under the sandstone which forms the country. Limestone is nowhere else to be seen within six miles of this place. In this limestone rock is a cave similar to others already described.

Paint Rock, in the vicinity of the Warm Springs, is interesting on many accounts. It is a bold precipice on the bank of French Broad river. At this place the river passes with a very rapid current directly across the course of a mountain, which terminates abruptly, and forms the precipice on the north bank of the river. On looking at the rock, the opposite end of the mountain, and the ruins around it, the mind is insensibly carried back to the contemplation of some dreadful commotion in nature, which probably shook these mountains to their bases.

The rock is composed of a *clay slate;* and it is here again remarkable, that this stone is not to be seen in any other place within some miles. It has received its name from some red paintings, (probably left on it by the Indians,) which have the appearance of hieroglyphics.

To conclude. It will be seen from the above observations, that this country presents a vast field of most interesting research, and claims the attention of every traveller who is interested at all in geological inquiries. If what has been said will at all contribute to the enlargement of the general stock of our knowledge on these subjects, the writer will be much gratified; and it is his sincere wish, that the accuracy of his remarks may be tried, and his mistakes corrected, by the researches of succeeding travellers.

Art. XI. *Notice of Professor Mitchill's Edition of Cuvier's Essay on the Theory of the Earth.*

THE American scientific public are under obligations to Professor Mitchill for bringing this book within their reach. It is one of the most eloquent, impressive, and instructive works on this grand but obscure subject, with which the world has ever been favoured. The reader is no sooner drawn within the current of Cuvier's eloquence, than he is borne along almost without the power or wish to escape. It is believed there are few intelligent and enlightened persons, whether geologists or not, who would fail to be gratified by a book which secures the understanding by a strict course of reasoning from facts, and delights the taste by a style bold, terse, and lucid, but at the same time rich and flowing.

The analysis of this work has been ably performed in Europe, and there is, therefore, the less necessity to attempt it here. While we take the liberty thus to recommend it, we do not hold ourselves strictly bound to the admission of *every one* of Cuvier's doctrines ; and might, perhaps, wish that in a few instances he had been somewhat more explicit, or somewhat more qualified.

The additions by Professor Jameson, of Edinburgh, are valuable and interesting, and are retained in the present edition.

Those by Professor Mitchill will be perused with pleasure and advantage. The learned author has assembled, in one view, a great mass of facts, partly resulting from his own journeys and observations, and partly deduced from other respectable sources. We have no doubt that most of these facts will be considered by the scientific world as very interesting, whatever views they may entertain of the conclusions built upon them. The author has occupied himself principally upon those portions of the United States, which, by the organized remains both of animals and vegetables, with which they more or less abound, exhibit the most decisive and interesting

evidence of changes and catastrophes, whose history is to be sought in the memorials entombed in the strata themselves.

We give no opinion regarding the theories of Professor Mitchill, not intending to review the work, but merely to aid, as far as in our power, in drawing the public attention to the interesting subjects about which it is occupied.

If we have any remark to add, it is, that an adherence to the technical precision with which most rocks are at the present day described, appears desirable in mineralogical and geological descriptions. When in the valuable additions before us we read of schorl rock, we gain only the idea of a rock containing that mineral; but as it occurs occasionally in several of the primitive rocks, we are at a loss which is intended; we believe it never forms a rock by itself. So with the slate rocks: there are several varieties of them—mica slate, clay slate, greenstone slate, &c. besides some subdivisions; and the mere word slate does not always give us the precise idea. But we are aware that, in the present case, it was less in view to go into all the details of geological description, than to give a view of our organized remains and of their supposed origin.

ART. XII. *Notice of Eaton's Index to the Geology of the Northern States, together with a Transverse Section of the Catskill Mountain to the Atlantic.*

THE extensive collection of facts in this little book of fifty-four pages, is creditable to the author's industry and discernment: he informs us that he has travelled 1000 miles on foot, while investigating the geology of the district concerning which he has written. This district is certainly interesting, and every attempt to diffuse correct information concerning it, deserves encouragement. Mr. Eaton's account of the regions he has explored, has every mark of verisimilitude; and we commend his efforts to diffuse geological information, by short courses of lectures, in different towns. In his arrangement of rocks, he has deviated from Werner—has adopted some views

of Bakewell, and some of his own. Werner's arrangement of rocks has, undoubtedly, its imperfections and its redundancies; and yet it may be questioned how far his system has been really improved by its different emendators. If Werner, by mentioning argillaceous schistus only in the primitive class of rocks, left us to dispose of it where we might, when we find it at one time, covering or sustaining anthracite, with impressions of ferns, and at another with impressions of fish and vegetables, and in contact with bituminous coal; still those who, with Mr. Eaton, throw argillaceous slate into the transition class, and omit it in the primitive and secondary, embarrass us with an equal difficulty; for we find argillaceous slate in contact, and alternating with, mica slate, and without any impressions of organized bodies, when we must, without a doubt, call it primitive.

This is the fact with the clay slate of the Woodbridge hills, near New-Haven, which is primitive; that of Rhode-Island, with anthracite, is transition; and that at Middlefields, west of Middletown, with impressions of fish, is secondary. Slate then appears to belong to all these three great classes of rocks.

As to the *metalliferous* limestone, we do not so much object to the introduction of this term by Bakewell, although it appears to us quite as well to say that certain limestones, those of the transition class for example, are metalliferous. But is Eaton correct in referring such limestone as that of which the New-York City-Hall is built, to a metalliferous class? Is not that limestone decidedly primitive? The fact mentioned of its containing pyrites, hardly proves it to be metalliferous; since most rocks contain more or less of pyrites. Some other remarks of less importance we might add, but we prefer concluding by recommending this tract to the perusal of those who wish for information respecting the geological structure of New-England; and we think that Mr. Eaton is seriously aiding the progress of geology in the interior of New-England.

Art. XIII. *Notice of M. Brongniart on Organized Remains.*

THIS distinguished mineralogist, so advantageously known by his excellent work on mineralogy—his researches in company with Cuvier, into the subterranean geography of the environs of Paris, and his superintendence of the great porcelain manufactory at Sevres, is attempting to form an extensive collection of organized remains.

Through Professor Cleaveland, we have received from him the following

NOTICE

Concerning the method of collecting, labelling, and transmitting specimens of fossil organized bodies, and of the accompanying rocks, solicited by M. Brongniart.

The study of fossil organized bodies appears to be of the utmost importance in determining the relations of different formations, one of the principal objects of geology.

In order more effectually to appreciate the value of this method of investigation, it is necessary to multiply observations—to endeavour to render them exact and precise—and especially to make them upon a general plan.

M. Brongniart has been long occupied in such researches. The essay published by M. Cuvier and him, upon the geology of the environs of Paris, has afforded an example of their use.

He has laboured since this period to apply this method to other formations, which contain the relics of organized bodies; but he stands in need of much assistance, and he presumes to ask it, not only of naturalists, but even of all persons interested in the sciences. By means of the following instructions, he endeavours to avail himself of the kindness of persons the least conversant in the discrimination of fossils.

1. To collect all the fossil organized bodies which can be obtained; especially *the distinguishable impressions and remains of vegetables* from coal countries, and beds of wood, coal, and others. The *shells*, *crustaceæ*, *madrepores*, *fishes*, &c. It is not necessary that these bodies should be either large or entire, but they must be sufficiently characterized to be capable of being recognized.

It is useless to transmit large unmeaning pieces, which are recommended only by their size—such as large ammonites—large madrepores—large pieces of petrified wood—fragments of the one, or small individuals of the other, are often sufficient. We may avoid also collecting the inner moulds ("des moules interieurs") of shells, because they are almost invariably incapable of being recognized.

2. Petrifactions, isolated and detached from their rock, are the most convenient in the determination of species; but when they cannot be separated from the rock, we need not hesitate to send them engaged; it is sufficient if a portion large enough for discrimination is visible.

Among shells, those are preferable which have the mouth or hinge in view; among madrepores, those on whose surface the figures (les étoiles) are distinguishable; among vegetables, those whose leaves are distinctly expanded, (expalmées.)

3. Upon the objects tramsmitted it is desirable to have, at least in part, the following notices:

1. The exact place from which the object comes: this is the most important circumstance, and the easiest to obtain.

2. The kind of formation in which it is found, and a specimen of the stratum, or at least of the rock, which contained it. It is desirable that this rock exhibit remains of petrifactions similar to those found in the stratum from which it has been drawn.

3. The nature of the formation of which this stratum or rock composes a part, and specimens of as many of the superior and inferior strata as can be obtained, designating the order of superposition of the strata.

4. It is important to designate, by the same mark, all the petrifactions *unquestionably* found in the same stratum, or at

least in the same formation. The specimens ought to be almost square—about three inches or more on a side, and one and a half thick.

5. It is equally important not to mix petrifactions found in different formations, or in different strata of the same formation; or if they are packed together, to distinguish them by numbers, marks, or labels.

When the preceding notices cannot be obtained, the first will suffice.

In order to collect the petrifactions, and to render them useful, it is not necessary to know them, nor to be perplexed to find them out; nor to be afraid of sending objects already known or of little note. A part of the preceding indications, connected with the most common petrifactions, will always render them useful. The important point then is, not to mix those which are found separate, nor to separate those which are found associated in the same stratum.

This is easily attained, by designating by a common number, letter, or any sign whatever, one particular formation or stratum, and by marking with the same sign all the petrifactions which are evidently found together.

The labels designating the place or the geological situation, may be placed in the papers which envelope the specimens, or a number, referring to an explanatory catalogue, may be attached to each specimen.

As far as possible, it is necessary to stick the labels or numbers to the pieces, by pasting; and the surest way is, to write upon the piece itself, 1st, the place where it is found; 2d, the number by which it is indicated in the historical notes above requested.

If there is not time to make out as many numbers or labels as there are pieces, it will be sufficient to unite in one box or packet all the petrifactions of one particular stratum, and to designate them by a general label.

It is necessary to pack the shells and other fragile pieces in separate boxes, and to wrap each piece in a separate paper.

M. Brongniart cannot allow himself to prefer such requests, except under the express condition, that a memorandum of all

the expenses which the transportation and packing of the specimens may create shall accompany the letter of advice.

The objects destined for him may be sent by the common modes of conveyance, with a letter of advice, to the following address:

Mr. A. BRONGNIART, Member of the Royal Academy of Sciences, Engineer of Mines, etc. Rue Saint-Dominique, Faubourg Saint-Germain, No. 71, Paris.

ART. XIV. *Observations on a species of Limosella, recently discovered in the United States, by Dr. Eli Ives, Professor of Materia Medica and Botany, in the Medical Institution of Yale College.*

THIS small plant was observed in flower in July, 1816, by Mr. Horatio N. Fenn (now of Rochester, State of New-York) in company with Dr. Leavenworth. The plant and the seeds have been preserved by me, in a flower-pot, from that time to the present. The plant was taken a few rods south of Mr. Whitney's gun manufactory, on the margin of the river, where it was covered by every tide. I have since observed the plant in great abundance on the margin of the Housatonuck, in Derby, and in those small streams in East Haven, Branford, and Guilford, which empty into Long-Island Sound.

A specimen of the limosella (with some specimens of the tillea) was sent to Z. Collins, Esq. of Philadelphia, who wrote me that Mr. Nuttall had found the same plant, a few days previous to the receipt of my letter, and that they had no question on the subject of the generic character, but that it would probably prove to be a new species.

In the transactions of the Medico-Physical Society of New-York, page 440, it is described under the name of limosella subulata. A description of the plant was published about the same time, by Mr. Nuttall, in the Journal of the Academy of Natural Sciences of Philadelphia. (See Vol. I. No. 6. p. 115.)

In the paper written by Mr. Nuttall is the following query: "Does this plant, with a lateral mode of growth and alternate leaves, germinate with two cotyledons?" The following observations were made in answer to this question. In the winter of 1816-17 this plant was kept in a situation exposed to severe frost; yet whenever the weather became warm for two or three days, it became quite green, but for the last winter there was no appearance of life in the plant. In March 1818, the vessel in which the limosella had been preserved for two summers preceding, and in which were a great quantity of seeds, was exposed in a warm situation to the sun. There was no appearance of vegetation until the last of March, when were observed several cylindrical leaves, some of them evidently arose from bulbs which had formed the last summer, on account of the dryness of its situation, which frequently occurs when plants are removed from a moist to a dry situation. In other instances single cylindrical leaves arose from the earth, where no bulbs were to be found; these cylindrical leaves were thought to arise from seeds, which, if it was a fact, would prove that the plant vegetated with but one cotyledon. In a short time the vessel was crowded with the seeds of the limosella raised by the cotyledons. These were carefully observed, and in every instance, when the coat of the seed was cast off, two linear cotyledons were observed, soon a cylindrical leaf arose from the centre of the cotyledons, and when this leaf had grown to the length of half an inch, a leaf of a similar kind arose laterally to a line made by the first leaf and the cotyledons.

From the facts above stated, it is thought to be proved that the limosella vegetates with two cotyledons. This was the fact in every instance where the husk of the seeds was obviously attached to the cotyledons, and in the few instances where the plants appeared to vegetate with but one cotyledon, it is probable that it arose from a bulb or some portion of the old plant, in which life had not been extinguished, during the past winter, which was made more probable by the fact that several of the leaves arose obviously from bulbs. This

limosella,* with its congeners, hence will take its place in the natural order of Jussieu lysimachiæ.

ART. XV. *Professor* BIGELOW *on the comparative Forwardness of the Spring in different Parts of the United States, in* 1817.

WE have been favoured with an ingenious memoir on this subject, by the author, Professor Bigelow of Boston; it is a part of the fourth volume of the Memoirs of the American Academy of Arts and Sciences.

Professor Bigelow, availing himself of a hint given him some years ago by the late venerable Dr. Muhlenberg of Pennsylvania, ascertained, through the medium of correspondence with accurate observers in different parts of North America, the time of flowering, for "1817, of the common fruit-trees and a few other plants"—"found in most parts of the United States."

The peach-tree was the one most uniformly returned, and the following table exhibits the time of its flowering, in places sufficiently numerous and remote, to afford a fair specimen of these observations:

Places.	*Lat.*	*Long.*	*Peach-tree in blossom.*
Fort Claiborne, Alab. Ter.	31° 50′	87° 50′	March 4
Charleston, S. C.	32 44	80 39	. . . 6 12
Richmond, Va.	37 40	77 50	. . . 23 Ap. 6
Lexington, Ky.	38 6	85 8	April 6 15
Baltimore, Md.	39 21	77 48	. . . 9
Philadelphia, P.	39 56	75 8	. . . 15
New-York, N. Y.	40 42	74 9	. . . 21 26
Boston, Mass.	42 23	70 52	May 9
Albany, N. Y.	43 39	73 30	. . · 12
Brunswick, Me.	43 53	69 55	. . · 15 †
Montreal, Can.	45 35	73 11	. . . 12

* In the Journal of the Academy of Natural Sciences of Philadelphia this plant is called limosella tenuifolia.

† No return of this tree was made from Brunswick. The date of the cherry-tree is therefore substituted, which is usually in blossom at the same time.

Professor Bigelow infers, "that the difference of season between the northern and southern extremities of the country is not less than two months and a half." "Difference of longitude does not seem very materially to affect the Floral Calendar within the United States." It appears, that in the same year peach-trees were in blossom at Valencia, in Spain, about the 19th of March; the apple-tree near London, May 8th; the cherry-tree and pear-tree at Geneva, in Switzerland, April 3d.

We hope that this research will be prosecuted in the manner it has thus been happily begun. It evidently affords an excellent criterion of the actual temperature, on a scale more extensive than it is practicable to obtain from thermometrical registers.

Floral Calendars kept in various parts of the United States would afford very interesting information, as to the changes of climate in particular places; a common topic of popular remark, but generally with few and inaccurate data.

ART. XVI. *A Journal of the Progress of Vegetation near Philadelpha, between the 20th of Ferbuary and the 20th of May*, 1816, *with occasional Zoological Remarks.* By C. S. RAFINESQUE.

THE importance of observations on the annual progress of vegetation is obvious, and, as connected with agriculture, gardening, &c., eminently useful. Comparative observations acquire a particular degree of interest, when made by skilful observers, at the same time, but at different places. Dr. Bigelow, of Boston, issued a circular, proposing that such contemporaneous observations should be made in the spring of 1817; and I wish that his request may have been attended to, when the collection of those observations may afford valuable materials for an American Calendar of Flora. The blossoming

of plants is easily watched, but their foliation and budding ought not to be neglected. Having been prevented, by various causes, from keeping an exact record of the progress of vegetation near New-York in 1817, I submit an accurate journal which I had kept the year before, at Philadelphia, in which I hope that some interesting facts may be noticed. Dr. Benjamin Barton has published a sketch of a Calendar of Flora for Philadelphia, in his Fragments on the Natural History of Pennsylvania; by comparing it with mine, many material differences may be traced, which evince a gradual change of temperature, although the spring of 1816 was remarkably cold and late. The greater quantity of species observed by me may, besides, render this journal a sort of vernal Flora of the neighbourhood of Philadelphia; and many species found by me are not to be met in the *Flora Philadelphica* of Dr. William Barton.

February 20. The *Hyacinthus orientalis* begins to show its flowers, and on the

24. In full blossom, as well as *Convallaria majalis*, in rooms.
25. The grass begins to look greenish in some parts.
26. Seen the first larva of insect in a pond.
27. The *Motacilla sialis*, or bluebird, is heard for the first time.
28. The first shad (*Clupea sapidissima*) is taken in the Delaware, while on the same day, the first smelt (*Salmo eperlanoides*) was taken in the Raritan, at New-Brunswick.

March 1. The *Tulipa gesneriana*, and *Hesperis matronalis*, are in blossom at the windows: the suckers (*genus Catostomus*) appear in the fish-market.

2. The catkins of the *Alnus serrulatus* begin to swell.
3. Those of *Salix Caprea* begin to appear.
4. The grass looks green by patches in the country.
5. The leaves of *Veronica officinalis*, *Plantago virginiana*, *Saxifraga virginica*, &c. are quite unfolded.
6. The new leaves of *Kalmia latifolia* begin to appear.
7. The spathas of *Spathyema fetida*, or *Fothos fetida*, begin to appear in blossom.

8. The *Alnus serrulatus* is in full blossom.
10. Found several mosses and ferns in blossom; these last were covered with capsules or old fructification: they were *Asplenium ebeneum*, *Aspidium marginale*, *Asp. acrostichoides*, *Polypodium medium*, N. Sp., &c.
11. Seen the first spider, in the country, brown, oblong, walking. A fall of snow at night.
12. Seen in blossom, at the windows, *Narcissus tazzetta*, *N. jonquilla*, and several saffrons, genus *Crocus*, &c.
14. The grass looks quite green; the *Draba verna?* is in blossom in the State-House garden, the *Viburnum tinus*, *Primula acaulis*, &c. in the rooms, &c. The following fish are at market: white perch, (*Perca mucronata*, Raf.) yellow perch, (*Polyprion fasciatum*, Raf.) mamoose sturgeon, (*Accipenser marginatur*, Raf.) elk-oldwives, (*Sparus crythrops*, Raf.) &c.
15. The *Populus fastigiata*, Lombardy poplar, begins to show its catkins.
17. The big-eye herring (*Clupea megalops*) begin to be seen at the fish-market.
18. Many plants begin to grow and show their leaves.
19. A fall of snow. The first shad (*Clupea sapidissima*) appear in New-York: they are now common here.
20. *Crocus aureus* in blossom in gardens; likewise *Iris persica*, &c.
21. *Betula lenta* begin to show the catkins.
22. *Galanthus nivalis*, and *Lamium amplexicaule*, are in blossom in gardens at Cambden.
24. *Populus fastigiata*, and *Salix caprea*, are in full bloom.—The gooseberry bushes shoot their leaves.
25. *Populus angulata* in blossom at Cambden.
26. *Salix babylonica* begins to blossom and shoot the leaves. *Viburnum prunifolium* is budding.
27. *Draba verna?* is in seed already in Cambden: the *Rhododendron maximum* begins to shoot in gardens.
28. *Juniperus virginiana* is in bloom. *Saxifraga virginica* begins to show its flowers. *Laurus benzoin*, and *Cornus florida*, are budding.

April 1. In the morning, a large flight of wild geese went over the city northwards, making a great noise. In the afternoon there was a thunder storm from the southwest.

2. The frogs begin to croak. Found in blossom near Cambden, *Arabis rotundifolia*, Raf., *A. lyrata*, *Saxifraga virginica*, *Draba verna? Betula lenta*, &c. *Pinus inops* is budding.

3. Seen the first swallow. Found in blossom on the Schuylkill, *Fumaria cucullaria*, *Anemone thalictroides*, *Saxifraga virginica*, many ferns and mosses.

4. The fresh-water turtle (*Testudo picta*) begins to show itself.

7. Found in blossom to-day, *Hepatica triloba*, *Laurus benzoin*, *Sanguinaria canadensis*, *Spathyema fetida*, *Acer rubrum*, &c. The first bee is seen.

10. In blossom at the woodlands, *Viola blanda*, *Luzula filamentosa*, Raf., *Gnaphalium? plantageneum*, &c.

12. In blossom at Cambden, *Viola lanceolata*, and *Houstonia cerulea*.

14. The apricot-trees begin to blossom in gardens. *Acernegundo* is in bloom at Gray's Ferry.

15. Seen the first butterfly—it was small and gray. Found in blossom, near Cambden, *Phlox subulata*, *Arabis parviflora*, Raf, and *Vaccinium ligustrinum*.

18. Seen in blossom, *Epigea repens*, *Carex acuta*, and *Taraxacum dens-leonis*. In gardens, the peach and cherry trees are in bloom. Observed many insects. The *Camellia*, the *Magnolia chinensis*, &c. are seen in the hot-house of the Woodlands.

20. The first snake is seen, *Coluber trivittata*, Raf. Also a beautiful large butterfly, red and black. The *Salix vitellina*, and *Capsella bursa*. (*Thlaspi bursa-pastoris*,) are in blossom.

21. Found in blossom, near Gray's Ferry, *Narcissus pseudonarcissus*, and *Sedum ternatum*, both naturalized. Likewise the *Populus tremuloides*, and *Mespelus canadensis*. The leaves of *Podophyllum pettatum* are fully expanded.

23. Seen in full bloom in gardens, the pear-tree, plum-tree, *Riber grossularia*, and *R. rubrum.*
24. Found in blossom along the Schuylkill, *Aguilegia canadensis, Hyacinthus botryoides, Ranunculus fascicularis, Violapapilionacea. V. decumbens*, Raf., *Houstonia cerulea, Cerastium pumilum*, Raf.
25. Found in blossom near Cambden, *Viola pedata, V. lanceolata, V. ovata*, Raf., *V. primulifolia, Arabis parviflora*, Raf., *Cerastium pumilum*, Raf., *Carex acuta, Meopilus botryapium, Laurus sassafras, Cercis canadensis, Potentilla simplex, Andromeda racemoca.*
28. Seen in blossom in gardens, *Calycanthus floridus, Syringa persica, Phlox pilosa*, &c. The leaves of *Liriodendron tulipifera, Æsculus hippocastanum, Populus fastigiata, P. angulata*, are unfolded.
30. In blossom on the Schuylkill, *Obolaria virginiana, Anemone trifolia, Hydrastis canadensis*, &c.

May 1. In blossom in the Neck, *Cerastium vulgatum? Veronica serpyllifolia, V. arvensis, Ranunculus bulbosus, Viola cucultata.*
3. Found above the Falls of the Schuylkill, *Viola striata, V. concolor, V. primulifolia, V. blanda, Fumaria aurea, F. cucullaria, Charophyllum procumbens, Uvularia sessitifolia, U. perfoliata, Cercis canadensis, Arabis falcata, Stellaria pubera, Erigeron pulchellum, Orchis spectabilis, Hydrastis canadensis, Dentaria diphylla, Azalea nudiflora*, &c.
4. Found on the Vissahikon, *Arabis bulbosa, Panax trifolium, Viola pectata, V. rotundifolia, Cardamine pennsylvanica, Krigia virginica*, and several grasses.
7. Found in blossom over the Schuylkill, *Laurus sassafras, Viburnum prunifolium, Aronia arbutifolia, A. melanocarpa, Fragaria virginica, Cerastium nutans*, Raf., *Convallaria majalis*, naturalized, and several species of the genus *Vaccinium.*
10. Found below the falls of the Schuylkill, *Floerkea uliginosa, Viburnum acerifolium, Oxalis violacea, Cerastium tenui-*

folium, *Glechoma hederacea*, &c.: and the following above the Falls—*Trillium cernuum*, *Viola pubescens*, *V. pennsylvanica*, *Hydrophyllum virginicum*, *Polemonium reptans*, *Senecio aureus*, *Saxifraga pennsylvanica*, *Staphylea trifoliata*, *Obolaria virginica*, *Caltha palustris*, *Ranunculus abortivus*, &c.

11. Seen the first bat.

12. Near Haddonfield, *Bartsia coccinea*, *Helonias bullata*, *Trifolium repens*, &c.

15. Found between Cambden and Haddonfield, *Trifolium pratense*, *Silene virginica*, *Antirrhinum canadense*, *Lithospermum tenellum*, Raf., *Festucatenella*, *Seleranthus annuus*, *Oxalis biflora*, Raf., *Poa rubra*, *Vaccinium corymbosum*, *Viola palmata*, *V. parvifolia*, Raf., *Rubus flagellaris*, &c. Also in blossom, *Quercus rubra*, *Q. obtusiloba*, *Q. alba*, &c.

20. Found near Burlington, *Plantago virginica*, *Euphorbia ipecacuanha*, *Comptonia asplenifolia*, *Myosotis lappula*, *Senecio obovatus*, *Scirpus acicularis*, *Lithospermum trinervum*, Raf., *L. tenellum*, Raf., &c.; besides several *Carex*.

ART. XVII. *Description of a New Species of North American Marten, (Mustela vulpina)* by C. S. RAFINESQUE.

THE regions watered by the Missouri are inhabited by many animals, as yet unknown to the zoologists, although many have been noticed by travellers. A species of marten has lately been presented to the Lyceum of Natural History in New-York, which was brought from that country, and appears to belong to a peculiar species, very different from the common martens of Europe, Asia, and America, although it has, in common with it, the character of the yellow throat; but the head, feet, and tail, afford so many peculiar characters, that no doubt can be entertained of its diversity. I have, therefore, given

to it the name of *Mustela vulpina*, or Fox Marten, owing to its head and tail being somewhat similar to that of a fox.

Mustela Vulpina. Definition—Brown, three large yellowish spots underneath on the throat, breast, and belly; cheeks, inside of the ears, and a spot on the nape, white; tail tipped with white one-third of total length; feet blackish, toes white.

Description.—This animal is of a fine shape: its size is rather above mediocrity, being about half a foot high, and the total length being twenty-seven inches, whereof nine form the tail. The general colour of the fur is of a drab brown, and it is neither coarse nor very fine. The head is elongated, oblong, about four inches long, shaped like that of a fox; the snout is narrow; the nose is black, notched, and granulated, furnished on each side with black whiskers, two inches long. there are three long black hairs, or *vibrissa*, above each eye, and a few shorter ones scattered behind them on the cheeks, chin, and tip of the lower jaw, which is white: the cheeks are whitish, and there is a white spot on the nape of the neck: the ears are large, broad, and white inside. There are three large, oblong spots, on the throat, breast, and belly; this last is the largest; that on the breast the smallest. The fore legs are shorter than the hind ones, and have, behind, three very long hairs or vibrissa: the feet and toes of all the legs are covered with long fur; the former have a dark brown or blackish ring, and the latter are of a dirty white: there are five long toes to all the feet, of which the inner one is the shortest; the nails are white, retractible, and shorter than the fur. The teeth are as in the genus *Mustela*, and white; those of the lower jaw are larger and stronger: the grinders are four on each side; they are broad, trifid, with the middle lobe sharp and very long: the tusks, or dogteeth, are very strong, curved, and approximated, leaving a very small place for the incisores, which are very small, very short, and flat; the two lateral ones on each side are situated diagonally, the second behind, and the two middle ones are only half the size of the others. The tail is bushy, particularly at the top, where there is a white pencil of long hairs; the brown of the remainder is darker than on the body.

From the above accurate description, it will appear evident that this animal is very different from the common marten of North America. It must be a ferocious little animal, and very fierce; which is indicated by the strength of the teeth.

ART. XVIII. *Natural History of the Scytalus Cupreus, or Copper-head Snake.* By C. S. RAFINESQUE.

AFTER the rattlesnake, the copper-head snake is the most dreaded in the northern states, being the next largest venomous snake: he is also more common in the cold parts, where the former is very rare. Strange as it may seem, this conspicuous and dangerous animal has escaped the notice of naturalists, and is not found described in Shaw nor Lacepede. Having seen two of them near Fishkill, in the summer of 1817, I endeavoured to describe them completely, and investigate their history. They were both killed in a meadow, and one of them while sleeping coiled up near a fence; a slight stroke of a rod was sufficient, as usual with venomous snakes. It appears that they are killed much easier than the innocent snakes; these are often seen to revive after an apparent death, and do not really die until the next sunset; while venomous snakes do not easily revive, particularly if the head is slightly bruised.

This snake is known by a variety of names in different parts of the State of New-York, since he has every where attracted the attention of the inhabitants: these names are, *copper-head, copper-snake, chunk-head, copper-adder, copper-viper, copper-belly, pilot-snake, deaf-adder, deaf-snake;* and in New-England, by the names *rattlesnake's mate* and *red adder*, &c. They have all been given in reference to his colour, or to some presumed peculiarities in his manners, &c. *Chunk-head* is a vulgar expression, meaning thick-head or blunt-head. He has been called sometimes *pilot-snake*, on a false supposition that he was the pilot or guide of the rattlesnake; and he has been

considered as deaf, because he is easily surprised, and does not appear to hear the noise of your approach.

It belongs to the genus *scytalus* of Daudin, &c., which differs from the *Boa* of Linnæus, as the genus *Vipera* does from *Coluber*, being provided with fangs. I have given to it the name of *Scytalus Cupreus*, which means coppered scytalus. The following definition of the species may be considered as comparative and characteristic.

Scytalus Cupreus. Tail one-eighth of total length, with 45 caudal plates entirely brown ; 150 abdominal plates, the last very broad ; head oval, coppered above, yellow underneath ; scales carinated on the back, which is coppered, with reddish brown rings cross-shaped ; belly variegated of brownish.

Description. Total length about three feet ; body thicker than in the innocent snakes. Head large, broad, oval, obtuse, very distinct from the neck, nearly two inches long, flattened, coppered brown above, and covered with large, smooth scales ; yellow underneath, as well as the neck, and with rhomboidal smooth scales. Mouth very large ; fangs yellowish white. Back flattened anteriorly, a little angular in the middle, covered with small rhomboidal, obtuse, keeled scales ; those of the sides larger and smooth, not keeled ; centre of the back of a brownish copper colour ; sides of a bright copper ; broad bands or rings, becoming forked on each side, and assuming nearly the shape of a St. Andrew's cross ; they are of a reddish brown : there is a round spot opposite to the sinusses, and the scales of the sides are minutely dotted of brown. The abdominal plates are 150, beginning under the head ; the last, covering the vent, is very broad, double the other : they are of a shining, pale copper colour, with two longitudinal and lateral rows of great, irregular, brown spots, with some light brownish clouds between them, and each plate is marginated of whitish. The belly is very flat and broad, about $1\frac{1}{4}$ inch in diameter ; and the skin may be distended on the sides, when the animal is not fed. Tail short, tapering gradually, about four inches long, cylindrical, brown, without spots, with 45 plates underneath, and having at the end a small, obtuse, horn

claw, of an oblong, compressed, obtuse shape, and carinated underneath.

This snake has many of the habits of the rattlesnake ; he is very slow in his motions, rather clumsy, owing to his thick shape and short tail. He retires in winter into caves, hollow rocks, and trees, where he lies, in a torpid state, from November to April ; several have been found coiled up together, the head lying over the back : it is in the same situation he sleeps in the fields. When found in the torpid state, they may be carried without waking ; but might wake in a warm room. They do not eat during all that time : their food consists of birds, frogs, mice, and even squirrels, which they catch by surprise, as they do not climb on trees. They kill their large prey by breathing a poisonous effluvia, crushing it in their folds, and they swallow it whole after covering it with their clammy saliva. They can remain a very long time without a meal, and one meal is a long time digesting.

They are generally found in meadows, pastures, and the edge of woods. They creep slovenly through the grass, and if surprised by the sight of man, they assume an erect and threatening posture, darting their tongue and swelling their head ; but they do not attack men, unless alarmed and struck. They are considered more dangerous than the rattlesnake, because they do not give notice of their vicinity, and lie concealed in the grass ; but they are easily killed, when assuming the threatening posture, by a slight touch of a cane, spade, or any other instrument. The effects of their bite is similar to that of the rattlesnake, and cured in the same way, by the prompt application of the *Aristolochia serpentaria, Polygala senega, Prenanthes serpentaria, Macrotry serpentaria,* &c. and other plants, bearing in consequence the name of snakeroots.

This snake is found in New-England, New-York, New-Jersey, Pennsylvania, &c., and perhaps all over the United States.

Art. XIX. *On a Method of Augmenting the Force of Gunpowder.*

Extract of a Letter to the Editor, from Colonel George Gibbs.

I EMPLOYED, the last year, a man in blowing rocks, and having seen an account of a method of substituting a portion of quick lime for a part of the gunpowder usually employed, I was induced to make a number of experiments upon it. I now send you the results in the certificate of the person employed, whose statement might be relied on, even if I had not superintended myself a number of the experiments.

"*Sunswick Farms, Oct.* 19, 1817.—I certify that, having been employed by Colonel Gibbs in blasting rocks on his farm, I, by his orders, made use of a composition of one part quick lime and two parts gunpowder, and uniformly found the same charge to answer equally well with a like quantity of gunpowder. I made upwards of fifty blasts in this manner, as well as several hundreds in the usual way, and can therefore depend upon the accuracy of this statement. I found, however, that when the powdered lime was mixed with the gunpowder the day before, that the effect was diminished. It should be always used the day it is mixed.

(Signed) T. Pomeroy."

This preparation was made generally in the morning, put in a bottle and well corked, to prevent the access of the external air. The rationale of the process was not explained in the original recommendation, but it soon occurred to me, that it must be owing to the desiccation of the gunpowder by the lime.

The attraction of moisture by gunpowder, is known to be very great: according to Rees's Cyclopedia, upwards of 16 per cent. has been absorbed, and that the removal, simply,

from near the fire to the corner of the room, produces a considerable change in its weight. I presume, therefore, that the lime, which in its caustic state has also a great affinity to water, attracts a portion of it from the powder, and leaves it in a state of dryness best fitted for inflammation. But if the lime should remain too long mixed with the powder, it would probably attack the water of crystallization of the saltpetre, and, according to Count Rumford's idea, destroy a great part of the power. If also left exposed, attractions of moisture would take place from the atmosphere, the gunpowder would remain surcharged with humidity as before, and the lime would be only an inert mass.

The examination of this subject led me to consider the increase of the power of gunpowder in various situations, and of its use in the field. It is well known that after a few discharges a cannon becomes heated, and the range is much greater, as well as the recoil. The charge of powder is therefore reduced about one quarter, to produce the original effect. As I have not heard or seen any explanation of this fact I shall take this opportunity of mentioning, that it appears to arise from the same cause as the first explained, viz. the desiccation of the powder. No person will dispute the heat acquired by a cannon, or even a musket, after repeated discharges; and this heat must volatilize or destroy a great portion of the moisture combined with the powder, assist its speedy inflammation, and perhaps add to its power, by causing a more perfect combustion of the inflammable parts of the gunpowder. This would cause a much greater volume of gas to be produced, and the high temperature would also greatly augment its elasticity; and it is well known that the effects of gunpowder depend upon the rapid production and high degree of elasticity of a great quantity of aeriform fluids or gases.

Art. XX. *On The Connexion between Magnetism and Light.* By Col. Gibbs.

Extract from a Letter to the Editor.

I VISITED, the last year, the mine of magnetic iron at Succassunny, belonging to Governor Dickerson of New-Jersey. The mine had not been worked for a year past, and I did not descend it. The proprietor, a gentleman of distinguished science, informed me of a singular circumstance attending it, which was too important to be left unnoticed. The mine is worked at the depth of 100 feet; direction of the bed, north-east and southwest; inclination nearly perpendicular. The ore in the upper part of the bed is magnetic, and has polarity; but that raised from the bottom has no magnetism at first, but acquires it after it has been some time exposed to the influence of the atmosphere. This fact, of which there is no doubt, struck me as most singular. I could not recollect any similar observation; and it is only lately that I have found that Werner had observed, that iron sand, raised from the depth of 100 feet, had no magnetism. See Rees's Cyclopedia, Art. Sand.

I could only account for this circumstance by supposing that magnetism existed not in the interior of the earth, as was supposed, but only on the surface, and in such bodies as received this principle from atmospheric, or celestial influence.

The late discovery of the magnetic influence of the violet rays of light, by M. Morechini, a notice of which has since reached us in the journals, connected with the above fact, leads me to believe that light is the great source of magnetism. A learned foreigner,* whose residence in this country has contributed much to its scientific improvement, has also informed me that other substances than metallic have been found, by compression, to be magnetic.

It is well known that the violet ray is the most refrangible,

* Mr. Correa de Serra, Minister of the King of Portugal.

or has the most attraction to matter. But there are other rays, which Herschel, who some years since discovered them, calls invisible rays, which are still more refrangible, are next beyond the violet, when refracted, and partake of most of its properties, except that they are invisible. I have not yet seen any account of the experiments of M. Morechini, other than the notice in the journal; but I trust I shall soon be able to determine whether those invisible rays do not possess the magnetic power as well as the violet; or, perhaps, possess it exclusively.

As the refraction of the atmosphere in the polar circles, is at least ten times greater than in the tropics, a greater quantity of the magnetic rays will there be separated and combined than elswhere; and of course arises excess of magnetism. Hence the direction of magnetic bodies towards the northern and southern extreme regions. The great absorption and emission of light in the polar regions, by the ice and snow, may cause the extraordinary illumination of that country during the absence of the sun, and the emission of the magnetic rays with electricity may, perhaps, give us the aurora borealis.

The coincidence of the diurnal variation of the compass with the solar influence, deserves particular notice, and will have considerable weight on this subject.

That there are many facts which cannot readily be explained by the theory of light, I shall not deny; but in the infancy of this system we may be allowed to hope that future observations may enable us to remove present difficulties. One thing must be admitted, that no theory has heretofore been published relating to magnetism, which has received or seems entitled to much confidence. In your next number I hope to be able to furnish you with further remarks on this subject; but, I have no doubt that philosophy will finally determine that we owe to the solar ray light, heat, electricity, and magnetism.

G. GIBBS.

Sunswick, January, 1818.

ART. XXI. *On a new Means of producing Heat and Light, with an Engraving, by J. L. Sullivan, Esq. of Boston.*

BOSTON, May 7, 1818.

To Professor Silliman.

SIR,

IF the following account of a method of using tar and steam as fuel, recently invented by Mr. Samuel Morey, should be found sufficiently interesting to occupy a place in the Journal of Science, I am sensible its usefulness will be much extended through that medium of information.

The inventor, not unskilled in chemistry, and aware of the attraction of oxygen for carbon, conceived it practicable to convert the constituents of water into fuel, by means of this affinity.

Whatever may be the fact, chemically considered, the operation, in various experiments, promises to afford a convenient mothod of applying to use several of the most combustible substances, not hitherto employed as fuel. By the process I shall briefly describe, *all carbonaceous fluids* may be conveniently burnt, and derive great force from their combination with the oxygen and hydrogen gases of water or steam, before or at the moment of ignition.

A tight vessel, cylindrically shaped, was first employed, containing rosin, connected with a small boiler by a pipe which entered near the bottom, and extended nearly its length, having small apertures, over which were two inverted gutters, inclining or sloping upwards over each other; the upper one longer than the other, intended to detain the steam in the rosin, in its way to the surface. The rosin being heated, *carburetted hydrogen gas* would issue from the outlet, or pipe, inserted near the top of the vessel, and being ignited, afforded a small blaze, about as large as that of a candle; but, when the steam was allowed to flow, this blaze would instantly shoot out many hundred times its former bulk, to the distance of two or three feet

It is presumed the steam was decomposed, and carburetted hydrogen and carbonic oxide, or carbonic acid, produced as the steam passed, very near the hot bottom of the vessel.

Another apparatus was constructed, consisting of two vessels, one within the other, having a cover common to both; the inner one to contain *tar*, (as a more convenient substance than rosin;) the outer vessel to contain water, which surrounds the other, and lies under its bottom; or, in other words, a vessel of tar set into a vessel of boiling water. The boiler has a lining of sheet copper, or tin, to promote the ebullition. The tar vessel being riveted to the cover, holes are made through its sides, near to the cover, to allow the steam to pass in, and act on its surface. The cover being secured on, a safety valve is provided for the steam vessel, and two cocks; one over the tar, the other over the water, are fixed contiguously; the first has a tube, or is elongated to reach nearly to the bottom of the tar, which ascends, and is driven out by the pressure of the steam on its surface. Both cocks conduct to a pipe, wherein is placed a large wire, or metallic rod, which about fills the tube, and is perforated obliquely, or zig zag, to increase the length of the passage, and to mingle the tar and steam more intimately. The gases, or vapours, issue from a small orifice at the end of the pipe; and, being ignited by a little fire, into which it is directed, an intense and voluminous blaze is produced, and continues as long as the materials remain unexhausted. A hot brick, instead of the fire, answers the same purpose.

This apparatus contained but about one quart of tar, (which must always be nicely strained,) and it lasted one and a half hour, and the flame was sufficient to fill a common fireplace, if not allowed to escape, by its violence, up the chimney. Its force will be according to the elasticity of the steam. I regret being unable, since, to make more exact and varied experiments, to demonstrate the economy of this fuel This point, however, and the chemical facts, will be the subject of a future communication. And probably a form of a stove may be devised, wherein it may be used for the purposes of warmth, light, and cooking; and another apparatus to light streets.

But this invention will be of more special use *as fuel for steam engines applied to navigation*—the purpose principally for which I have purchased the patent right.

This may be the subject of another communication.

ART. XXII. *On the Changes which have taken place in the Wells of Water situated in Columbia, South-Carolina, since the Earthquakes of* 1811–12. By EDWARD DARRELL SMITH, M. D., *Professor of Chemical and Experimental Philosophy and Mineralogy in the South-Carolina College.*

To Professor Silliman.

DEAR SIR,

IN answer to your inquiry respecting the changes in our wells, since the memorable period of the earthquakes, I would make the following observations :

These tremendous convulsions of nature commenced in December, 1811, and were continued, at intervals, until the latter end of the succeeding month of March, with different degrees of violence, in this and some of the adjacent states. In November, 1812, I visited this town, and then understood that the wells, which are generally very deep, had an abundance of water in them. This continued to be the case for about one year after ; and in the College well, in particular, which was a remarkably fine one, there were always about twelve feet of water, notwithstanding its daily consumption by more than two hundred persons. Shortly after this time, many of the wells in the town began to fail in their usual supply of water, although they were frequently cleaned out and occasionally deepened. Their state became worse every year, until, at length, about three years since, some of them proved to be entirely dry, and most of the others had their water turbid, and diminished to the depth of only two or three feet.

A little anterior to this period, what were called the dry years had commenced, and there were, comparatively, very scanty falls of rain until the last spring; since when there has been a very large quantity. To elucidate the subject more fully, it may not be amiss to give some topographical account of the town of Columbia. About a mile from the eastern bank of the Cogaree the town begins to be thickly built up, and at this distance the elevation of ground is supposed to be one hundred feet above the level of the river in its ordinary state. The hill is then tolerably level for the space of a mile or more in its western extent, and its soil is principally composed of a loose, porous sand, with which few, if any, stones are intermixed at any depth that has yet been penetrated. In attempting to account for the failure of the well-waters, it was supposed by some that the earthquakes had produced such changes in the loose texture of the soils, that the veins of water which used to supply the wells, had sunk beneath the level of these reservoirs; but on this head it is to be observed, that there was no remarkable failure of water for one or two years after these changes were supposed to have been effected. Others again, connecting the greatest failure of water with the concurring dearth of rain, conceived that the fact might be explained by the droughts occasioning a deficiency in the river-water, and thus cutting off the supply which they supposed had heretofore peccolated from the margin of the river into the wells. If their hypothesis was correct, it was believed that the difficulty would be removed, either by deepening the wells, or by subsequent large supplies of rain. Many wells were immediately deepened from two to eight or ten feet, but the remedy proved very inadequate. And since the great falls of rain, within a year past, although there are somewhat larger supplies of water in some wells, yet there is not the half as much as existed before the earthquakes. The College well, although deepened several feet, does not now contain generally more than four or five feet of water. I must not omit to remark, that two wells, situated in a longitudinal line from north to south, with regard to each other, and also in a lower spot of ground, never failed entirely, although they diminished

considerably, and now yield more copious supplies than any others.

Whatever may be the cause of this phenomenon, the effects are so inconvenient, and it is so generally believed that they are likely to be permanent, that the inhabitants of the town are beginning to build cisterns, in order to accumulate artificial reservoirs of water.

Art. XXIII. *Respiration of Oxygen Gas.*

It is not extraordinary, when oxygen gas was first discovered, and found to be the principle of life to the whole animal creation, that extravagant expectations should have been formed as to its medicinal application. Disappointment followed of course, and naturally led to a neglect of the subject; and, in fact, for some years, pneumatic medicine has gone into discredit, and public opinion has vibrated to the extreme of incredulity. Partaking in a degree in this feeling, we listened with some reluctance to a very pressing application on this subject during the last summer. A young lady, apparently in the last stages of decline, and supposed to be affected with hydrothorax, was pronounced beyond the reach of ordinary medical aid. As she was in a remote town in Connecticut, where no facilities existed towards the attainment of the object, we felt no confidence that, even if oxygen gas were possessed of any efficacy in such cases, it would *actually* be applied in this case, in such a manner as to do any good. Yielding, however, to the anxious wishes of friends, we furnished drawings for such an apparatus as might be presumed attainable, and also written and minute directions for preparing, trying, and administering the gas. It was obtained from nitrate of potash, (saltpetre,) not because it was the best process, but because the substance could be obtained in the place, and because a common fire would serve for its extrication. The gas obtained had, of course, a variable mixture of nitrogen or azot, and probably on an average, might not be purer than nearly the

reversed proportions of the atmosphere—that is, 70 to 80 per cent. of oxygen to 20 or 30 nitrogen; and it is worthy of observation, whether this circumstance might not have influenced the result.

Contrary to our expectations, the gas (as we are since informed by good authority) was skilfully prepared and perseveringly used. From the first, the difficulty of breathing and other oppressive affections were relieved: the young lady grew rapidly better, and in a few weeks entirely recovered her health. A respectable physician, conversant with the case, states, in a letter now before us, "that the inhaling of the oxygen gas relieved the difficulty of breathing, increased the operation of diuretics, *and has effected her cure.* Whether her disease was hydrothorax, or an anasarcous affection of the lungs, is a matter I believe not settled."

Should the revival of the experiments on the respiration of oxygen gas appear to be desired, it would not be difficult to simplify the apparatus and operations so as to bring them within the reach of an intelligent person, even although ignorant of chemistry: and this task, should there be occasion, we would cheerfully undertake to perform.

This interesting class of experiments ought to be resumed, not with the spirit of quackery, or of extravagant expectation, but with the sobriety of philosophical research; and it is more than probable that the nitrous oxyde which is now little more than a subject of merriment and wonder, if properly diluted and discreetly applied, would be productive of valuable effects.

ART. XXIV. *On the Compound Blowpipe. Extract from the Journal de Physique, of Paris, for January* 1818.*

CONCERNING HEAT.

"HEAT, considered as one of the most important agents, especially in relation to chemistry, and even to mineralogy, has also been the subject of numerous labours, both with regard to the means of augmenting and of diminishing its effects.

"To the former belong the numerous experiments made, especially in England, with the blowpipe, supplied by a mixture of oxygen and hydrogen gases. Mr. Clarke has evidently been more extensively engaged in these researches than any other person, as our readers have perceived in the extracts which we have given from the labours of this learned chemist; but it is proper also to give publicity to the protest (réclamation) made to us in favour of Mr. Silliman.

"We have already stated that Mr. Hare, of Philadelphia, first conceived the idea of forming a blowpipe with explosive gas; but as we have not been conversant with the memoirs of the Society of Arts and Sciences of Connecticut, we have not made mention of Mr. Silliman.

"The fact is, that this chemist, Professor at New-Haven, published, on the 7th of May,† 1812, a memoir containing the results of experiments made upon a very great number of bodies, until that time reputed to be infusible; and, among others, upon the alkaline earths, the decomposition of which he effected.

"The experiments of Mr. Clarke were therefore subsequent; but, having been made upon a still more extensive list of substances, they are scarcely less interesting.

"It results then, from the experiments of Messrs. Hare, Silliman, Clarke, Murray, and Ridolfi, that there is really no substance which is infusible in the degree of heat produced by this kind of blowpipe.

"In this new department of physics, it is attempted not only to apply the blowpipe to a very great number of bodies, but

* Communicated by a friend at Paris.

† See Transactions of the Connecticut Academy, and Bruce's Journal, Vol. I. p. 199.

so to modify the instrument or apparatus as to give it the highest degree of convenience, and especially to obviate the danger of explosion." pp. 38 & 39.

REMARKS.

As the results produced by Mr. Hare's Compound Blowpipe, fed by oxygen and hydrogen gases, continue to be mentioned in Europe, in many of the Journals, without any reference to the results long since obtained in this country, we republish the following statement of facts, which was, in substance, first published in New-York, more than a year since. It should be observed, that Mr. Tilloch has since published, in the Philosophical Magazine in London, the memoir which contained the American results, and there have been some other allusions to it in different European Journals, and to Mr. Hare's previous experiments; but still this interesting class of results continue to be attributed to others than their original discoverers.

Yale College, April 7, 1817.

Various notices, more or less complete, chiefly copied from English newspapers, are now going the round of the public prints in this country, stating that "*a new kind of fire*" has been discovered in England, or, at least, new and heretofore unparalleled means of exciting heat, by which the gems, and all the most refractory substances in nature, are immediately melted, and even in various instances dissipated in vapour, or decomposed into their elements. The first glance at these statements, (which, as regards the effects, I have no doubt are substantially true,) was sufficient to satisfy me, that the basis of these discoveries was laid by an American discovery, made by Mr. Robert Hare of Philadelphia, in 1801. In December of that year, Mr. Hare communicated to the Chemical Society of Philadelphia his discovery of a method of burning oxygen and hydrogen gases in a united stream, so as to produce a very intense heat.

In 1802, he published a detailed memoir on the subject, with an engraving of his apparatus, and he recited the effects of his instrument; some of which, in the degree of heat produced, surpassed any thing before known.

In 1802, and 1803, I was occupied with him, in Philadelphia, in prosecuting similar experiments on a more extended scale ; and a communication on the subject was made to the Philosophical Society of Philadelphia. The memoir is printed in their transactions ; and Mr. Hare's original memoir was reprinted in the Annals of Chemistry, in Paris, and in the Philosophical Magazine, in London.

Mr. Murray, in his System of Chemistry, has mentioned Mr. Hare's results in the fusion of several of the earths, &c. and has given him credit for his discovery.

In one instance, while in Europe, in 1806, at a public lecture, I saw some of them exhibited by a celebrated Professor, who mentioned Mr. Hare as the reputed author of the invention.

In December, 1811, I instituted an extended course of experiments with Mr. Hare's blowpipe, in which I melted lime and magnesia, and a long list of the most refractory minerals, gems, and others, the greater part of which had never been melted before, and I supposed that I had decomposed lime, barytes, strontites, and magnesia, evolving their metallic basis, which burnt in the air as fast as produced. I communicated a detailed account of my experiments to the Connecticut Academy of Arts and Sciences, who published it in their Transactions for 1812 ; with their leave it was communicated to Dr Bruce's Mineralogical Journal, and it was printed in the 4th number of that work. Hundreds of my pupils can testify that Mr. Hare's splendid experiments, and many others performed with his blowpipe, fed by oxygen and hydrogen gases, have been for years past annually exhibited, in my public courses of chemistry in Yale College, and that the fusion and volatilization of platina, and the combustion of that metal, and of gold and silver, and of many other metals ; that the fusion of the earths, of rock crystal, of gun flint, of the corundum gems, and many other, very refractory substances ; and the production of light beyond the brightness of the sun, have been familiar experiments in my laboratory. I have uniformly given Mr. Hare the full credit of the invention, although my researches, with his instrument, had been

pushed farther than his own, and a good many new results added.

It is therefore with no small surprise that, in the Annales de Chimie et de Physique, for September, 1816, I found a translation of a very elaborate memoir, from a Scientific Journal, published at the Royal Institution in London, in which a full account is given of a very interesting series of experiments performed by means of Mr. Hare's instrument; or rather one somewhat differently arranged, but depending on the same principle. Mr. Hare's invention is slightly mentioned in a note, but no mention is made of his experiments, or of mine.

On a comparison of the memoir in question with Mr. Hare's and with my own, I find that very many of the results are identical, and all the new ones are derived directly from Mr. Hare's invention, with the following differences.—In Mr. Hare's, the two gases were in distinct reservoirs, to prevent explosion; they were propelled by the pressure of a column of water, and were made to mingle, just before their exit, at a common orifice. In the English apparatus, the gases are both in one reservoir, and they are propelled by their own elasticity, after condensation, by a syringe.

Professor Clarke, of Cambridge University, the celebrated traveller, is the author of the memoir in question; and we must presume that he was ignorant of what had been done by Mr. Hare and myself, or he would candidly have adverted to the facts.

It is proper that the public should know that Mr. Hare was the author of the invention, by means of which, in Europe, they are now performing the most brilliant and beautiful experiments; and that there are very few of these results hitherto obtained there, by the use of it, (and the publication of which has there excited great interest,) which were not, several years ago, anticipated here, either by Mr. Hare or by myself.

As I have cited only printed documents, or the testimony of living witnesses, I trust the public will not consider this communication as indelicate, or arrogant, but simply a mat-

ter of justice to the interests of American science, and particularly to Mr. Hare.

BENJAMIN SILLIMAN,

Professor of Chemistry and Mineralogy in Yale College.

ART. XXV. *The Northwest Passage, the North Pole, and the Greenland Ice.*

IN looking over the foreign journals, we find no articles of intelligence so interesting as those which respect the three subjects mentioned above. Indeed, as they have found their way into most of our newspapers, it is now generally known in this country, that, in consequence of the reported breaking up of the Greenland ice, an expedition has already left England, in two divisions, the one for the purpose of exploring a northwest passage to Asia, around the North American continent, by the way of Davis's Straits; the other, for effecting the same object *by passing over the north pole.*

If Horace thought that man almost impiously daring who first adventured upon the open sea, what shall we say of the hardihood of the attempt to visit THE POLE?—the pole, which it is impossible to contemplate without awe—which, in all probability, has never been visited by any living being—where the dreary solitude has never been broken by human voice—where the sound of war has never been heard, and darkness and cold exert an almost undisputed dominion! What must be the emotions of that man who first stands upon the point of the earth's axis! Who, no longer partaking of the revolution, in circles of latitude, slowly revolves on the axis of his own body, once in twenty-four hours—to whom the sun does not rise or set, but, moving in a course very oblique to the horizon, makes scarcely a perceptible progress in twenty-four hours, and at the end of three months, when he has attained his noon, is only 23° 28′, on the arc of a vertical circle, above the horizon—to whom longitude is extinct, and who can move in no possible direction but south—to whom the stars are a blank, and to

whom the polar star, could he see it, would appear in the zenith. Such are some of the most obvious results of a position on the pole. The man who first establishes himself on this sublime point, will have more reason for self-congratulation than he who led the Persian myriads into Greece, or he who pushed the Macedonians to the Indus.

On these interesting subjects, we beg leave to refer our readers to a very able treatise in the Quarterly Review for February, 1818, where all the topics at the head of this article are discussed with much learning and ability.—We extract the following passage :

"If an open navigation should be discovered across the polar basin, the passage over the pole or close to it, will be one of the most interesting events to science that has ever occurred. It will be the first time that the problem was practically solved with which the learners of geography are sometimes puzzled—that of going the shortest way between two places lying east and west, by taking a direction of north and south. The passage of the pole will require the undivided attention of the navigator. On approaching this point, from which the northern coasts of Europe, Asia, and America, and every part of them, will bear *south* of him, nothing can possibly assist him in determining his course, and keeping on the right meridian of his destined place, but a correct knowledge of the *time:* and yet no means of ascertaining that time will be afforded him. The only *time* he can have, with any degree of certainty, as long as he remains on or near the pole, must be that of Greenwich, and this he can know only from good chronometers ; for, from the general hazy state of the atmosphere, and particularly about the horizon, and the sameness in the altitude of the sun at every hour in the four-and-twenty, he must not expect to obtain an approximation even of the apparent time, by observation, and he will have no stars to assist him. All his ideas respecting the heavens and the reckonings of his time will be reversed, and the change not gradual, as in proceeding from the east to the west, or the contrary, but instantaneous. The magnetic needle will point to its

unknown magnetic pole, or fly around from the point of the bowl in which it is suspended, and that which indicated north will now be south; the east will become the west, and the hour of noon will be that of midnight.

"These curious circumstances will probably be considered to mark the passage by the pole, as the most interesting of the two, while it will perhaps be found equally easy. We have, indeed, very little doubt, that if the polar basin should prove to be free from land about the pole, it will also be free of ice. A sea of more than two thousand miles in diameter, of unfathomable depth, (which is the case between Greenland and Spitzbergen,) and in constant motion, is not likely to be frozen over at any time. But if all endeavours to discover a passage to the Pacific by either route should prove unavailing, it will still be satisfactory to have removed every doubt on this subject by ascertaining the fact. In making the attempt, many objects interesting and important to science will present themselves to the observation of those who are engaged in the two expeditions. That which proceeds up Davis's Straits, will have an opportunity of adjusting the geography of the north-east coast of America, and the west coast of Greenland; and of ascertaining whether the latter be not an island or an archipelago of islands; and much curious information may be expected from both.

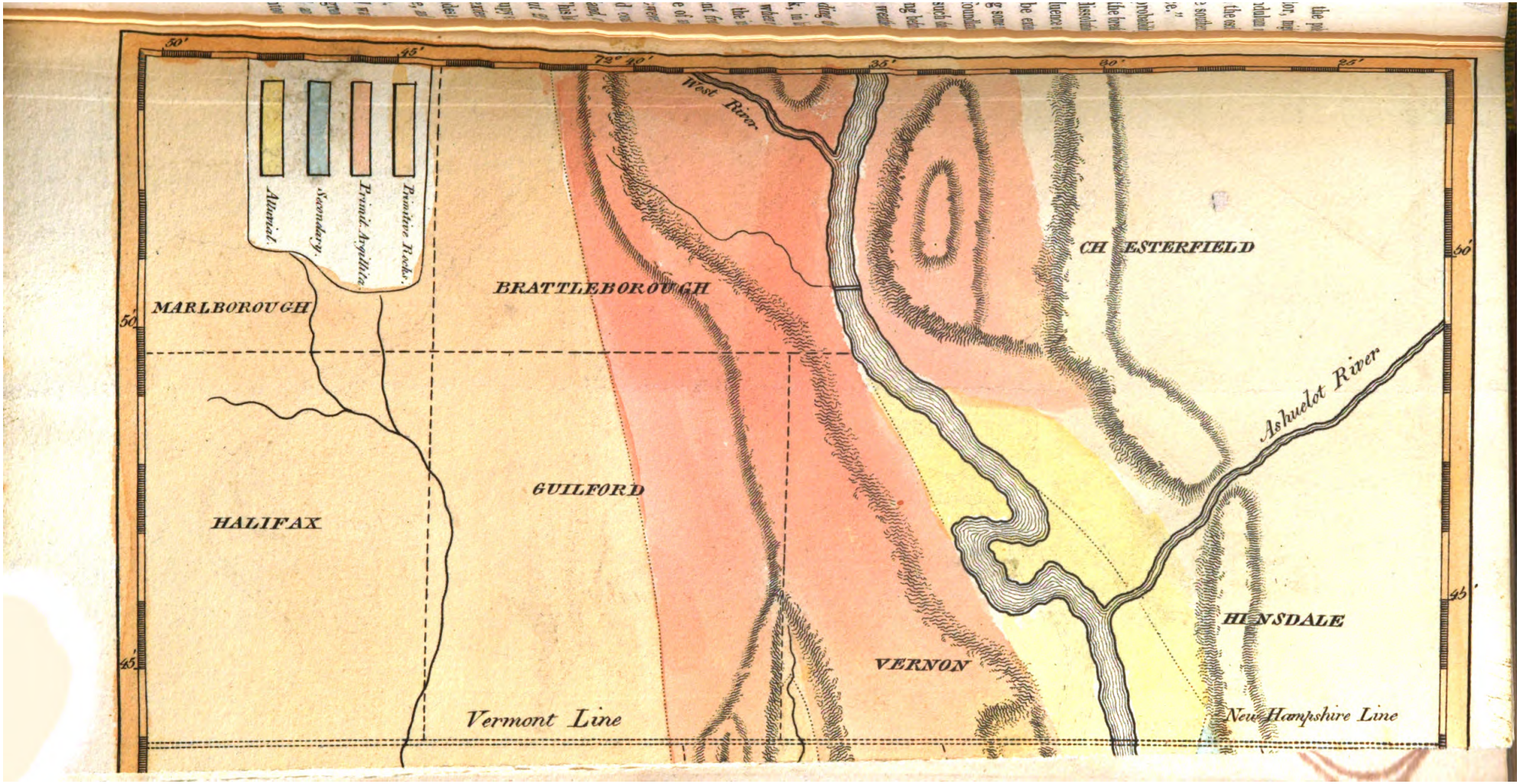
"They will ascertain, what is as yet but very imperfectly known, the depth, the temperature, the saltness, and the specific gravity of the sea-water in those high latitudes—the velocity of the currents, the state of atmospherical electricity in the arctic regions, and its connexion, at which we have glanced, with the inclination, declination, and intensity of force of the magnetic needle; on which subject alone, a collection of facts towards the upper part of Davis's Straits would be worth a voyage of discovery. It has, indeed, been long suspected that one of the magnetic poles will be found in this neighbourhood, as in no part of the world have such extraordinary phenomena been observed, or such irregularities in the vibration and the variation of the needle.

"A comparison of the magnetic influence near the pole, with what it has been observed to be on the equator, might lead to important results; and the swinging of a pendulum as near the pole as can be approached, to compare with the oscillations observed in the Shetland Islands, and in the southern hemisphere, would be a great point gained for science."

We have no room in this Number to consider the probability of success in this attempt, nor the question, whether the breaking up of the Greenland ice, and its passage to, and dissolution in, the south, have been attended with a chilling influence on the continents. That such a chilling effect might be extensively exerted, is certainly credible. Approaching some of the icebergs, in April 1805, on the shoals of Newfoundland, we were rendered very sensible of the vicinity of such dangerous neighbours, by the great chill in the air, long before they were visible; and when we had passed them, the weather again grew milder.

Perhaps it militates against the probability of finding the northern polar basin free of ice, that Captain Cook, in his approximation to the southern pole, in January, 1773, when in latitude 67° 15′ south, "could proceed no farther; the ice being entirely closed to the south, in the whole extent from east to west-southwest, without the least appearance of any opening." The advanced season of the year did not, however, permit Captain Cook to ascertain whether he could coast around this ice—whether it was ultimately attached to land, or was a part of a vast field extending to the south pole. This last is however highly improbable, because being found about 23° from the pole, it is hardly credible that it would occupy so extensive a region as to embrace the pole, and, perhaps extend as much farther beyond; especially as in similar latitudes in the opposite hemisphere, navigation is comparatively free, and has been pushed even to more than 80° of north latitude.

The scientific, as well as the commercial world, will wait with no small impatience for the termination of the two grand arctic expeditions, which are among the most original and daring, and may be among the most interesting and momentous hitherto undertaken by man.


50'
45'
72° 40'
35'
30'
25'
West River
Alluvial.
Secondary.
Primd. Argillite
Primitive Rocks.
MARLBOROUGH
BRATTLEBOROUGH
CH ESTERFIELD
Ashuelot River
GUILFORD
HALIFAX
VERNON
HINSDALE
Vermont Line
New Hampshire Line

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* *ERRATUM.*

In the text this Article was, by inadvertence, numbered XIX, and all the succeeding Articles of this Number are marked *two* higher than they ought to be.

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

MINERALOGY AND GEOLOGY.

ART. I. *Remarks on the Geology and Mineralogy of a Section of Massachusetts on Connecticut River, with a Part of New-Hampshire and Vermont; by* EDWARD HITCHCOCK, A.M. *Principal of Deerfield Academy.*

THE geology of this tract, from a few miles south of Northampton in Massachusetts, to the north boundary of Brattleborough in Vermont, and of Chesterfield in New-Hampshire, is shown on the subjoined map. The primitive formation, except the argillite, is coloured vermilion; the secondary, blue; and the alluvial, gamboge yellow, according to Cleaveland. The alluvial part is elevated above the bed of Connecticut river from 10 to 100 feet, and, in most places, reposes on red sandstone. The soil in the northern part is generally argillaceous; but in the southern more siliceous. The secondary formation consists chiefly of detached eminences that rise abruptly from the plain, and are composed of red sandstone and puddingstone alternating, except the elevations A and B, (Holyoke and Tom) and a part of the range CD, passing through Deerfield and Greenfield, which are greenstone. The part coloured rose-red consists of argillite, sometimes

alternating with mica slate, siliceous slate, or chlorite slate. It is thus coloured to show the extent of the argillite, and not from a belief that this rock is of the transition class; for in this region the argillite is undoubtedly primitive. Some quarries of this rock have been opened in Massachusetts; and in Vermont are extensively wrought. I have not learnt how far the argillite extends northward in Vermont and New-Hampshire. Its strata are almost perpendicular, inclining a few degrees to the west.

The primitive region on the west side of Connecticut river, included by the map, is made up of mica slate, as a prevailing rock, particularly in the northern part. Hornblende slate sometimes alternates with this, and sienite appears in various places, though its strata are generally thin. Limestone also occurs in Deerfield, Conway, Colrain, &c. of a dull brown colour. It contains so large a proportion of silex that it is often but little removed from granular quartz. Lime for building has sometimes been obtained from it. A range of granite, containing veins of lead ore, appears at Southampton, and proceeds to Hatfield. North of this, the other rocks cover it, and it does not again rise within the limits of the map.

Sienite is the prevailing rock on the east side of Connecticut river in the primitive region, more particularly in the southern part. In some places a narrow stratum of mica slate lies next to the conglomerate of the secondary formation, and a low range of graphic and common granite has been observed in Amherst and Leverett, lying next to the mica slate. Other veins of granite also traverse the sienite; and gneiss occurs in many places. The proportion of hornblende in the sienite is generally small, and mica is often present in considerable proportion. Porphyritic sienite is common in this quarter, and steatite occurs in its eastern part.

Most of the primitive region on the map is broken and mountainous, being made up of parallel ridges and detached eminences. The strata run nearly north and south, and dip to the east at angles between 20° and 60°. It would be easy to extend the map on the west to the top of Hoosack mountain, since the country is all primitive; and on the east the primi-

tive continues, with a few exceptions, to the ocean. The map might also be extended to the boundary of Connecticut, by prolonging the primitive ranges with some divergency, and colouring the intermediate space secondary, except a narrow tract on the east side of Connecticut river, which is alluvial. These extensions were not thought necessary.

In the town of Gill, at E, there is a cataract in Connecticut river, from 30 to 40 feet in height; and it is believed that the alluvial region, and part of the secondary shown on the map from this fall to the place where the river passes between mount Holyoke and Tom, was formerly the bed of a lake: for the logs are still found undecayed in many places, from 10 to 20 feet below the surface; the river has evidently worn a passage between Holyoke and Tom: many of the hills on the northern part, and the sandstone on the plain, bear the marks of having been washed by water, and the channels of two rivers are still visible in Deerfield, the one 30, and the other 100 feet above the present bed of Connecticut river. Between mount Tom and the mountains west, there is a secondary plain of sufficient height to throw back the water over the supposed bed of the lake, before a passage was worn between Holyoke and Tom. South of these hills commences another alluvial and secondary tract, extending on both sides of the river to Haddam, in Connecticut, where the river passes between mountains, and perhaps this region also was the bed of a lake.

The plain on which the village of Deerfield stands, with the adjoining meadows, is sunk 50 or 60 feet below the general alluvial tract, and was undoubtedly the bed of a pond, or small lake, that remained after the larger one of which we have spoken had subsided. When this larger lake decreased, Deerfield river was cut off from a communication with the Connecticut by the mountain CD, and the plain extending westward from this mountain. There is a tradition, derived from the aboriginals of Deerfield, that the passage in which Deerfield river now runs through the mountain CD, was begun by a squaw with a clam-shell.

On the margin of these meadows, at considerable elevation, numerous small conical excavations appear. On digging below the surface, stones are found calcined by fire. These are probably the spots where Indian wigwams formerly stood. Many vestiges of the aboriginals are frequently found in Deerfield, such as beads, stone pots, mortars, pipes, axes, and the barbs of arrows and pikes. Near the village they had a burial-ground, where many skeletons have been uncovered. A roll of human hair was lately found here, by Mr. J. C. Hoyt of Deerfield, three-fourths of an inch in diameter, and three inches long, closely tied by a string made of the hide of some animal, which string was encircled by brass or copper clasps greatly oxidized; but the hair and string were in a good state of preservation, though they must have lain there more than a century. In the meadows, logs, leaves, butternuts, and walnuts are found undecayed, 15 feet below the surface; and stumps of trees have been observed at that depth standing yet firmly where they once grew. In the same meadows, a few years since, several toads were dug up from 15 feet below the surface, aud three feet in gravel. They soon recovered from a torpid state, and hopped away.

The small range of hills beginning at the south line of Deerfield, and terminating in Gill, deserves description. At its commencement on the south, a conical hill, called Sugar Loaf, of red conglomerate, rises abruptly from the plain 500 feet. The appearance of this hill, as you come from the south, is picturesque, and it is an interesting feature of the country. The range becomes higher for three miles, where, at its greatest elevation, it is 730 feet above the bed of Deerfield river. The west side of the mountain is precipitous, and in some places naked. The ascent on the other side is gentle.

Both sides of this hill are sandstone and puddingstone, frequently alternating: though these are most extensive on the west side, and as we rise the puddingstone predominates. The strata dip to the east about 10 degrees. Near the centre of this range is a ridge of greenstone, with a mural face on the west, and amorphous masses lying at the base, half way up to

its summit. This ridge does not rise so high as the puddingstone on the west of it, as may be seen in the view of strata with the map. It commences on the west bank of Connecticut river, about a mile north of the hill C, and increases in elevation nearly to the spot where it disappears at the fall of the river in Gill. This rock does not appear to rest on sandstone, but to descend through it, where there is an opportunity for observation. Deerfield river has worn a passage through the sandstone and greenstone 150 feet deep, and the greenstone passes under its bed, and the sandstone, at a few rods distant lies on each side of the greenstone. A similar fact has been noticed at the fall in Connecticut river, in Gill. Yet I have coloured this greenstone secondary on the map; for it is certain that Mount Tom rests on sandstone, and it is stated by Professor Silliman, that the same rock does in Connecticut. Could we penetrate deeper below the surface, it is probable the same would be found to be the case with this greenstone.

As stated above, this rock disappears near the cataract in Gill, and it is succeeded by puddingstone. But four miles farther north, it again emerges in Bernardstone, though it rises but little above the surface. Here its character is changed. The hornblende is more crystalline, and the rock becomes decidedly primitive, as you approach a mountain of argillite and mica slate, into which it passes, and no greenstone has been observed north of this. It terminates not far from the line of Vermont. The red sandstone and conglomerate also terminate on the opposite side of the river in Northfield.

The greenstone in the above described range, is of a finer texture than the same rock in Connecticut; and the feldspar, in some specimens, is scarcely discernible with a microscope. Indeed, in many instances, the eye would decide the rock to be basalt. Much of it is fissile, the laminæ varying from half an inch to a foot in thickness. This is most perceptible among the loose masses; but it exists also in that in place. Whether this circumstance be accidental, I will not attempt to decide.

A large proportion of the greenstone of our vicinity constitutes the base of amygdaloid. The imbedded substances are calcareous spar, quartz, chalcedony, analcime, prehnite, &c.

as will be more particularly mentioned hereafter. Globular concretions of greenstone are common in this amygdaloid, several inches in diameter, and of greater specific gravity than the other parts of the rock. A great number of columns occur in the same range, having from three to six sides. Some of them are quite regular, and are well articulated, exhibiting at their joints considerable concavities and convexities. They are from one to thirty feet long, and, in their natural position, incline a few degrees to the east, as may be seen in the view of strata with the map. A few have been noticed that make lateral curves. One of these hexagonal columns measures at one end as follows :—Diagonals, 27, 29, and 29½ inches ; sides, 16½, 13¼, 11½, 17, 11½, and 16½ inches. The convexity of this column is a little more than an inch. The best instances of these prisms occur one mile east from the village of Deerfield.

Masses of greenstone are found at considerable distance from the range, among the puddingstone. One has been noticed weighing many tons, a hundred rods from the range of greenstone, and on much higher ground. Some of these scattered fragments contain chalcedony. A specimen of petrosiliceous porphyry has been found among the same puddingstone, and also a mass of singular, though not well defined, amygdaloid, whose base is similar to wacke, and imbedded substances are calcareous spar, chlorite, and green earth.

The elevation in the north part of Sunderland, called Toby, from 800 to 900 feet high, is chiefly conglomerate, red, brown, or greenish, which, in some parts, alternates with chlorite slate, secondary argillite, and a sandstone that seems to be passing into gray wacke slate. Some of the imbedded masses in this puddingstone are quite large, its cement is frequently calcareous, its aspect is singular, and it is very different from the puddingstone before described, on the opposite side of the river. At the foot of this mountain, in the bottom of Connecticut river, distinct impressions of fish are found on a schistose rock, like the one above mentioned as passing into gray wacke slate. This same species of slate occurs in several other places at the bottom of Connecticut river, as at the

fall in Gill. In this last place bituminous shale has been noticed.

In Mount Toby, in Sunderland, is a cave nearly 150 feet above the bed of Connecticut river. It opens to the north and west, forming a quarter of a circle, is 130 feet in extent, 60 feet deep, and from 3 to 20 wide. A little to the south of it, is a fissure in the puddingstone, formed by a separation of the rock, ten feet wide, and as deep as the cave. So perfect is this division, that it appears as if cloven down by the sword of some Titan. Perhaps this cave and fissure were formed by the washing of the waters of the lake we have mentioned on the sandstone and conglomerate beneath; thus causing the superincumbent rock to fall and separate. There is no appearance of any other convulsion. Imperfect, calcareous stalactites are found in this cave.

The falls in Connecticut river, at E, are not unworthy of notice. The river here is about 40 rods wide, and the height of the main cataract, raised considerably by an artificial dam, is 30 feet. The fall continues two miles. On the north bank you view the cataract from elevated ground, and can see the river nearly a mile above and below—above, perfectly smooth and calm, below, forming a quarter of a circle, and tumbling among the broken rocks. On the opposite side of the river are a few buildings, the commencement of a canal, and, behind these, moderately elevated hills, covered with woods. Two rocky islands near the middle of the descending sheet, and another thirty rods below, add much to the beauty of the view. Looking from the southeast shore, you have a partial prospect of the falls, and a view of an amphitheatre of greenstone hills, through which a small river empties. The pleasure derived from the view proceeds more from its wildness than its sublimity.

The position of the hills, boundaries, and rivers, on the accompanying map, may not, in all cases, be precisely correct. The general outlines were enlarged by a pentegraph from Carleton's map of Massachusetts, and the intermediate objects were placed chiefly by the eye; their relative situations being determined by travelling over the ground, and viewing them

from different elevations. The boundaries of the several formations have not been so carefully noticed near the angles of the map as in the central parts. Of their correctness generally, however, I am confident. The latitude and longitude of Deerfield, from which those on the map were marked, were obtained by taking a mean of the observations given by Gen. E. Hoyt, in the Transactions of the American Academy of Arts and Sciences, and of twelve lunar observations since made. The result is, Lat. 42° 32′ 32″. Long. 72° 39′ from Greenwich.

With the map is given a view of the strata of rocks from Hoosack mountain to eleven miles east of Connecticut river, on a line nearly east and west, passing through Deerfield. The horizontal distances are laid down from a scale : the elevations are assumed. The principal rocks only are coloured; for it is very difficult to determine the breadth of many, since they frequently alternate with one another. I have not examined the country on the east side of Connecticut river with sufficient care to be able to extend the section on that side more than a few miles.

It may not be amiss to mention, that Mount Holyoke, so much celebrated for the delightful view from its top, has been found, with a sextant, to be 830 feet above Connecticut river. Its height has been frequently overrated.

The mineralogy of this section of the country has been but imperfectly explored. I shall mention those minerals only of which I have obtained specimens, and whose localities have not been noticed by mineralogists.

Quartz—several varieties.

1. *Rock Crystal*—abundant. Some good specimens are found in Conway, on feldspar, with the usual hexagonal, prismatic crystals, and these crystals cross each other in all directions.
2. *Irised Quartz*—found in Leyden.
3. *Granular Quartz*—in Deerfield.
4. *Radiated Quartz*—in Whately and Shelburne.
5. *Blue Quartz*—in rolled masses on the banks of Deerfield river.

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6. *Greasy Quartz*—in same place.
7. *Pseudomorphous Quartz*—in greenstone, Deerfield.
8. *Lamellar Quartz*—in same place. The laminæ sometimes penetrate crystals of common quartz.
9. *Tubular*, or *Pectinated Quartz*—in same place.
10. *Quartz Geodes*—in same place.

Prase—in the north part of Sunderland. (Not good specimens.)

Amethyst—in Greenstone, Deerfield: the colour is not deep, but delicate.

Chalcedony—in same place—considerably abundant, but generally in small masses.

Carnelian—in same place, not plenty. The chalcedony, in some specimens, seems to be passing into cacholong, and the carnelian into sardonyx.

Agate—in same place. It is made up of chalcedony, carnelian, and quartz. They are generally small, but some are elegant.

Jasper, red, and yellow—found in rolled masses on the banks of Deerfield river and in Leyden. Some has been found imperfectly striped. It occurs frequently as it was formed by the aboriginals into barbs for pikes and arrows.

Petrosilex—on the banks of Deerfield river—not good specimens.

Feldspar—the red variety occurs in puddingstone, Deerfield. It is not necessary to mention any other locality of a mineral so common.

Hornblende—very abundant—mostly black in this vicinity.

Mica—this is very abundant on the east side of Connecticut river. Some crystals of it have been found in Amherst.

Talc—in Shutesbury.

Steatite. The localities of this are seen on the section. The aboriginals formed many articles from this mineral, as pots, pipes, &c.

Chlorite—in Shutesbury: also in amygdaloid, Deerfield. In Deerfield academy there are some Indian pipes of this mineral, well wrought.

Green Earth—in small quantities, in amygdaloid, Deerfield.

Schorl—the black variety occurs in Pelham, Shutesbury, and Orange, Mass., and in Brattleborough, Vermont.

Epidote—in Deerfield, Shutesbury, Leyden, and Pelham, and in Athol, Worcester county. The specimens poor.

Tremolite—in the west part of Leyden, near Green river. The rock in this region is chiefly mica slate, and the quantity of tremolite is very great. Tons of it might be easily collected.

Cyanite, or *Sappare*—in Deerfield, in mica slate; discovered by Dr. S. W. Williams.

Actynolite—rare, found in Shutesbury.

Serpentine—found in Leyden in rolled masses. Some of the specimens admit a fine polish, and the ground is handsomely variegated. It has not been noticed *in situ*.

Asbestus—compact, in Pelham.

Garnets—very plenty in Conway, Deerfield, Shelburne, &c. Good specimens of the melanite occur in Conway.

Native Alum—in Leyden, in small quantities, effloresing on argillaceous slate.

Sulphur—in Conway, Shelburne, and Warwick, effloresing on mica slate.

Prehnite—in greenstone, Deerfield, encrusting the columns and in radiated masses, but rarely crystallized. The veins of it, when in place, are nearly perpendicular.

Zeolite—in same place, not abundant. Some good specimens of the radiated variety are found.

Chabasie—in same place, considerably abundant. No crystals have yet been found whose sides exceed a quarter of an inch. It occurs in the veins of the greenstone, in geodes, on balls of zeolite, on chalcedony, on lamellar quartz, &c.

Stilbite—in same place, not abundant. It is commonly associated with chabasie, and the crystals, though small, are well defined.

Analcime—in same place, very abundant, and is associated with quartz and amethyst, which are sometimes enclosed by analcime. It generally occurs in cylindrical, reniform, and radiated masses. A few perfect crystals only have been observed.

Laminated Calcareous Spar—in the same place, not uncommon.

Chalcedony, carnelian, agate, amethyst, prehnite, zeolite, chabasie, stilbite, and analcime, have been found nearly in the same place; and it may not be amiss to observe, that this spot is distant from Deerfield Academy about one mile, and bears from the same, by a true meridian, E. 2°, 15′ S.

Iron Sand—found in considerable quantity near the falls in Connecticut river, on the Montague shore.

Sulphate of Iron—in Conway, in small quantities, effloresing on mica slate.

Sulphuret of Iron—in Halifax, Vermont, in abundance; also in Charlemont, Mass., Deerfield, &c.

Magnetic Oxide of Iron—very common in the region west of Connecticut river. I have observed it in Athol, Worcester county.

Specular Oxide of Iron—some veins of this ore occur in Hawley, Bernardstown, and Warwick, and have been wrought to a small extent.

Micaceous Oxide of Iron—in the iron mine in Hawley.

Green Carbonate of Copper—in greenstone, in Greenfield. This ore constitutes a vein on the bank of Connecticut river, passing into the hill on one side, and under the river on the other. It has never been wrought, nor, indeed, is its locality publicly known.

Copper Pyrites—in the same vein, not abundant, at the surface.

Sulphate of Barytes—in the same place, constituting the immediate walls of the vein. Its breadth on the wall varies from an inch to a foot, and the breadth of the vein is 6 or 8 feet.

Galena—in Whately. This is probably from a continuation of the vein of this ore that appears at Montgomery, Southampton, and Hatfield. A single crystal has been found in the same range, in Greenfield, twelve miles north of Whately; but it was not in place.

Red Oxyde of Titanium—in Leyden, crystallized on quartz and tremolite, chiefly on the latter; colour brownish red—specific gravity 4.232; scratches glass, handsomely geniculated, and sometimes several geniculations in the same specimen; in one as many as six could be perceived.

Eagle Stone, or *Nodular argillaceous Oxide of Iron*—one specimen on the banks of Deerfield river.

Rose-red Quartz—a loose mass in alluvial soil, Deerfield.

Red Oxide of Titanium—in Shelburne.

I would acknowledge my peculiar obligations to Professor Silliman, of New-Haven, and to Dr. David Hunt, of Northampton, Mass. for the very generous assistance they have given me in a commencement of the study of mineralogy, and for their liberal aid in this particular communication. Their kindness, it is believed, will not soon be forgotten. To several others, also, I am indebted for communicating facts of importance.

Deerfield, October, 1817.

Art. II. *On the Prairies and Barrens of the West, by* Caleb Atwater, Esq. *in Letters to the Editor.*

Circleville, Ohio, May 28, 1818.

Dear Sir,

I SEND you for publication in the Journal of Science, an Essay on the Prairies and Barrens found in this country.

Description of the Prairies.

Prairie is a French word, signifying a meadow, but is here applied only to natural meadows. They are found in all the states and territories west of the Allegany mountains, more or less numerous, of greater or less extent. They are covered with a coarse kind of grass, which, before the country is settled

in their vicinity, grows to the height of six or seven feet. After these natural meadows are fed upon by domestic animals, the grass does not grow to a greater height than it does in common pastures. Sometimes this grass is intermixed with weeds and plum-bushes. Some of those prairies are dry, while others are moist. Pickaway Plains, in Pickaway county, in the State of Ohio, lying a small distance south of this place, are nearly seven miles in length, and about three miles in width, on ground considerably elevated above the Scioto river, almost perfectly level, and, in their native state, were covered with a great quantity of grass, some weeds and plum-bushes; and in the most elevated places, there were a few trees. This was one great prairie.

Sandusky Plains, lying on the high ground between the head waters of the Whetstone branch of the Scioto river, and the waters of streams running into Lake Erie, are still more extensive than those of Pickaway, covered with a coarse, tall grass, intermixed with weeds, with here and there a tree, presenting to the eye a landscape of great extent.

The moist prairies generally lie along some stream, or at the head of one, on level land, or on that which gently descends. The moist prairies are too wet for trees to grow on them; and whether moist or dry, the soil, for a greater or less depth, is always alluvial, resting on pebbles and sand, such as are found at the bottom of rivers, ponds, and lakes. In some instances, the writer is credibly informed, that the shells of muscles are found imbedded in the pebbles and sand. That these shells, such as abound in our rivers, ponds, and lakes, should be found in low prairies along the banks of waters which frequently overflow them, excites no wonder, nor even surprise; but that these shells should be found thus imbedded in pebbles and sand underneath several feet of alluvial soil, in situations more than one hundred feet above the waters of any stream now in existence, is calculated to perplex the mind of the superficial observer. These prairies are found in the western half of the State of Ohio, and north of the hills adjacent to the river of that name. They are also found in every state and territory west of the Alleganies, from the great northern lakes

on the north, to the Mexican Gulf on the south; from the western foot of the Allegany mountains, to the eastern one of the Rocky mountains, up the Missouri. In summer, the grass which spontaneously covers them, feeds immense herds of cattle; in winter, the hay that is cut on them, with a little Indian corn or maize, feeds and fattens the same herds. Some of these prairies extend as far as the eye can reach; others contain only a few perches of ground.

Description of the Barrens.

But besides these prairies, there are also extensive tracts of country in this part of the Union which deserve and shall receive our notice; they are called "*Barrens.*" From their appellation, "barrens," the person unacquainted with them is not to suppose them thus called from their sterility, because most of them are quite the reverse. These barrens are found in a level country, with here and there a gentle rise, only a few feet higher than the land around it. On these little rises, for they are not hills, trees grow, and grass also; but grass and weeds are the only occupants of the soil where there is no rise of ground. The soil is alluvial to greater or less depth in these barrens, though on some of the highest rises there is little or none; the lower the ground the deeper the alluvion. On these gentle rises, where there is no alluvion, we find stiff, blue clay, and no pebbles. Under the alluvial black soil, in the lower grounds, we find pebbles similar to those in the prairies, owing to similar causes. On the little ridges, wherever the land is not too moist, the oak or the hickory has taken possession, and there grows to a moderate height, in clusters. It would seem, that whenever the land had become sufficiently dry for an acorn or a hickory-nut to sprout, take root, and grow, it did so; and from one or more of these trees, in time, others have grown around them in such clusters as we now behold. Where the land is lower, the soil deeper, more moist and more fertile, the grass was too thick, and the soil too wet, for such kind of trees to grow in as were found in the immediate vicinity. Imagine, then, natural meadows, of

various dimensions, and of every figure which the imagination can conceive, with here and there a gentle rise of ground, decked with a few scattering trees or a thick cluster of them, and bearing a tall, coarse grass, which is thin on the rises, but on the lower grounds thick and luxuriant; imagine, also, a rill of a reddish colour scarcely meandering through ground a little lower than the surrounding plain, and you will have a very correct idea of the appearance of these barrens. They are *generally* (not always) found on what, in our western dialect, is called *second bottom*, and not on a level with any streams of magnitude, but rather at their sources. To mention all the counties of this State where these prairies and barrens are found, would be too tedious, and illy comport with the object which we have in view. We shall therefore content ourselves with describing those found in the north half of Fayette county, and the adjoining county of Madison, which may be said to be almost entirely one great barren of more than forty miles extent from north to south, and generally half as much in breadth from east to west. The great barren in Fayette, Madison, and, we may add, in the counties still north of them, is on land elevated from fifty to one hundred feet above the level of the Scioto river, into which the streams that have their sources in this tract of country generally run. This land lies so level that the waters stand on it too long for grain to thrive equally with grass, unless, indeed, the farmer should dig a long drain, which is easily effected by the plough, with a little assistance from the hoe and the spade. But as nature seems to have intended this tract of country for the raising of cattle instead of grain, the husbandman has listened to the suggestion, and in this great barren are found some thousands of the finest cattle which the State affords. Here the horse, the ox, and the swine feed, thrive, and fatten with little expense to their owner; but sheep do not, and never will, thrive on prairie grass, or wet grounds. Fruit-trees, the peach, the apple, the plum, &c. do very well when planted on the gently rising grounds, where the hickory or the oak had once stood. Fruit-trees, such as have been named, thrive very well also on the dry prairies. On the eastern side of the Allegany

mountains there neither is, nor was there ever, any thing like these prairies and barrens, if we except those found in the western part of New-York, in the Genesee country, and in the vicinity of the lakes in that quarter. These, the writer of this saw nearly thirty years since, and before that country was much settled. Those prairies were similar in appearance to ours in the west, and were, beyond doubt, formed by similar means.

Speculations on the Origin of the Prairies and Barrens.

What were the causes which contribute to form these natural meadows? That water was the principal agent in their formation, we very little doubt; but this is not the common opinion. According to that opinion, our prairies and barrens, and especially the latter, were occasioned entirely by the burning of the woods by the Indians, in order to take the wild game. Let us try this opinion by the indubitable appearances exhibited by these prairies and barrens.

They are invariably found in a level country, or in one which is nearly so; and the soil is generally, if not always, more moist than that which is uneven and hilly. Would not the leaves, where the land is dry, burn over with as great facility, or even with greater facility, than the grass would where the land is wet? Would there not be more wild game where they could find their food in plenty, such as acorns and hickory nuts, on which they feed in winter, than on land where no food, except dry grass and weeds, were to be found? It is well known that these prairies and barrens could not be burnt over when the vegetable productions which cover them were growing. At the only season when it is possible to burn them, that is in winter, to what kind of regions do the wild animals resort? Is it not to the thick woods? Every hunter will answer in the affirmative. For the space of twenty-five years, the writer of this lived in the vicinity of Indians, and from information on which he relies, as well as from his own actual observation, he confidently avers that the Indians neither are, nor ever were, in the habit of firing the woods

in order to take game. Erroneous information first propagated such an opinion, and blind credulity has extended it down to us. Another opinion, equally groundless, prevails to a considerable extent; and that is, that these prairies have all been heretofore cultivated by the aborigines, and that the grass having overspread these plains, prevented the growth of trees on them. The Indians, it is to be presumed, never cultivated any other grain than maize, or Indian corn, and yet we see few or no corn-hills in any part of this country. In the western part of New-York, before it was settled by its present inhabitants, thousands and thousands of acres were to be seen, where the trees were as large as any in the forest, and yet the rows of corn-hills were plainly discernible. I refer in a particular manner to what is now called Cayuga county. There the growth of grass had not prevented the growth of trees, nor did it here. We know that some of these prairies were cultivated by the Indians, but never to any very considerable extent. This country never was thickly settled by Indians, like the shores of the Atlantic and the banks of the rivers running into it. No, it was the ancestors of the Peruvians and the Mexicans who lived here in great numbers, before they migrated to South America.

The question then recurs, by what powerful means were these prairies and barrens formed?

That water was the principal agent, we infer from the fact, that the soil is always alluvial to greater or less depth; the former we call prairie, the latter barren. But how could the country from the southern shore of Lake Erie to Chillicothe, a distance of more than one hundred and fifty miles from north to south, ever be covered with water long enough to form alluvial soil, in many places from four to six feet in depth? I answer, that the Niagara river, the present outlet of Lake Erie, has worn away several hundred feet, and in that way the lake is lowered in the same proportion. The high land, composed entirely of sand, originally extending from the Ohio northerly upwards of forty miles, to Chillicothe, has been worn through by the Scioto river; and the waters which once for ages covered the whole country north of the hills along the Ohio

river have been drained off, and the dry land appears where once stood the waters of lakes Erie and Michigan, then forming but one great lake. I am fully impressed with the belief, that were the bottom of Niagara river as high as it once was, the upper lakes would now, as formerly, empty themselves into the Ohio by the Scioto and Miami rivers, and into the Mississippi by the Illinois. I might proceed to examine every part of the country where prairies and barrens are found; but they have all been formed by the same agent, and that is water. An objection to this opinion may be raised by some, that these prairies and barrens are frequently found in the counties of Delaware, Champaign, Madison, Fayette, &c. on ground considerably elevated. Are they higher than the hills near Chillicothe? From a careful inspection, but without any instruments, I am convinced that they are none of them as high.

There is no perpendicular fall of water, but merely a gradual descent, from Columbus to the Ohio; nay, there is no fall from the very source of the Scioto to its mouth. Every one acquainted with hydrostatics, knows that water will run briskly where the descent is only a few inches in a mile. The writer believes that the Scioto, from its source to the Ohio river, does not descend more than one hundred feet, and that the present surface of Lake Erie is about on a level with the Ohio in a freshet; that before the channel of Niagara river was deepened, as it evidently has been, by the attrition of that mighty stream; and before the hills adjacent to the Ohio were worn down by the waters of the Scioto, the whole country north of Chillicothe, where these hills commence, to Lake Erie inclusive, was covered with water, except the very highest hills in the counties of Greene, &c. which were then islands. What tends to corroborate this opinion is, that on these high grounds we find limestone and other rocks, and indications of gypsum; but no alluvion, and none of those fragments and ruins which are produced by water acting mechanically upon a country for a long space of time. We might mention other parts of country where prairies and barrens abound, and which have been formed by water. Those along Greene river, in Kentucky, have evidently been covered by

the waters of that river. The bed of that stream has been deepened by the constant flowing of the water along its channel; the water is drained off, and the prairies and barrens now occupy the soil which the water had made and formerly covered. The prairies above the falls of Hockhocking, along that river, have evidently been formed in the same way, and owe their origin and appearances to similar causes. There is near Lancaster, on the last-mentioned river in the State of Ohio, and near the great road, a gentle rise of ground in the prairie, which has every appearance of having been an island, and is so called by the people of the vicinity.

In fine, wherever prairies and barrens are found, there, for a long space of time, water once stood, but was gradually drained off. Else why alluvial soil to such a depth, in low situations, and growing thinner as we ascend on ground more elevated? Else why do we find rocks in more elevated tracts of country, and not in prairies or barrens? Else why do we find no alluvion, no grass, but a thick growth of ancient forest-trees on the higher lands? Else why do we find beneath the alluvion of the prairies, pebbles and shells similar to those at the bottom of lakes and ponds? Else why do the higher grounds to this moment present the appearances of so many islands? And all these indications where no stream now in existence could by possibility have reached them?

That the waters which once covered so great a part of this State (Ohio) were drawn off gradually, we infer from the fact, that there is not a single indication of the effects of an earthquake or volcano, from the foot of the Allegany to the banks of the Mississippi: in this region not a stone nor a layer of earth has been misplaced, nor its position changed.

But an interesting inquiry here presents itself. Were the hills along the Ohio, before they were worn away by the streams which now empty themselves into that river, ever high enough to raise the water to the north of them to such a degree that it would overspread the country where the prairies and barrens are now found? Although the height of these hills has not been ascertained by the proper instruments, yet from appearances, not to be mistaken by any person who

examines them and the country towards Lake Erie, these hills are much higher than any land between them and that lake. And from certain indications, (as already remarked,) had not the bed of the Niagara been deepened by the running of that mighty river, Lake Erie, as formerly, would empty itself into the Ohio by the Scioto and Miami; and the great northern lakes would once more discharge themselves into the Mississippi by the Illinois. Lake Ontario, from some cause, (possibly an earthquake, or the wearing away of its outlet, or both,) is considerably lower than it was formerly: in that way the land along its banks, once covered by its waters, is drained, presenting appearances exactly similar to those seen in many of our prairies.

Miscellaneous Remarks on the Prairies and Barrens relative to their Picturesque Features, and to Agriculture and Health, as affected by the peculiarities of these Tracts.

To the traveller, who for several days traverses these prairies and barrens, their appearance is quite uninviting, and even disagreeable. He may travel from morning until night, and make good speed, but on looking around him, he fancies himself at the very spot whence he started. No pleasant variety of hill and dale, no rapidly running brook delights the eye, and no sound of woodland music strikes the ear; but, in their stead, a dull uniformity of prospect "spread out immense." Excepting here and there a tree, or a slight elevation of ground, it is otherwise a dead level, covered with tall weeds and coarse grass. The sluggish rivulets, of a reddish colour, scarcely move perceptibly, and their appearance is as uninviting to the eye, as their taste is disgusting to the palate. Such are the prairies and barrens of the west; but, in order to make ample amends for any deficiency, nature has made them exuberantly fertile. The farmer who settles upon them, by raising cattle, becomes rich with little labour. He ditches those which are too moist for grain; he ploughs and fences them, and raises from seventy to one hundred bushels of maize or Indian corn to the acre, without ever hoeing it. The United States own

thousands and thousands of acres of such land in these western States and territories, which, for prompt payment, may be purchased for one dollar and sixty-two and a half cents an acre. One objection to these lands is, the want of timber for fuel and other purposes; and another is, that they are unhealthy: but in many places there is an abundance of peat in the wet prairies, and cultivation will every year render them more and more healthy. Some of them have been cultivated for fifteen or twenty years past with grain, and are as fertile as they ever were. As M. Volney says, "They are the Flanders of America."

Yours, &c. C. A.

ART. III. *Account of the Coal Mines in the vicinity of Richmond, Virginia, communicated to the editor in a letter from Mr.* JOHN GRAMMER, *Jun.*

PETERSBURGH, *Virg. Jan. 28th*, 1818.

Dear Sir,

IN compliance with your request, that I would send you some account of the Virginia coal pits, I paid a visit to them soon after my return, in company with Mr. R. W. Withers, and I will now proceed to give you the account proposed.

The pits, which we made the particular object of our visit, are situated in the county of Chesterfield, about 14 miles distant, in a direction W. S. W. from Richmond, and 3 miles south of James' River. The country rises gradually from Richmond to the pits; and, from its sandy appearance, is evidently an alluvial deposit, although its substratum is the granite mentioned by Mr. M'Clure, as extending through this state from S. S. W. to N. N. E. The coal is found on the western or upper surface of the granite, coincident with it both in direction and inclination; but whether they come immediately in contact or not, has not yet been ascertained. The 'bed' of coal is supposed by the miners to be coextensive with

the granite, and I can discover no very good reason for disagreeing with them in this particular; but, on the contrary, many circumstances concur to strengthen the opinion that it is really coextensive with the granite. The coal is now procured from at least 25 different pits, opened at convenient distances through an extent of from 50 to 70 miles. It every where commences at the upper surface or termination of the body of granite. Some suppose that it is imposed on the granite; and others, that a thin stratum of slate is interposed between the coal and granite. It is always found covered by the slate. The granite is inclined to the horizon at an angle of 45°, and the coal has the same inclination. And since the coal, as far as it has been discovered, is found to accompany and correspond with the granite, why may we not suppose that it continues to accompany the granite, where it has not yet been discovered? At Heth's pits, the coal is 50 feet thick, measured on a line perpendicular to the surfaces of the extreme strata. At some of the pits between Heth's and James' River, it is 30 feet thick; and at the river, not more than 25 feet. The thickness of the coal on the north side of James' River, at the pits in Henrico and Hanover counties, is variable, but at no place greater than 25 feet; and to the south of Heth's, in the pits extending to the Appomatox river, it is still less thick. These facts would induce the supposition, that the coal was deposited in a bed, near the centre of which Heth's pits were sunk. But, on the other hand, the coal is distinctly stratified, and the number of strata increases as the coal proceeds from the surface of the earth; of course, therefore, the farther you proceed from the outer extremity of the coal, the thicker the body of it will be found; and from the inclination of the coal, the farther you are from its outer extremity the deeper it must be under the surface of the earth. Heth's pits are 100 feet deeper than any that have yet been sunk; and all the pits, that I have seen, appear to be nearer to the outer extremity of the coal. We may conclude, therefore, that if the others had been sunk as far from the outer extremity, they would have been as deep, and the coal would have been found as thick in them as in Heth's. Heth's pits, now so called,

were first opened about 30 years since, and worked to some considerable extent. Experiencing, however, much inconvenience from the near approach of the works to a part of the coal which was on fire ; and finding, from their unskilful mode of mining, that the business was not profitable, they abandoned the works, and filled up their shaft. Some few years after, Mr. Heth obtained possession of the land ; and, having imported two Scotch miners, commenced working the coal again. He has now three shafts open, in a line with each other, in the direction of the vein. They are sunk near the brink of a steep hill, which rises about 180 feet from the western bank of a small brook. The depth of one of the shafts is 350 feet. The other two are about 300 feet deep, each. A steam-engine, constructed by Bolton & Watt, is erected at the middle and deepest shaft. It is used exclusively for pumping out water ; but I will not trouble you with an account of the *modus operandi*, as it would be only a repetition of your own description of the same operation at the Cornwall mines. The coal is raised in a box, called by the miners a *cowe*. These cowes contain about two bushels each, and two of them are alternately rising and descending in each shaft. They are raised by means of ropes, fastened to a simple wheel and crank, which is turned by mules. In sinking their shafts, they cut, in the first place, perpendicularly (i. e. to the surface of the earth) through the coal, to its lower surface ; and then turning westwardly, they open a horizontal gallery through the inclination of the vein, to its upper surface ; by this means, to use their own terms, "gaining a double cut on it." Their principal gallery passes (in the direction of the vein,) by the mouth of each shaft. Its length is 1350 feet, and it is terminated at each end by a hitch or dyke of hard sandstone. (The passage was stopped with rubbish in such a manner as to prevent me from seeing the stone myself, and the gentleman who escorted me through the mines is my authority for its being sandstone ; he might possibly, however, have been mistaken, as it is difficult to ascertain what a stone is, in such a place, until it is broken.) When I was at the pits, they were preparing to blast through this rock. At right angles to the principal gallery, they have

opened, at convenient distances apart, shorter galleries, running westwardly, and these are again connected by passages parallel to the first or principal gallery. Pickaxes are the only tools used in working the coal, as it breaks very readily, in the direction of the strata. The roofs of some of the passages are perfectly smooth; and in such, the light of the lamps, reflected from the great variety of colours in the coal, presents a very brilliant sight. The gloomy blackness, however, of most of the galleries, and the strange dress and appearance of the black miners, would furnish sufficient data to the conception of a poet, for a description of Pluto's kingdom. A strong sulphurous acid ran down the walls of many of the galleries; and I observed one of the drains was filled with a yellowish gelatinous substance, which I ascertained, on a subsequent examination, was a yellow, or rather a reddish, oxide of iron, mechanically suspended in water.

I mentioned above that a part of the coal was on fire: I could not ascertain when this fact was first observed to exist; and it is not impossible that the coal may have been burning a century, or more. It is highly probable, however, that a comparatively small quantity of the coal is consumed, as the combustion must be greatly retarded by the absence of a sufficient portion of atmospheric air. A strong sulphurous fume issues from an irregular hole in the side of the hill of about 2 feet diameter. The hole appears to be only 4 or 5 feet deep, and the smoke rises into it from cracks, partly filled with loose clay. The earth is very much cracked around the hole, to the distance of 12 or 15 feet; and these cracks are from 1 to 4 inches wide. The mouth of the hole is encrusted with acicular crystals of pure sulphur. Attempts were formerly made to extinguish the fire, by turning water into this hole; and, after every attempt, there was a temporary disappearance of the smoke for several weeks; but never longer than three months. For several years, however, they have desisted from such vain attempts, and have taken advantage of the facility afforded, by the existence of this fire, for ventilating the mines, in the following manner:—They opened a passage from their present, to the old deserted, works; this they can open

or shut, by means of a close door. As the old works are very near the fire, the air in them becomes very much rarified by the heat; and probably a considerable portion of it is consumed (as the principal pabulum for the combustion) and a partial vacuum is produced. When the air in their present works, therefore, becomes impure, they open the door, and a strong current rushes into the old works; its place is again supplied with fresh air through the shafts. Previous to the adoption of this mode of ventilation, they experienced great inconvenience from carbonic acid gas; and some of the workmen had been killed by an explosion of carburetted hydrogen gas. Since this mode has been adopted, they have experienced no inconvenience at all from noxious gases. On inquiry, I was told that the substances passed through, in getting to the coal, varied in the different pits. As far, however, as I could learn by inquiry, and an examination of the heaps of rubbish, the following substances, in the order in which they stand, have been found in Heth's pits:—mould, clay, gravel, fuller's earth, sandstone, (at first extremely coarse and friable, but becoming more compact and hard, and having an appearance somewhat stratified as they descended,) gray and bluish clay slate, hard bluish sandstone, shale, or, as they term it, shiver, white micaceous sandstone, extremely hard; blue slate and shale intermixed, black slate, and then the coal. The depth of these strata differed so much in different pits, that their individual thickness could not be ascertained. Vegetable impressions are very common in the slate next the coal; and they have found the impression of a fish. Pieces of pure charcoal, in the form of sticks, or logs, are frequently found in or on the coal. In sinking one of the pits they met with a perpendicular column, 8 inches in diameter, extending through the slate into the coal; in all about 50 feet. Its surface was distinctly serrated, and at intervals of about 2 inches it appeared jointed, breaking easily at the joints. For the want of a better name I must call it a "lusus naturæ;" for it is neither clay-slate nor mica-slate, nor shale, nor sandstone; but appears to be composed of them all. Masses of a black oxide of iron are sometimes found in the slate; and from its weight and hardness the miners very

properly call it ironstone. Iron pyrites are very abundant in the slate, and the heaps of rubbish are white with the sulphate of alumine ; yellow ochre is found among the rubbish, but I could not ascertain its relative position with any precision. The side of the hill at the pits is covered with quartz pebbles ; some of which are as transparent and beautiful as I ever saw. The country, for several miles around the pits, (i. e. as far as I have seen,) appears to be entirely destitute of rocks or pebbles, and is covered with a light sandy soil. I am unable to inform you of the number of hands employed at, or of the quantity of coal annually furnished from, these pits, as a part of my notes has, by an accident, been rendered illegible.

Thus, sir, I have endeavoured to comply with my promise of giving you an account of the coal pits.* In doing this, I have *only* attempted to state facts as they existed ; although I have no doubt that my imperfect acquaintance with geology has occasioned many omissions which might have been interesting. To the same cause must be attributed the use of language not always strictly scientific, and a method less exact than might have been desired. With all its imperfections, however, if you can, from the mass of facts, cull any one which may be useful or interesting, I shall be fully compensated by the pleasure of having furnished it, for any trouble I may have been at in doing so. And, if at any time I should be able to furnish you with any information relative to the mineralogy or geology of this part of the country, I hope you will let me know it.

* In using the word "pit," instead of "mine," I have accommodated my language to the custom of the country.

ART. IV. *Sketch of the Geology and Mineralogy of a part of the State of Indiana, communicated in a letter to the Editor, by Mr.* W. B. STILSON.

LOUISVILLE, (Ken.) August 11, 1818.

Dear Sir,

I HAVE employed a short period of leisure in passing over a portion of the state of Indiana. Among other objects, I was not wholly inattentive to the mineralogical and geological features of the country. I now, with diffidence, transmit to you the result of my inquiries.

SKETCH, &c.

The secondary formation of the state of Indiana is abundantly evident. The surface of the soil is undulating, and marked with few elevations which deserve the name of mountains. The rocks are sandstone, limestone, and clay-slate; all of which are disposed in horizontal strata. The sandstone presents nothing remarkable in its appearance. Its colours are various shades of gray and brown. The principal hills are of this formation. The principal colours of the limestone are blue and gray, and their various mingled and intermediate shades. Its secondary formation is very manifest from its almost earthy appearance. In innumerable instances, the limestone rocks contain immense quantities of imbedded shells, of great similarity in form and appearance, and having considerable resemblance, to the common escallop-shell of the ocean. Owing to the easy decomposition of these rocks, and the horizontal position of their strata, they afford many subterranean passages for water. A considerable stream, called Lost River, runs into a cave in the side of a precipitous hill; and, after a passage of 6 or 7 miles under the earth, again makes its appearance, with a large accession to its waters. The traveller's attention is continually excited by cavities in the earth, where the temporary rivulets, proceeding from

rains, make a sudden exit through perpendicular perforations in the upper stratum of the rock. There are many such cavities, which do not receive any water from the surface. Some of them are many yards in diameter, forming a regular circular concave, of considerable depth towards the centre. They are vulgarly known among the inhabitants by the name of "*sink-holes.*" The localities of slate are few, and present nothing uncommon.

With regard to the particular minerals. On Sand Creek, 60 miles from White River, is an interesting locality of that variety of silex, commonly called burrstone. It has been examined by several practical millers, who do not hesitate to pronounce the specimens which it affords, equal, if not superior, to the French burrs. The locality is twenty acres in extent, and appears to be inexhaustible. The mineral varies very much in its appearance; it is generally porous, and appears to have been puffed up by the escape of some gas, while it was in a state of fusion. A mass of well-raised bread gives no inadequate idea of its configuration. It produces most vivid sparks with steel. Some labourers are employed in procuring millstones from this place; and, such is the size of the siliceous rocks, that they are under no necessity of constructing them of detached masses. They form, of a single rock, millstones of five and a half feet in diameter, which are not defaced by any irregularity, or even earthy cavity. These millstones may be carried down the White, Wabash, Ohio, and Mississippi rivers, to New-Orleans, with great facility. And if they should prove as excellent as it is expected they will, this discovery will shed new lustre upon the accumulating evidence of the mineralogical resources of this republic.

Many other varieties of silex are common: rock crystal, agate, and chalcedony, are often found in the beds of rivulets. I passed a considerable distance upon the banks of a small stream, called Leather-wood creek: the bottom of the creek was covered, the whole distance, with siliceous masses, shaped like oblate spheroids, and of every size, from that of a large melon downwards. On being broken, they presented beautiful geodes of crystallized quartz, amethyst, &c. The outside was often

fine chalcedony, and sometimes the interior was the same substance, in the form of balls; all these were sometimes combined, forming agates of great beauty.

Carbonate of lime, crystallized, is sometimes found; and many of the caves afford fine stalactites.

There is a large cave near Corydon, celebrated for the production of sulphate of magnesia, or Epsom salts. It has been explored for the distance of several miles. When it was first discovered, the bottom, in many places, was covered to the depth of several inches, with pure, brilliant, needle-shaped crystals of sulphate of magnesia. By some mysterious process of nature, or rather of Divine benevolence, the production of this useful salt is continually going on. This cave also produces some other salts in small quantities: nitrate of lime, nitrate of magnesia, sulphate of lime, &c.

Where the basis of the country is limestone, the waters always take up a great quantity of lime, and some of them possess great petrifying powers. I saw many specimens of petrifactions: a tuft of moss, the form perfectly preserved; leaves, bark, and branches of tress; insects, and many others.

Many of the springs are strongly impregnated with sulphur, and some of them are saturated with sulphuretted hydrogen. I found the opinion universally prevalent among the people of this state, that the first appearance of these sulphur springs was immediately subsequent to the earthquakes of 1812. They say, that then new springs, impregnated with sulphur, broke out, and the waters of some old springs, for the first time, gave indications of this mineral. A sensible farmer, who has a large sulphur-fountain, boiling up from the bottom of a river near its bank, assured me, that there was no trace of this spring until after the period to which I have alluded. He could have no interest in deceiving me; and if he did deceive me, his conduct could originate only in that love of the marvellous which is so characteristic of the human mind. He moreover assured me that the "water had been growing weaker, (to use his phrase) ever since its first appearance." I have room only to mention, among the minerals of Indiana, many varieties of clay, ochres, gypsum, alabaster, muriat of soda, (very common,) iron ore, and antimony.

Art. V. *New localities of Agate, Chalcedony, Chabasie, Stilbite, Analcime, Titanium, Prehnite, &c.*

DEERFIELD, &c. In the account of the Mineralogy and Geology of Deerfield, by Mr. Hitchcock, in the present Number, it will be seen, that these interesting minerals (with the exception of titanium) exist in the secondary greenstone of that place. We have specimens, (through the kindness of Mr. Hitchcock,) and observe that the agates, chalcedony, analcime, and prehnite, are imbedded in the trap; the agates are in some instances very delicate in the disposition of their bands, and need nothing but polishing to make them beautiful; the same is true of the chalcedony. The chabasie and stilbite occupy cavities, and the chabasie is often distinctly crystalized in a rhomboid, so nearly approaching a cube, in the quantity of its angles, that the mistake is easily committed of supposing them to be cubes; the crystals are sometimes transparent, and the largest a quarter of an inch in diameter. Titanium is found in Leyden; it is the red oxide—very well characterized—in reddish brown crystals as large as a common goose quill,* and, in some instances, perfectly geniculated. It is rare to see finer specimens.

East-Haven. It will be observed, that the great ranges of secondary greenstone, which cut Connecticut and Massachusetts in two, terminate at New-Haven, on the one hand, and some way above Deerfield on the other. By comparing the account of the termination at New-Haven (Bruce's Journal, v. i. p. 139.) with that now published, of the termination at or near Deerfield, it will be seen that the geology and imbedded minerals are very similar. At East-Haven, (one of the branches of the greenstone of New-Haven, and within from three to four miles of the latter town,) chalcedony is often found, sometimes imbedded in the trap, (but perhaps more frequently loose among the fallen stones,) which, although in

* Since the above article was written we have received some as large as a finger.

small pieces, is as perfect in its characters as the chalcedony of the Feroe Islands. It is of a delicate gray, translucent, mamillary, botryoidal, stalactitical, or impressed by crystals of quartz, which have usually fallen out; sometimes these crystals incrust the chalcedony.

Agates also are found in considerable numbers, both imbedded and loose. They usually consist of bands of chalcedony and quartz, and sometimes of the latter only, variously striped or spotted, or interlaced with jasper, carnelian, and cacholong.

The form of the imbedded agates at East-Haven is commonly ovoidal, or egg-shaped, and frequently it is conical. Some portions of pure chalcedony occur, which are shaped like a long, slender carrot or parsnip, and the situation of the latter in the ground would exactly represent that of the chalcedony or agate in the rock.

The imbedded masses are frequently altogether quartz, and then they are most commonly geodes or hollow balls lined with crystals, commonly very perfect and brilliant, although rarely large. These crystals are commonly transparent and colourless—but they exhibit also most of the varieties of colour which quartz assumes—the amethyst—the smoky—yellow, &c., and occasionally they are tipped and spotted with red jasper.

The spontaneous decay of these trap rocks causes many specimens to be found among their ruins, and many more are imbedded in the solid rock; but the industry of successive classes from the neighbouring college, issuing from Col. Gibbs's cabinet, has now made specimens more scarce.

Woodbury. Twenty-four miles from New-Haven, N.W.

In a geological sketch of parts of the counties of New-Haven and Litchfield, which may appear in a future Number, it will be seen that prehnite, stilbite, and agate are found at Woodbury, in the little basin of secondary greenstone which exists there; the prehnite is abundant—it is not known whether the agates are so, although it is asserted to be the fact; the stilbite was not observed to be abundant, although it was well characterized.

ART. VI. *Account of the Strata perforated by, and of the Minerals found in, the great adit to the Southampton Lead Mine. Communicated to the Editor by Mr.* AMOS EATON, *Lecturer on Geology, Botany, &c.*

To Professor Silliman.

AFTER a laborious geological excursion along M'Clure's Springfield section, for about one hundred miles, I visited Dr. D. Hunt, at Northampton. He observed that you had expressed an opinion, that an attentive examination of all the strata constituting the walls of the artificial avenue or drift at the Southampton mines, would bring facts to knowledge, which might, in some degree, subserve the cause of geological science. I am now at the mouth of the drift, having just completed the labour which you had marked out.

I employed two miners to commence with me, at the termination of the drift, which is now extended 800 feet into the hill. We broke off large specimens, at very short intervals, throughout the whole extent of the drift. We arrived at its mouth with almost a boat load of specimens. I kept a memorandum of every thing which occurred, while under ground; and I have now arranged the specimens, before the mouth of the drift, in the same order in which they were situated in the earth.

Fatigued as I am, I will make my remarks here, in the field, lest something should hereafter escape me, which is now fresh in my recollection. Beginning with the greatest distance to which the miners have penetrated, I will set down my remarks, in fact, in reversed order.

800 feet. The rock is fine-grained gray granite, traversed by veins, lined with quartz crystals, and mostly filled with calcareous spar, often beautifully crystallized. In the same veins blue and purple fluate of lime and copper pyrites frequently occur.

790 feet. The same fine-grained granite is continued, occasionally traversed by veins lined with crystals of quartz; but containing no other minerals.

774 feet. A narrow vein of sulphuret of lead, with walls lined with crystals of quartz. The fairest cubic crystals are slightly attached to the points of the quartz crystals. Yellowish crystals of carbonate of lime are often interspersed among the lead. Sulphate of barytes occurs here also; sometimes in plates meeting at various angles, and forming chambers lined with minute crystals of quartz. Minute crystals of copper pyrites and a little fluate of lime have been found here; also fine specimens of bitter spar. The walls are very compact, fine-grained granite.

760 feet. Coarse, parti-coloured granite. The felspar is flesh-coloured and white; the quartz often bluish or greenish; the mica silvery, greenish, or purplish.

725 feet. A stratum of gray-wacke slate. Texture less firm than of the same rock at the west of Pittsfield. This stratum is very distinct, and about two feet thick.

723 feet. A stratum of serpentine rock, containing very red quartz imbedded in various directions. It is very compact, and mostly green. Here it is but about three feet thick. About ten miles south of this place, on Maclure's Springfield section, near the line between Westfield and Russel, and four miles west from Westfield Academy, I found this same stratum of very great breadth. I say the same stratum, because it is situated in the granitic hill, east of the highest ridge of granite, which is evidently a continuation of this range. Perhaps I may, hereafter, give you an account of my excursion along that section of Maclure, in which I may give you a more particular description of the Westfield serpentine.

720 feet. Coarse granite, with white and flesh-coloured felspar, black and silvery mica.

700 feet. A stratum of red mica slate, about four feet thick.

694 feet. Coarse, flesh-coloured granite. This is the handsomest granite in the whole drift. Here we find the most beautiful specimens of graphic granite, both flesh-coloured and gray.

680 feet. A stratum of Kirwan's stell-stein. That is, an aggregate of fine-grained quartz and mica, without any felspar. The quartz is mostly greenish, probably coloured by the next stratum.

670 feet. Beautiful green soapstone. Very compact, but rather softer than that kind in common use for inkstands.

666 feet. A green, granular aggregate. It seems to be made up of fine fragments of quartz, soapstone, and mica, rarely a little felspar, slightly compacted together.

Remark. All the strata, from the inner termination of the drift to this place, a distance of one hundred and thirty-four feet, are nearly vertical, or a very little inclined. Here they begin to approach a horizontal position.

The green aggregate continues as far as the air-well, a distance of 66 feet, with some trifling variations in the size and proportions of the aggregated fragments.

500 feet. A granulated, schistose aggregate, chiefly of quartz and mica. Though the constituents and the form of the rock correspond very nearly with mica slate, it cannot be considered as the primitive mica slate rock. It is so slightly compacted that it can scarcely be kept from falling to pieces. Its position is nearly horizontal.

480 feet. A stratum of coal, half an inch thick. This stratum may be traced, at different intervals, one hundred and eighty feet along the drift towards its mouth. It lies between the strata of the last described schistose aggregate.

400 feet. An aggregate appears, alternating with the loose schistose rock, which resembles the red sandstone, but is of a less firm texture.

From this place all the strata, east of the soapstone, occasionally appear, for the distance of about three huudred feet. This is probably on account of their undulatory forms and horizontal position. Most of the way we find the lower part

of the walls to consist of a kind of semi-indurated pudding-stone. Sometimes a thin stratum of fine, loose sand occurs. At 300 feet the coal stratum disappears, passing below the bottom of the drift.

The last hundred feet is chiefly gravel, which is now supported by timbers.

Southampton, Aug. 26, 1818.

ART. VII. *On the Peat of Dutchess County—read before the Lyceum of Natural History, in New-York, by the* Rev. F. C. SCHAEFFER, *of New-York, and by him communicated to the Editor.*

IN May, 1817, I brought specimens of marl and *peat* from Dutchess county, which were taken from a fen or bog occupying an area of some acres. These fens occur frequently in the towns of Rhinebeck, Northeast, Clinton, &c. in Dutchess county. During a part of the year they are covered with water.

A pit was dug in the bog from which I procured the specimens. The order and depth of the well-defined strata which were exhibited by this excavation, I noted in my memorandum book, from which I extract the following:

After clearing away the fresh sod and recent vegetable mould, there appeared,

1. A stratum or bed of *peat*, commonly called *turf*, varying in depth from three to four feet.
2. A stratum of peat and marl commingled; depth two feet.
3. A stratum of pure marl, from two to three feet. Below these there was an appearance of sand and blue clay.

The first, or upper stratum, consists of *compact peat.* This substance, when first taken up, is of a dark brown colour, soft, and rather viscid. Some vegetable fibres and vacuous seeds are distributed throughout the mass. It may be moulded to any convenient form. When perfectly dry, the texture of

this variety, of which there is a specimen before you, acquires a high degree of solidity. Its fracture is earthy; the colour is lighter.

I should not have offered more on this subject than the labelled specimen, had I not made a most satisfactory experiment with this kind of fuel, which may be obtained in great abundance in our own State. It is easily kindled; burns with a bright flame; yields a bluish smoke, and produces an odour similar to that which attends the combustion of gramineous substances. But this is momentary. When thoroughly kindled, it burns with less flame, yields a small proportion of blackish smoke, and sulphurous acid gas is evolved, though I cannot discover any pyrites. It burns for a long time, and emits a great body of heat. It leaves a very small proportion of light, grayish white ashes; on which I have as yet made no experiments, having this day, for the first time, paid particular attention to this substance, attracted by the unusual hardness which it acquired since it is in my possession: and not many hours have elapsed since I subjected it to combustion. The attempt succeeded so well, that I cannot refrain from expressing my opinion, that this variety of peat will answer as an excellent substitute for the best Liverpool coal.

Art. VIII. *Notices of Geology in the West-Indies.*

REMARKS.

In the former Number of this work, a notice was published respecting siliceous petrifactions of wood, from Antigua. We now publish a geological sketch of the island, with notices of some other parts of the West Indies. This communication is made by a friend, with permission to publish it. It is a production of the pen of Dr. Nugent, of St. Johns, Antigua, a gentleman of eminent scientific acquirements, who, it is hoped, will continue his laudable and able efforts to illustrate the natural history of the West-Indies.

Memorandum concerning the Geology of Antigua, &c.

THE southern and more mountainous part of the island consists of *trap* rocks; more particularly of trap breccia and wacké-porphyry. On these beds rests a series of very peculiar stratified conglomerate rocks. These strata vary exceedingly in colour and thickness, but all dip, at a considerable angle, to the northwest. The more usual character of this rock, is that of a clayey basis, with minute particles of felspar, and small spots of *grünerde** (or chlorite Baldogée.) This latter is frequently diffused over the whole, and gives it a green tinge: the colour has been thought by some to proceed from the impregnation of copper, but I am rather of opinion that is owing to manganese and iron. The conglomerate character of this rock, is derived from its having imbedded in it, or incorporated with it, numerous fragments, of all sizes, of petrified wood, chert, with and without coralline impressions, agate, jasper, amygdaloid, greenstone, hornstone, porphyry, porphyry slate, and other substances.

On this singular class of strata, reposes an extensive calcareous formation, occupying the northern and eastern part of the island, having *subordinate* to it, and at its lowest part, where it is in contact with the conglomerate, large beds and patches of chert, which contains also a vast variety of petrified woods, several of which are of the palm tribe, with silicified shells, chiefly *cerithea;* though at the Church-hill, at St. Johns, formed of this chert, casts of bivalve and ramose madrepores are likewise found. The calcareous beds are principally of a friable marl, with blocks and layers of limestone irregularly included. In this *formation*† are many fossil shells, both in the calcareous and siliceous state; and there appear to be some beds, wherein is a mixture of shells of marine, and others of a fresh water, or at least a terrestrial origin. The coralline agates found in nodules and patches therein, and which may readily be dis-

* The green earth of most mineralogists. EDITOR.

† *Formation*—a geological phrase, of German origin.

tinguished from the coralline chert of the previous beds, are the most beautiful which have any where been yet noticed; and when well selected and polished, make very pleasing ornaments.

The island, as well as Barbuda, thirty miles to the northward, the Grande Terre part of Guadaloupe, at a similar distance to the southward and eastward, with several others of the West-India Islands, give proof of an extensive formation, more recent than those to which naturalists have heretofore principally confined their attention; and which is, perhaps, contemporaneous with, if not later than, the Paris Basin, so well described by Cuvier and Brongniart.

April 10*th*, 1818. N. N.

N. B. A few specimens are sent.

REMARKS.

If the above paper be read attentively, in connexion with that in No. I. on the petrified wood of Antigua, it will afford some very curious information to the geologist respecting these petrifactions, and must lead to interesting speculations respecting their origin, under circumstances so very peculiar, and to which we do not recollect to have heard of any parallel.

ART. IX. *Discovery of Native Crystallized Carbonate of Magnesia on Staten-Island, with a Notice of the Geology and Mineralogy of that Island, by* JAMES PIERCE, ESQ. *of New-York, in a Letter to the Editor.*

NEW-YORK, October 19, 1818.

DEAR SIR,

I FORWARD you a few mineral specimens characteristic of Staten-Island, including native carbonate of magnesia, in acicular crystals. I discovered this new form and locality of mag-

nesia in examining the strata exhibited in an excavation now making, under the delusive expectation of finding gold, about three miles from the Quarantine. In descending the shaft, sunk perpendicularly in steatite, magnesite, veins of talc, and green translucent asbestus were observed at depths from six to thirty-five feet. The magnesite was found to embrace veins and cavities containing native carbonate of magnesia, in very white acicular crystals, grouped in minute fibres radiating from the sides, but not always filling the veins and cavities. The crystals were, in some instances, suspended, assuming a stalactical form. This carbonate of magnesia dissolves entirely in diluted sulphuric acid, with considerable effervescence and chemical action, producing a bitter compound, from which salts of easy solution are formed by evaporation. The magnesite in which these crystals are found, appears to be composed of carbonate of magnesia, steatite, and talc, disintegrating readily upon exposure to air and moisture : it effervesces considerably in sulphuric acid, forming a very bitter fluid that soon exhibits crystals, indicating that magnesia enters in large proportion into its constitution. Magnesite may perhaps be found at this place in quantity sufficient for a successful manufacture of Epsom salts. Small regular hexaedral crystals of mica, were noticed in steatite. Chromate of iron was sparingly diffused through the different minerals raised from various depths.

A few remarks and facts respecting the geology and mineralogy of Staten-Island, may, perhaps, give some additional interest to the specimens presented.

Staten-Island (which constitutes Richmond county) is situated about seven miles southwest of the city of New-York, extends from northeast to southwest about fifteen miles, in a straight line, with an average width of six. It exhibits a considerable diversity of surface. The eastern part is composed principally of elevated ground : a mountain chain is observed to take its rise in the vicinity of a narrow sound called the Kills, and sweep, in a semicircular form, near the eastern shore ; it then ranges southwest, parallel with, and distant from Amboy Bay, about two miles, terminating near the centre of the island, and forming, with the exception of some passages, a

continued chain, which, on the eastern and southern sides, is very steep, but not precipitous; it gradually declines to the west and north, and, in some places, it presents on its summit table land of considerable extent. A prominent ridge crosses the island, connecting the elevated ground of the south, with the hills of the northern part. A species of steatite, containing veins of common, indurated, and scaly talc, amianthus, and most of the varieties of asbestus, and some chromate of iron, constitutes the nucleus of the whole mountain range and elevated ground of the eastern division, stamping it as primitive. This steatite approaches, in most places, within a foot and a half of the surface, and appears in small angular loose blocks, wherever the soil has been removed. Its colour is a greenish yellow; it is brittle, very adhesive to the tongue, but little unctuous, and probably contains more alumine and less magnesia than steatites in general. Much of it decomposes when exposed to air and moisture, and forms a good mould, whenever the descent of ground permits an accumulation of earth. It is not improbable, that in most places of the Staten-Island hills, when magnesia constitutes a considerable ingredient of the rock, it will be found saturated with carbonic acid, obviating the objection to common magnesian minerals in agriculture.

The minerals observed on the surface of the northeast part of this chain of hills are, secondary greenstone, asbestoid, sandstone, granite, and gneiss, sparingly scattered in rolled masses. In addition to these rocks, in the middle and western part of the chain, a mineral of uncommon appearance is observed. It is composed principally of quartz, rough, with numerous cells of various forms, in which small siliceous crystals are generally found: the veins or plates of quartz that intersect each other, often embrace talc and oxide of iron, which, decomposing, gives some specimens the appearance of volcanic origin. Associated with this cellular ferruginous quartz, brown hæmatite is often observed; this valuable ore often yields eighty per cent. of iron of best quality; its fibres assume a variety of shapes; they were observed at Staten-Island, straight and curved, radiating from a centre, and exhi-

biting the stalactical, cylindrical, and botryoidal forms, often displaying a black polished surface and glistening lustre. Ferruginous minerals are abundant on the mountain for several miles. A granular oxide, called by miners shot-ore,* from its being principally composed of spherical grains of various sizes, was often noticed, and appears in some places in extensive beds: it is easily fused, and affords a large per centage of good iron for castings. A heavy ore, with a smooth surface and some lustre, bearing a considerable resemblance to native iron, is sometimes seen. Banks of white sand, resembling the siliceous particles of the seashore, are noticed on the mountain tops, containing masses of compact, heavy ferruginous sandstone, similar to the rocks of our alluvial seaboard. Large beds of water-worn siliceous pebbles, in no way differing from those washed by the ocean, are seen on the height of the ridge, in which excavations have been made several feet, leaving the depth of the mass uncertain. On some of the eminences, for a considerable extent, vegetation is entirely excluded by an iron-bound soil. Iron ore, imbedded in an earth coloured by, and partly composed of, oxide of iron, occupies the surface; and chalcedony and radiated quartz are sometimes observed on the primitive ridge. Prospects from many of these eminences are extensive and diversified. On one side, the ocean and a great extent of coast are in view; on the other, a rich landscape of hills and plains, the eye resting on the highland-chain and the mountains bordering Pennsylvania; the harbour, at your feet, presents a busy, ever-varying scene, and the city of New-York appears to great advantage from this point of observation.

The district between the mountain and the narrows, the thickly settled and well-cultivated plain bordering Amboy bay, and much of the western division of the island, are decidedly alluvial. Adjacent to Fort Tompkins, detached pieces of copper ore have been found. I have observed petrifactions of marine shells in rocks excavated in that neighbourhood, twenty feet from the surface, and sixty above the ocean.

* Doubtless the pea ore of the Wernerians. EDITOR.

The western part of the island presents moderate elevations; the soil, a good medium of sand and clay, is in general fertile; but a tract near the termination is sandy and barren. Some creeks penetrate to near the centre of the island, and are bordered by extensive salt meadows. Except at the primitive range, I have observed in no part of the island large beds of rock that can be called in place; but rolled masses of greenstone, sandstone, gneiss, granite, red jasper, and indurated clay, appear in general sparingly, but sometimes in abundance, on the surface. Lignite has been found in small quantities in the western part of the island. A chalybeate spring, of no great strength, is the only mineral water met with in Richmond county. The ponds, wells, and streams, contain a soft water, holding no lime in solution.

REMARKS.

We have already published (p. 54.) Mr. Pierce's discovery of the pulverulent carbonate of magnesia, and have pointed out its connexion with Dr. Bruce's previous discovery of the hydrate of magnesia, or pure magnesia combined with water only. Mr. Pierce has now added another important link to this chain, and future mineralogists may quote the vicinity of New-York as affording,

1. Pure magnesia, crystallized and combined with water only.
2. Carbonate of magnesia, pulverulent and white.
3. Carbonate of magnesia, in very delicate and perfectly white acicular crystals.

We possess specimens of them all.

ART. X. *On a curious substance which accompanies the native Nitre of Kentucky and of Africa. Communicated in a letter to the Editor, from* SAMUEL BROWN, M.D. *late of Kentucky, now of the Alabama Territory.*

REMARKS.

THE scientific public were several years ago laid under obligations to Dr. Brown, for a very interesting and instruc-

tive account of the nitre caverns, &c. of Kentucky, published in the Transactions of the Philosophical Society, in Philadelphia, Vol. VI., and in Bruce's Journal, Vol. I. p. 100. The following communication arose from a conversation on that subject between Dr. Brown and the Editor.

New-Haven, July 27, 1818.

DEAR SIR,

I have just found the passage I referred to the other day, relative to the existence of native or sandrock nitre in the interior of Southern Africa. It is in Barrow, and not in Vaillant, as I thought when I had the pleasure of conversing with you concerning it. I am much obliged to you for recalling my attention to that curious subject, as it has brought to my recollection a fact, which I believe I omitted to mention in my memoir, (viz.) the existence of a black substance in the clay under the rocks, of a bituminous appearance and smell. This I remember to have seen in a rock-house, near the Kentucky river, where very considerable quantitites of sandrock nitre had been obtained. This substance was found in masses of a few ounces weight, and in the crevices of the rocks near the basis of the side walls. The smell was not wholly bituminous, but resembled that of bitumen combined with musk. I am quite unable to account for the formation of the nitre, or the production of this black substance which sometimes accompanies it, both in Africa and America. Had I seen Mr. Barrow's travels, when I noticed the bitumen, I should certainly have paid more attention to it. But perceiving no relation between the rock nitre and the masses of this substance, my examination of it was much too superficial. I do not very well understand what Mr. Barrow means by saying, that many wagon loads of animal matter lay on the *roof* of the caverns in Africa. I saw no such matter on the *roof* of the rock-houses in Kentucky. Certainly the caverns have been the habitations of wild beasts, and great quantities of leaves, &c. have been mixed with the debris of the superincumbent rocks, but it does not seem probable, that much animal matter could be filtrated through a roof of rock, perhaps forty or fifty feet

in thickness. The subject, however, is very curious, and deserves much more attention than any of us have bestowed upon it.

Extract from Barrow's Southern Africa, p. 291. New-York edition.

"About 12 miles to the eastward of the wells, (Hepatic wells,) in a kloof of the mountain, we found a considerable quantity of *native nitre*. It was in a cavern similar to those used by the Bosgesmans for their winter habitations, and in which they used to make the drawings above mentioned. The *under surface* of the projecting stratum of calcareous stone, and the sides that supported it, were incrusted with a coating of *clear, white saltpetre*, that came off in flakes, from a quarter of an inch to an inch or more in thickness. The fracture resembled that of refined sugar, it burnt completely away without leaving any residuum; and if dissolved in water, and thus evaporated, crystals of *pure prismatic nitre* were obtained. This salt, in the *same* state, is to be met with *under* the sandstone strata of *many* of the mountains of Africa; but, perhaps, not in sufficient quantities to be employed as an article of export. There was also in the same cave, running down the sides of the rock, a black substance, that was apparently bituminous. The peasants called it the urine of the das. The dung of this gregarious animal was lying upon the roof of the cavern to the amount of many wagon loads. The putrid animal matter, filtrating through the rock, contributed, no doubt, to the formation of the nitre. The Hepatic wells and the native nitre rocks were in the division of Agster Sneuwberg, which joins the Tacka to the southwest."

Should I ever visit Kentucky again, I hope that I shall be able to give a better account of these caverns, which certainly are highly deserving of the attention of naturalists.

In Philadelphia you may have an opportunity of seeing some small specimens of the sandrock, containing nitre, now in the cabinet of the Philosophical Society.

BOTANY.

ART. XI. *Descriptions of species of Sponges observed on the shores of Long-Island. By* C. S. RAFINESQUE, *Esq.*

THE sponges are one of the most singular productions of nature; and, even to this time, naturalists are divided in opinion respecting their real rank in the scale of organized beings. Some believe that they are animals, belonging to the class of polyps, next to the genus of *alcyonium*, while many contend that they are not animals, but plants, of the tribe of *fuci*, or marine vegetables. I am inclined to adopt this latter opinion, since in all those which I have seen, in Europe and America, no perceptible motion nor sensibility was to be discerned in any stage of their existence; and those who have acknowledged their animality, bring no stronger proof thereof than an occasional slight shrinking under the hand, and an animal smell, which are common to some marine plants.

Whatever be the truth on the subject, these doubtful opinions prove that they are of the many connecting links between animals and plants. This is is not a proper place to decide this controversy; I mean merely to make known new species of this tribe of beings, which I observed last year, on the shores of Long-Island. Such a fragment will be, perhaps, the first attempt of the kind; when more species shall be known, the subject may be investigated with more certainty and accuracy.

1. *Spongia albescens*, Raf. (Whitish sponge.) Effuse, compressed, irregular, perforated, somewhat branched, unequally lobed, whitish, smooth; lobes truncated; cells porose, very minute, nearly equal; small unequal cells inside.

Found near Bath and Gravesend, in sandy bottoms. A large species, sometimes over a foot broad, of quite an irregular shape, rather flattened, about one inch thick; partly

gibbose ; concave now and then, and with large, irregular openings, as if large branches were anastomosed ; circumference branched or lobed, very jagged, sinus obtuse, lobes elongated obtuse, truncate or flat, unequally divided. The substance is entirely of a cinereous white, outside and inside, of a soft and brittle nature, rather friable ; covered outside with minute pores of an oblong or round shape, and full of small unequal cells inside.

2. *Spongia ostracina*, Raf. (Oyster sponge.) Very branched, erect, red, papillose ; branches unequal, often dichotome, obtuse ; cells porose, oblong, nearly equal.

It is often found on the common oyster. (*Ostrea virginica.*) It rises from four to six inches, the colour is a fine red, it branches from the base ; the branches are unequal, straight, cylindrical, or compressed. Substance stupose. Surface covered with small papilla and small oblong unequal pores.

3. *Spongia cespitosa*, Raf. (Bushy sponge.) Branched, cespitose, yellowish, rough, papillose ; branches fasciculated, upright, unequal, flexuose, compressed, slightly anastomosed, nearly dichotome upwards ; cells porose, oblong, nearly equal, margin lacerated.

Found also on the oyster, but more seldom than the foregoing ; the specimens which I saw was found on the Bluepoint oysters, by Dr. Eddy. It becomes brown by drying. It rises from four to six inches, the margin of the cells or pores is torn into papillar, stiff processes, which produce a rough surface. Substance stripose. Internal cells oblong, very small.

4. *Spongia cladonia.* (Cladonian sponge.) Branched effuse, smooth, pale fulvous, stem procumbent, branches distichal, one-sided, erect, simple or divided, obtuse ; cells porose, minute ; some larger round.

I have found this species at Bath and at Sandy-Hook, on sandy bottoms. Length about six inches. Stem and branches cylindrical or compressed. Substance fibrose, anastomed, branches divaricate, ascendent, semi dichotomose or simple, unequal, thicker towards the top.

5. *Spongia virgata.* (Slender sponge.) Nearly branched, smooth, fulvous, stem divided, slender, cylindrical, knobby,

branches erect, slender, nearly heads acute ; pores unequal, irregular, small.

A small species, three inches high, found at Oysterbay, on rocky bottoms, rare ; stem with few branches, and imperfect ones, like knobs. Substance stupose. Branches round, alternate, small. Pores without any determinate shape.

ART. XII. *Memoir on the Xanthium maculatum, a New Species from the State of New-York, &c. by* C. S. RAFINESQUE, ESQ.

PURSH and Michaux mention only one species of American *Xanthium*, the *X. strumarium*, while there are three noticed in the catalogue of Dr. Muhlenberg, the above species, and the *X. orientale*, and *X. spinosum*. The first and the last are natives of Europe, and have been naturalized in the United States, with many other plants. The species called *X. orientale* by Dr. Muhlenberg, appears however to be a native ; but the *X. orientale* of Linnæus, is a native of Siberia, Japan, and the East-Indies ; and when plants are found to grow in such opposite quarters of the globe, a strong presumption arises that they are not identical species, which presumption has been confirmed by experience in many instances, whenever the plants of both countries have been accurately examined. Decandolle, in the French Flora, (2d edit. of 1815.) vol. 6. p. 356. describes, under the name of *X. macrocarpon*, a species found in France, and which he takes to be the real *X. orientale* of Linnæus. He has changed its name, because, he says, that it is not certain that the *X. orientale* grows in Asia ; or, if any grows there, that it is identic with his species ; which, however, is really the *X. orientale* of Linnæus, Son, Lamark, and Gaertner. He adds, that he possesses in his herbarium, a species from Canada, different from his *X. macrocarpon* which has been figured by Morison, on whose authority some authors have asserted that the *X. orientale* grew in Canada, mistaking his figure for that plant.

From the above statement, it appears that much obscurity and difficulty arises in botany, when errors creep into the distinction of species: to detect those errors, and to ascertain the synonyme of obscure species, is not one of the least useful botanical labours. Having found, last year and this year, in the neighbourhood of New-York, a species of *Xanthium* different from any described by the authors, and intermediate between the *X. strumarium* and *X. orientale* of Linnæus, I presume that it may be the *X. orientale* of Muhlenberg, Leconte, and Morison, and the *Xanthium* of Canada, mentioned by Decandolle, Dumont, &c. I have given to it the name of *X. maculatum*, since the stem is spotted like the *Conium maculatum*. None of those authors having described it, I suppose that its description will be acceptable, and will serve to fix this new species among the American botanists.

Therefore it will appear, that the *X. orientale*, which had been considered as a native of Asia, Europe, and America, is composed of at least three species; the European species, which has been called *X. macrocarpon* by Decandolle, the American species, which I have called *X. maculatum*, and the Asiatic species, to which the name of *X. orientale* ought to remain; but which ought to be better described, and more fully distinguished from the *X. macrocarpon* by those who may chance to meet with it. I even suspect that many species grow in Asia, since that of Ceylon may be different from the Chinese and Siberian species.

Xanthium Maculatum.

Definition. Stem flexuous, round, rough, spotted with black; leaves long-petiolate, cuneate-reniform, nearly trilobe, sinuate-toothed, obtuse, rough, and thick; fruits elliptic, obtuse muricate; thorns rough.

Description. The root is annual, thick, and white. The stem rises from one to two feet; it is upright, without thorns, very thick, and with few branches; it is covered with oblong, black, and rough spots. The leaves are few, but large, with very long petiols; they are nearly reniform, with an acute base, and have three nerves; the teeth are unequal,

large, and obtuse. The flowers and fruits are disposed as in *X. strumarium;* but the fruits are generally solitary; they are half an inch long, nearly cylindrical obtuse, with the two beaks scarcely perceptible and bent in, covered with short, thick, and rough thorns, rather soft, and not uncinate. The whole plant has a peculiar smell, not unpleasant, somewhat between the camphorate and gravulent odour, but weaker than in *Conysa camphorata*, &c.

History. This plant grows on Long-Island, near the sea-shore and marshes. I have found it common near Bath, on the downs, and in New-Jersey, near Bergen, and Powles-Hook, on the margin of marshy meadows. According to Dr. Mulenberg, it grows also in Pennsylvania; Messrs. Torrey and Leconte found it on the island of New-York; and by Morison and Decandolle's account, it is found as far north as Canada. It blossoms in August and September, but the fruits remain on the plant till the severe frosts of December.

Observations. This species differs from the *X. macrocarpon* of Decandolle, by having smaller fruits, without horns, and whose thorns are neither hooked nor hispid; by not having an angular stem, but a round, spotted one, and by its leaves being broader, and not serrate, &c. Nearly all those differences exist between it and the *X. orientale* of Asia, which has not yet been isolated from the *X. macrocarpon.* The *X. edrinatum* differs from this by having oval fruits, with aggregated, echinate, and hooked thorns; and the *X. strumarium*, by having cordate hirsute leaves, the fruits aggregated, with hooked thorns and horned tops. The *X. spinosum*, and *X. fruticosum*, are so totally different that they need not be compared.

ZOOLOGY.

ART. XIII. *Description of the Phalaena Devastator, (the Insect that produces the Cut-worm,) communicated for the American Journal of Science, &c. by* Mr. JOHN P. BRACE, *of Litchfield, Conn.*

THIS moth, whose larva is one of our most destructive enemies, belongs to the Linnæan family noctua, in the genus phalaena. Its specific characters are as follow: Wings incumbent and horizontal, when at rest; body long and thin; thorax thick, but not crested; head small; eyes prominent and black; antennæ setacious, gradually lessening towards extremities, and slightly ciliated; palpi two, flat, broad in the middle, and very hairy; tongue rolled up between them, not very prominent; clypeus small, legs long, small and hairy; wings long as body; under wings shortest; colour a dark silvery gray, with transverse dotted bands of black on upper wings. The insect lays its eggs in the commencement of autumn, at the roots of trees and near the ground: they are hatched early in May. The habits of the cut-worm have been often and fully detailed. They eat almost all kinds of vegetables, preferring beans, cabbages, and corn. They continue in this state about four weeks; they then cast their skin and enter the *pupa* state, under ground. This is a crustaceous covering, fitted to the parts of the future insect. In this they continue for four weeks longer, and come out in the fly, or insect state, about the middle of July. All those chrysalids that I exposed to the sun, died; and all those that were kept cool under earth, produced an insect: hence I infer, that the heat of the sun will kill the chrysalids. If, then, the ground be ploughed about the first of July, many of those insects might be destroyed, and the destruction of the productions of the next year prevented; for the *pupa* is never more than a few inches under ground.

The phialaena devastator is never seen during the day; it conceals itself in the crevices of buildings, and beneath the bark of trees. About sun-down it leaves its hiding-place, is constantly on the wing, and very troublesome about the candles in houses. It flies very rapidly, and is not easily taken.

Such is the description of this formidable enemy to vegetation. No efficacious method has yet been taken to prevent its ravages, but the one who could accomplish it, would do the cause of agriculture an essential service.

ART. XIV. *Description of a New Genus of North American Fresh-water Fish, Exoglossum, by* C. S. RAFINESQUE, ESQ.

MR. LESUEUR has published, in the 5th Number of the Journal of the Academy of Sciences of Philadelphia, for September, 1817, the description of a new fish, which he calls *Cyprinus maxillingua:* he considers it as a very singular and anomalous species, owing to the peculiar structure of its lobed lower jaw and tongue, which is external, and situated as an appendage to the former. It was discovered in Pipe-creek, Maryland, in June, 1816, by said author, who confesses that he does not consider it as properly belonging to the genus *Cyprinus*, and presumes that when other species shall be discovered, possessing the same character, they will constitute a separate genus. Although this principle and presumption is correct, it was wrong to delay the formation of such a distinct genus, because only a species was then known, since so many genera are composed of single species. However, Mr. Lesueur's expectation was verified even before he wrote it, since in May, 1817, I had discovered in the Fishkill, State of New-York, another species, evidently congenerous with the *Cyprinus maxillingua*, having the same structure of the mouth, &c. I therefore venture to establish a separate genus for those two species, having no doubt that many more will hereafter be added to it by accurate observers, and I give to it the name of

Exoglossum, meaning *outside tongue*. It will belong to the same natural order and family of the genera *Cyprinus*, *Catostomus*, &c.

Exoglossum. *Generic Definition.*—Body oblong, thick, and scaly; head without scales, mouth without lips or teeth, upper jaw longer, entire; the lower trilobed, middle lobe longer, performing the office of tongue; dorsal fin opposite to the abdominal fins; three rays to the branchial membrane.

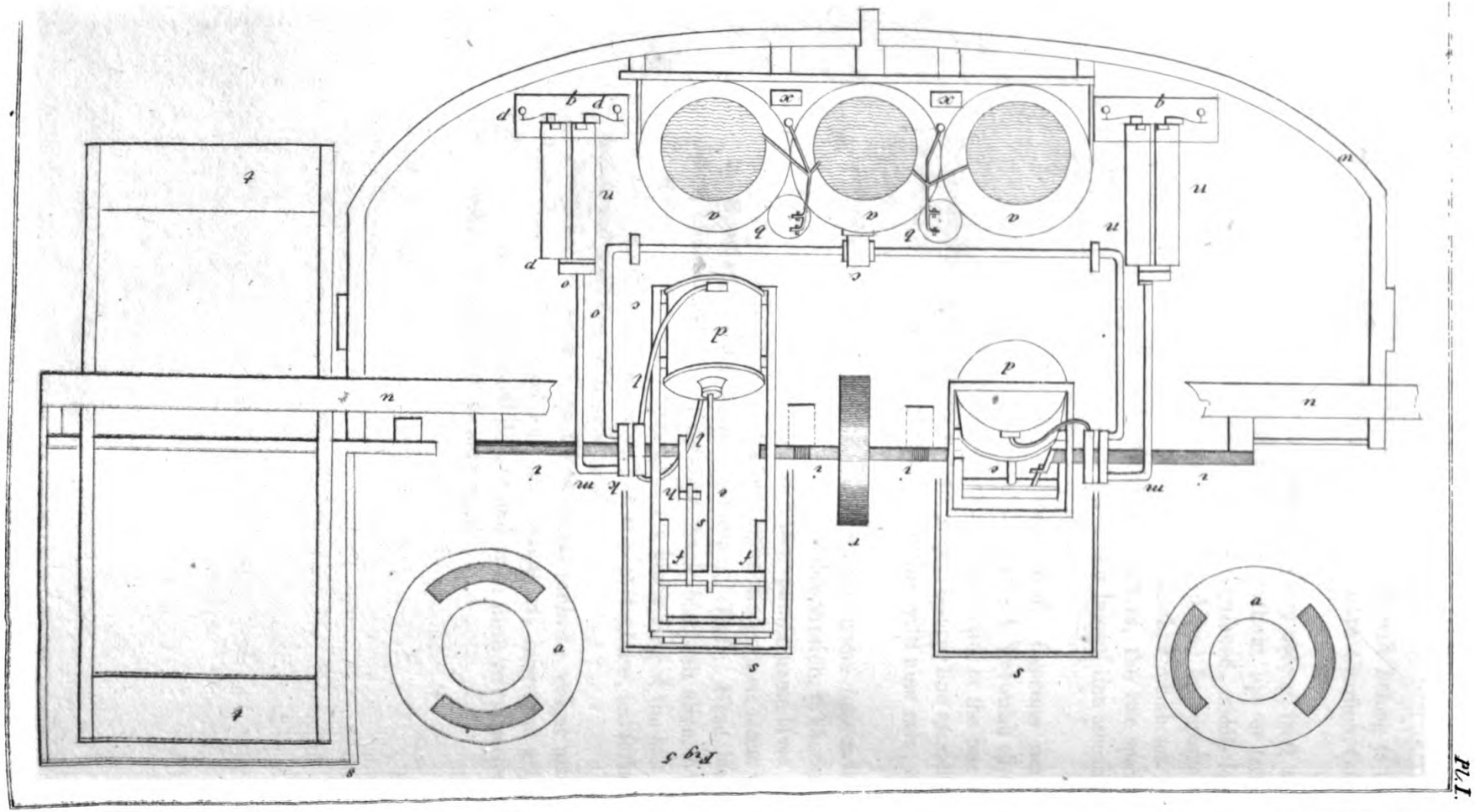
Remarks. Besides the above characters, the two species known at present have, in common, the lateral line ascending upwards at the base, the tail forked, &c.

1. Species. *Exoglossum vittatum*, Raf. *Cyprinus maxillingua*, Lesueur. *Specific Definition.*—Back brownish olive; sides blue, with a brownish band; a black spot at the base of the caudal fin, lower parts silvery gray; lateral line ascending upwards at the base; dorsal and anal fins with nine rays; tail forked.

Remarks. Length four inches; vulgar name *little sucker*. For further particulars, see Lesueur's description, p. 85. cum. ic. I have been obliged to change the specific name of *maxillingua*, since it has the same meaning as the generic name.

2. Species. *Exoglossum annulatum*, Raf. Head black above, cheeks and gills olivaceous, back blackish olive, sides olivaceous, lower parts olive gray; a black ring at the base of the tail; lateral line ascending upwards at the base, tail forked, dorsal and anal fins with nine rays.

Remarks. Length from three to six inches; vulgar name, *Black chub*. Head broad and flat above, iris large and gray; fins olivaceous, abdominal distant and with nine rays, pectoral with fifteen, caudal with twenty-four.



Pl. I

Fig 8

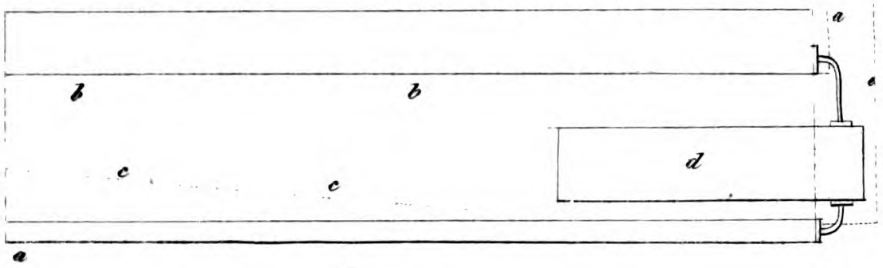


Fig 7

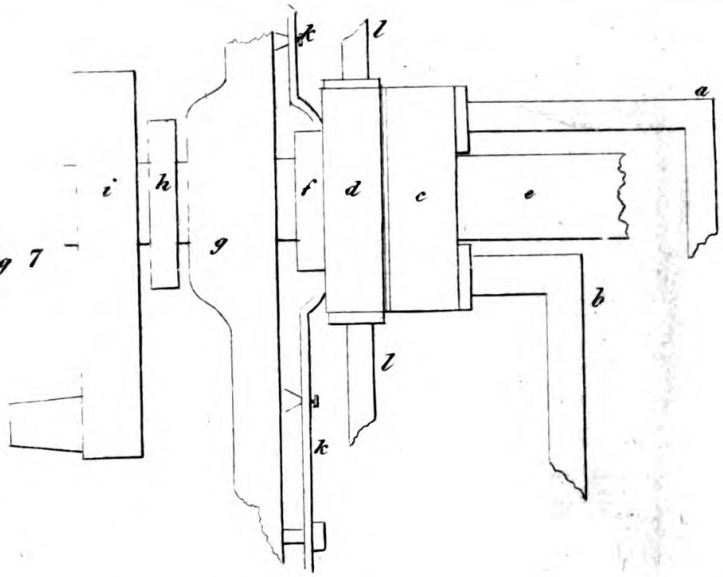
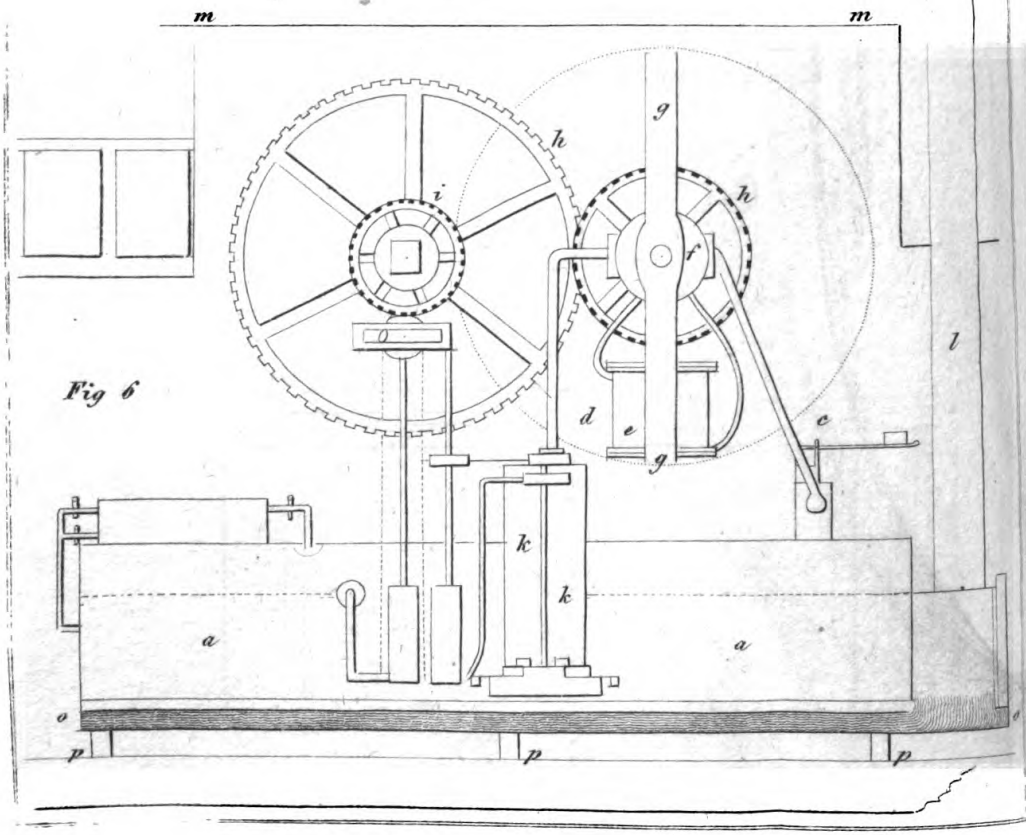


Fig 6



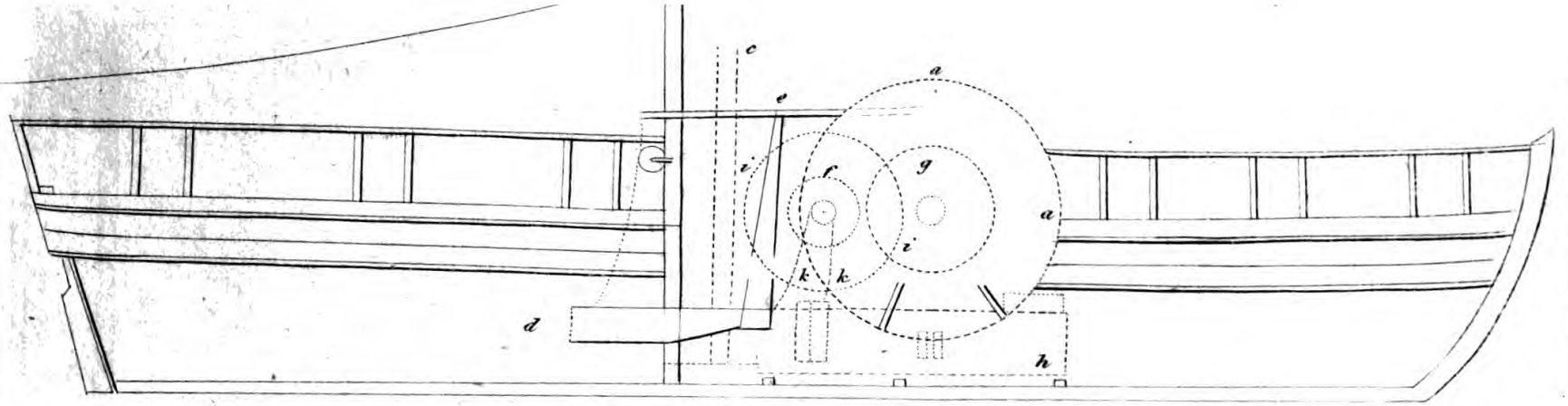


Fig 2

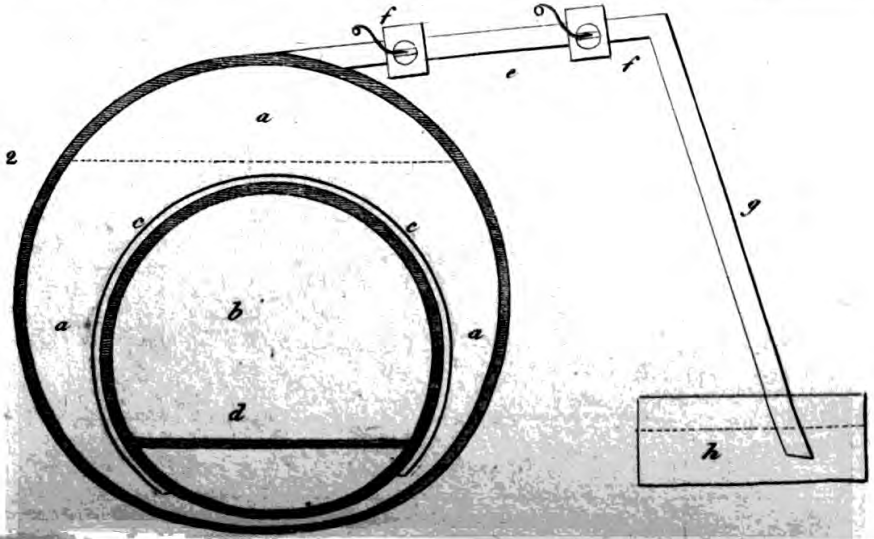
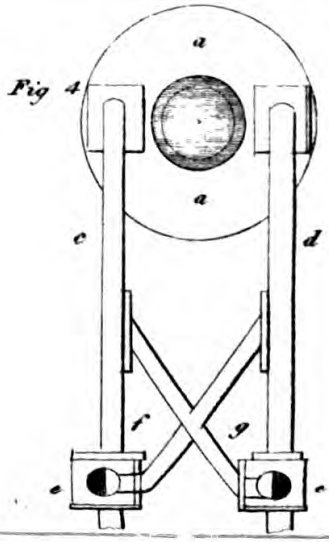


Fig 4



Pl. III.

P.IV.

Fig 3

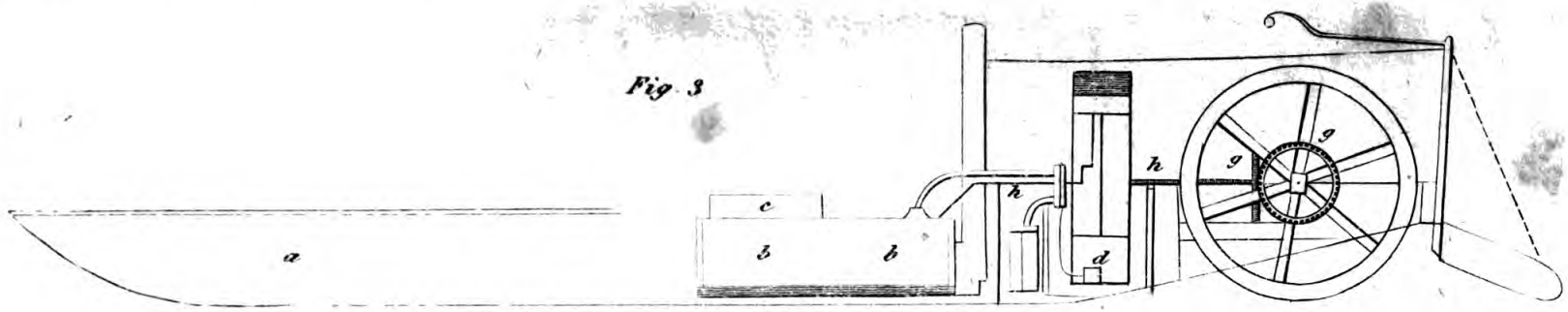
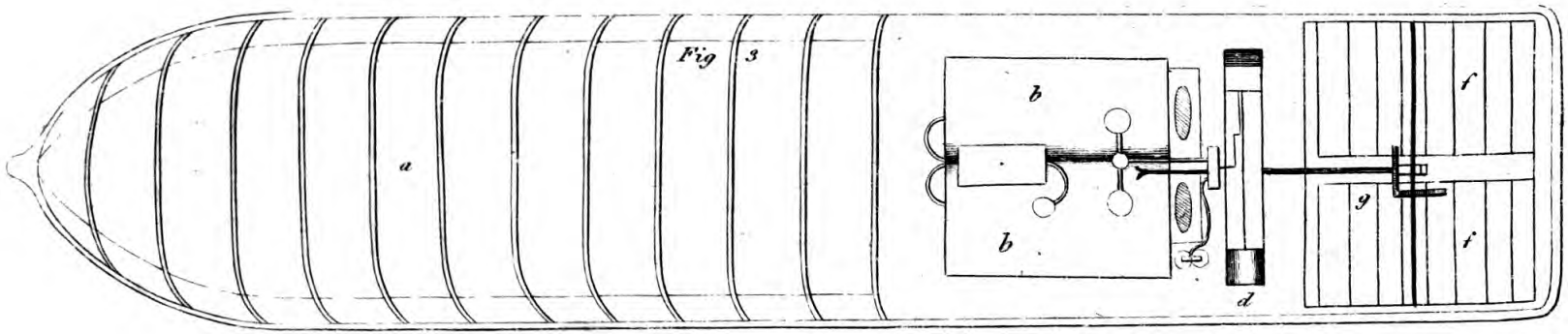


Fig 3



PHYSICS, MECHANICS, AND CHEMISTRY.

Art. XV. *On the Revolving Steam-Engine, recently invented by* Samuel Morey, *and Patented to him on the 14th July,* 1815, *with four Engravings.*

To Professor Silliman.

Sir,

THE successful employment of the steam-engine, in navigating the rivers and inland waters of the United States, and the probable extension of this mode of conveyance of persons and property, makes those improvements desirable which adapt the steam-engine to this purpose with less complication and expense, placing it more within reach of individual enterprise, and rendering it even useful on our small rivers and canals.

The steam-engine, though often seen in operation, is not readily understood by an observer, without an acquaintance with the facts in natural philosophy on which its power depends: and it may elucidate the subject of this communication to advert, for a moment, to the gradations by which this important machine has attained its present perfection.

It will be recollected that as early as 1663, the Marquis of Worcester published some obscure hints of a mechanical power derived from the elastic force of steam.

In 1669, Savary, availing himself of the suggestion, and pursuing the subject more scientifically, invented his engine, consisting of an apparatus to cause a vacuum by the condensation of steam, so that the water to be raised would thereupon, by the external weight of the atmosphere, rise into the chamber of the apparatus, which the steam had occupied.

As caloric becomes latent in the steam which it forms at 212° of Fahrenheit, and the steam thus formed occupies 1800 times the bulk of the water composing it; and as it returns instantly to

a state of water on losing its heat, by contact with any thing cold, Savary easily produced his vacuum by the injection of a little cold water.

He also used (though in a very disadvantageous manner) the expansive force of steam to drive the water out of the chamber, through a pipe different from that by which it entered.

It is doubtful whether this kind of engine was ever erected on a scale of any magnitude; for, a few years later, Newcomen and Crawley invented the first engine with a cylinder and piston; and Savary, abandoning his own, united with them in bringing their engine into use.

As steam drives out air, the principle of this engine was to let steam into the cylinder beneath the piston, where (the piston having risen to the top of the cylinder) a jet of cold water* condensed the steam, produced a vacuum, and the piston, working air tight, descended by the pressure of the atmosphere upon it, this pressure being a weight of nearly fifteen pounds to each square inch; so that if the cylinder were two feet diameter, it would amount to a weight of three tons.

This mode of operation prevailed for about fifty years, and though much used to pump water from mines, was found to have great inconveniences and defects; till, in the year 1762, Mr. Watt, being employed to repair a working-model of an engine at the University of Glasgow, was led to direct his mind to the improvement of the machine; and from his experiments sprung the most essential change, viz. the condensation of the steam in the cylinder, by opening a communication with a separate vessel, into which the injection of cold water was made, thus allowing the cylinder to remain hot.

On opening that communication, the steam instantly rushes to the cold, or rather is destroyed by the instant loss or reduction of its heat, and the vacuum thus made allows the piston to descend as before mentioned.

* This jet of cold water being let into the cylinder itself, necessarily cooled it at every stroke; and then it was necessary to heat it again to the boiling point, before the piston would reascend, and thus a vast loss of heat occurred. EDITOR.

Mr. Watt soon added the airpump to the condenser, to extract the air extricated from the water in boiling, together with the water injected.

The next step was to close the upper end of the cylinder, the piston-rod working through a tight packing to exclude the air, letting the steam in above, as well as below the piston, by an alternate communication, and then condensing it in both cases alternately, thus producing a double stroke ; at the same time deriving some aid from the expansive force of the steam on the side of the piston opposite to the vacuum. This is essentially the form of all the engines in use at the present day. The minor parts devised by Mr. Watt, as the working of the valves, &c. were such as would readily occur to a scientific mechanician.

While he was bringing the engine to its present perfection, and furnishing it for the numerous mines, manufactories, and breweries in Great Britain, variations were devised by Cartwright, by Hornblower, Woolf, and others in England, and more recently by Evans and by Ogden in America, evincing much ingenuity, but (with the exception of Evans's, which is a simple engine of high pressure) making the machine more complex.

Watt and Bolton's engine, as most generally used, being properly an atmospheric engine, or working with steam so low as merely to produce a vacuum in the cylinder, became of enormous dimensions, when the power required was that of an hundred horses : a scale of estimate adapted to the comprehension of those who had before used the labour of that animal, and preferred to substitute the steam-engine.

It had not, however, escaped the notice of Mr. Watt, that there existed in steam another source of power besides that of atmospheric pressure. The experiments of his learned friend, Dr. Black, of Glasgow, as well as those of the French chemists, and of Papin, in the instance of his digester, had ascertained the laws of its expansive force, and amongst other interesting facts, those subservient to our present purpose ; viz. That after water has reached the boiling point, 212° of Fahrenheit, the caloric which enters it no longer becomes latent, but sen-

sible in the steam, which thereupon acquires expansive force to an unlimited degree: that this force increases geometrically; or, that every accession of about 30° of heat, nearly doubles its power at those stages of progression; that when the pressure at a high temperature is taken off, or the steam allowed to flow, there is an instantaneous and rapid production of steam; a fact which proves there can be no necessity of a large space for the steam to form in above the water, provided it be sufficient to prevent water from issuing with the steam, and, therefore, that boilers of a small cylindrical form are best.

It may be a fair question, why Mr. Watt did not further employ this principle of expansive force? We may readily conceive of several motives to the contrary. Watt and Bolton's engines were in great demand; they gave entire satisfaction, and the work they performed saved so much labour as to afford the purchase at a high price. The public had gained immensely by this better form of the engine, and Mr. Watt enjoyed the benefits of the patent he had obtained; and, at a later period, this preference was increased by an accident which happened to Trevethick's engine, though caused by gross mismanagement, that would have been equally fatal to any other.

From an investigation, by a committee of parliament, into the causes of the several fatal explosions of steam-engine boilers within a few years, published in Tillock's Magazine, vol. I., it appears that in every instance the accident was fairly attributable to neglect or mismanagement. Many competent persons were summoned to give their opinions; and through the contrariety of their testimony, the prevalent opinion appears to have been, that cast-iron boilers cannot be safe; that as many engines of high steam as of low are now used in England, but that the high are much the most economical in fuel and cost; that they are more safe, if properly constructed; it being argued by some, that boilers for steam of 100 pounds to the inch, are easily made of strength to sustain 500 pounds; this excess being much greater than in those constructed for low steam, makes them comparatively the safest, as the safety valves are less liable to be accidentally prevented from venting the steam.

In the United States, instances are not wanting of the successful operation of high steam; of which the engine at the mint is a conspicuous example. There can, indeed, be no good reason why this great power should not be employed to an extent within the limits of safety, if more economical and convenient. If boilers can bear (as they are usually made of iron) 500 pounds, there can be no danger in using them with fifty; and this gives an increase of power, with a condenser, fourfold, or makes a ten horse power forty. The economy, therefore, of high steam, hardly admits of a question. It seems unphilosophical to neglect a power so great, merely because it is so.

Mr. Watt was desirous of an improvement by which to obtain a direct rotatory motion. His experiments, resembling those of Curtis, at New-York, were not found permanently practicable.

It was probably perceived to be a great object to get rid of a reciprocating movement of large masses, on the well-known mechanical principle, that it consumes power to check momentum, as well as to give it—to drag an inert mass into motion rapidly, in opposite directions. And in engines for navigation this is more disadvantageous than for land uses, as the foundation of the engine cannot be perfectly substantial.

An engine, therefore, that possesses the cylinder and other members of Watt's engine, working with or without a condenser, at pleasure—having a rotatory movement—requiring no ponderous balance-wheel—adapted to high steam—attended by no inconvenience from the rapidity of its stroke or movement—having no inert mass of machinery to move reciprocally—more powerful, proportionately, from its using steam as strong as that in the boiler—of a simple and durable construction, and by a combination of two similar machines attached to the same common intermediate axis, operating so as to give nearly an equal power at every moment of its operation, seems to combine every thing desirable in an engine for the purposes of navigation. Such appears to be the revolving engine invented by Mr. Morey.

When those who are acquainted with steam-engines of the atmospheric kind only, are told that Morey's cylinder revolves, their imaginations may suppose a moving mass as large as the enormous cylinders they have been accustomed to see: but it is not so; the elastic force of steam requires machinery but of comparatively small dimensions.

The revolving engine makes up in activity what in other engines is supplied by magnitude.

We will take for example the engine working at the glass manufactory in this vicinity, the cylinder of which has one foot stroke and nine inches diameter, and is at least a ten horse power, working with fifty pounds—or, the engine now building for the Hartford boat. This engine will have two cylinders of seventeen inches diameter and eighteen inch stroke; they will revolve fifty times a minute. The area of the piston in each being 227 inches, steam at fifty pounds will give an hundred horse power.

This boat is seventy-seven feet long, twenty-one feet wide, and measures one hundred and thirty-six tons. The engine, with its boilers, will occupy sixteen feet by twelve, or one-eighth only of the boat; the cylinders being hung on the timbers of the deck over the boilers. She is principally intended to tow vessels up the river to Hartford.

In towing, it is of importance that the engine admit of any inferior velocity or power, till some momentum is had. An engine working by atmospheric pressure does not admit of this. And as the boat herself, at the moment of commencing the operation, may have no steerage-way, by placing two blade-rudders at the sides, behind the water-wheel, where a current is occasioned by them, the boat is kept in her relative position.

The application of the steam-engine to the towing of other vessels was fully appreciated by the late Mr. Fulton, whose conspicuous labours and enterprise in the establishment of steam-boats, the public duly honours. His active mind had conceived of its utility; and he would have obtained a patent, had not the previous employment of steam in this way, and the award of arbitrators on the question been in my favour;

which I mention merely in reference to the supposed utility of this mode of operation, in connexion with Morey's engine.

Morey's engine should rather be denominated a revolving engine than a rotatory one, especially as it is essentially different from one so called invented by Mr. Curtis.

Plate I. Fig. 5, represents the arrangement of a double engine for a boat, with its cylinders in different positions. *a a a*, boilers ; *b b*, tar-vessel ; *c*, valve-box ; *d*, cylinders in different positions ; *e*, piston-rod ; *f*, pitman ; *h*, centre-piece ; *i i*, shaft ; *k*, valve ; *l*, steam-pipe ; *m*, escape-pipe ; *n*, condensers ; *t*, water-wheel ; *v*, face of the valves ; *x*, tar-fire. The frame, holding the cylinder (*d*) is, by its opposite sides, so hung as to revolve. To the end of the axis of one side, extended over the cylinder, is fixed the centre-piece (*h*) resembling a crank, from which the bar or pitman (*f*) communicates to the cross-piece of the piston-rod. On this same axis, but outside the frame, is placed two circular pieces, one of brass, the other of iron, (*k*) which we may call the valves. One is fixed on the axis, the other moves, and accompanies the frame and cylinder in its revolution ; from it, at opposite sides, pipes lead the steam to each end of the cylinder. It has a smooth face, which applies, and is kept by springs close to that of its counterpart fixed on the said axis. Steam-pipes lead from the boilers through the counterpart into the moving valve. On the opposite side of the fixed piece the eduction-pipe (*o o*) leads to the condensers.

The condensers (*p*) are upright vessels, two to each cylinder, connected at top by a sliding valve box, so that the steam enters them alternately. At bottom are two valves, kept closed by weights. A stream of water is injected into the condensers, which escapes by the bottom valves (*p p*) by which also the air is blown out, at every stroke, in the same manner the engine is cleared of air at first.

There are also two cocks and cross-pipes seen, Plate III. Fig. 4, to change the steam from one side to the other of the valve, to give a reversed motion of the engine.

The power is communicated to its object from the opposite side of the frame by the axis attached thereto, and supported

on bearings. This axis (*i i*) may be of any length; may terminate in a crank or cog-wheel, or another cylinder (as here represented) may be attached thereto at right angles to the first, to co-operate and produce, at every moment, equal power.

Plate II. Fig. 6. Profile of the above. *a a*, the boiler; *c*, valve; *d e g*, cylinder and frame; *f*, valve; *h h*, cog-wheels; *i*, cog-wheels to move the pumps; *k k*, condensers; *m m*, coverings in; *o o*, gas-fire flue.

Fig. 1. *a*, steam-pipe; *b*, escape-pipe; *c*, fixed valve; *d*, moving valve; *e*, axis; *f*, a washer; *g*, section of frame; *h*, a washer; *i*, centre-piece; *l l*, steam-pipe; *k k*, springs to keep the valves together.

The canal-boat has her wheel in the stern. (See Plate IV.) The motion is given by a cog-wheel upon its axis (*g*) played upon by another, upon a shaft, at right angles, to which the engine communicates motion. The wheel being divided by a space of two or three inches, into two parts, to allow room for this shaft, and for the support of its end.

Fig. 3, represents the arrangement of the machinery, occupying the after-part of the boat. An engine of twenty horse power may thus occupy half a canal-boat, can tow a number of others at such rate as may be proper on canals.* *b b*, the boilers; *c*, tar-vessel; *d*, the cylinder; *f* water-wheel.

The supply of water to the boilers is either by a pump, in usual form, or by the *supply-chamber* of my invention, (Plate III. Fig. 2.) which consists simply of a pipe having two stop-cocks, one end in a reservoir, the other opening into the boiler at top, sloping downward for a foot or two. The cocks are in the sloping point. The operation commences, by opening the cock nearest the boiler, the steam drives the air out of the pipe through the water into the reservoir; shut the cock, and the water rises from the reservoir to fill it;

* But it is not necessary (as in the plate) to crowd the engine into the after-part of the boat, the boilers may be placed forward, and near them, or over them, *the cylinder*, &c. The power is then communicated to the stern-wheel by a long shaft, supported on, or immediately under, the deck. This arrangement gives room for loading both behind and before the boilers and engine, and equalizes the burden. This is the actual arrangement of the Merrimack boat.

shut the second cock, and open the first, the water discharges from the chamber into the boiler; repeated by a movement from the engine, when in motion, the supply continues with more certainty than by a pump, because it is difficult to pump hot water, on account of the elasticity of the steam arising from it, which obstructs the operation of the valves. And it is important not to have to pump against the pressure of high steam.*

Plate III. Fig. 4. The mode of changing the passage of the steam to the opposite sides of the valves, in order to get a reversed motion of the engine. *a a*, the fixed part, or valves; *c d*, the pipes; *f g*, the cross pipes; *e e*, the cocks, which are represented open, to pipes *c* and *d*—turn them half round, they close *c* and *d*, and open *f* and *g*. Fig. 1 shows the side-rudders, *d*, *e*, &c.

To this engine is conveniently applied the gas-fire, in the following manner.

The boilers being cylindrical, with an inside flue for fuel, two or three are placed close together, and set in the following manner: First, cross-bars of iron are laid on the timbers, a platform of sheet-iron is laid on these bars, coated over with clay mortar, or cemented, to keep out the air. Upon the sheet-iron, and over the bars below, are placed cast-iron blocks in shape to fit the curve of the boiler, so as to raise it three or four inches above the platform. The sheet-iron is continued up the outsides of the outer boilers, so as to enclose them; and at one end, between the boilers, there are small grates for coal or other fuel.

The tar vessel or vessels, as the case may be, are lodged in the space between and upon the boilers, and a small fire may be made under them, if necessary. A pipe leads steam in at one end, two pipes at the other; one near the bottom, and one near the top, lead out the tar and steam. These pipes unite below; the steam and tar, thus mingled in suitable proportions, flow to the main fire, or the flues of the boilers,

* It is found with very high steam that the source of supply must be above the *chamber*, or a small quantity of cold water introduced to condense the steam therein.

as well as to the coal-fire below, where the gas and tar are ignited. The fireman judges of the proportion of each, by the effect; the object being to produce a nearly white flame without appearance of tar. Thus flame is applied to the greatest possible surface, and the apparatus adds very little to the cost of the engine.

There are also two improvements in the boiler, which I deem it important to mention. First, the lining or covering of the flue within with sheet-iron or copper, *perforated with small holes*, reaching down its sides, nearly to the bottom. Plate III. Fig. 2. *a* the boiler; *b* the flue; *d* the grate; *c c* the lining.

This causes the water to circulate rapidly between them to the top of the flue, and protects it from being run dry, or heated red hot, when the water gets, by accident, too low. The lining also *causes the steam to form much faster, in consequence of this circulation.*

The other is the interior boiler. A vessel occupying the back part of the flue. Plate II. Fig. 8. (*d*) communicating downwards with the water, and upwards with the steam of the main boiler. The fire acts upon it very forcibly, surrounding it on all sides.

I have said there is no reciprocating movement in Morey's engine. Should it be objected that the piston moves in the cylinder as usual, it must be apparent that it also moves circularly; it is in fact the cylinder that moves, carrying the piston with it, which gives and keeps up the motion, by drawing and pressing on the centre-piece, and communicating the resistance thence to the *guides* of the cross-piece on the insides of the frame, which thus receives its motion.

In fact, this form of the engine seems divested of all the usual drawbacks on its power, and leaves it to act freely with any velocity, according to the strength of the steam in the boilers.

Such it appears in principle, and such thus far in practise. I have therefore preferred it for the purposes of navigation, and have purchased the patent right. But, though interested to recommend it, I cannot expect it to be preferred by the

intelligent, if there is not merit in the invention, and great economy in its use. It may be considered the most direct application of the power, and the most unexceptionable mode of using the expansive force of high steam. And from the nature of its movement the most applicable to boats and vessels.

Your Journal being the intended medium of information to promote the useful arts, I hope it may be consistent with this object to explain the manner in which these improvements may be made extensively useful.

It being necessary to supply the engines at a reasonable rate, I have established a manufactory for this kind only. The great expense of steam-boats hitherto, has confined their use too exclusively to the accommodation of passengers. There is a wide field opening for their use, in freighting, on all our waters; and it is often of importance to a community, when great savings can be made, that large capitalists should be induced to engage that such savings may be greater. Where companies are formed for an extensive operation, the legislature may, with propriety, grant an extension of the time for patents to run, that such persons may be duly remunerated for their enterprise, by the duration of the service.

Our laws do not yet make a proper distinction between patents of a large and expensive kind and those requiring little or no capital to go into operation. The period of fourteen years remunerates the inventor of those improvements only that require no capital, and involve no risk.

On this ground several of the State legislatures have, with good policy, given encouragement to this kind of enterprise. They suspend the free use of the invention a few years, rather than loose its immediate operation on a large scale of public benefit.

The constitutionality of the measure plainly appears by its not interfering with the laws of the United States. It is not an act exclusive of, or in opposition to, patents, but acknowledging and confirming them. It is furthering and giving effect to the intentions of the general government, in the encouragement of useful inventions. For their own particular

section of the union, a State legislature may thus provide for the protection of capital, engaged in enterprise of uncommon risk, as well as of uncommon usefulness, without excluding other and better inventions, should they arise.

I shall ask leave to communicate, for some future Number, the results of experiments, now making, with the gas fire applied to engines.

I am your most respectful humble servant,

JOHN L. SULLIVAN

Art. XVI. *Cautions regarding Fulminating Powders.*

Fulminating Mercury.

DURING a late lecture in the laboratory of Yale College, a quantity of fulminating mercury, probably about 100 or 150 grains, lay upon a paper, the paper lay on a small stool, which was made of pine plank, *one inch and a half thick;* a glass gas receiver, 5 or 6 quarts capacity, stood over the powder, as a guard, but without touching it, and stool and all stood on one of the shelves of the pneumatic cistern, surrounded by tall tubes and other glasses, several of which were within 6 or 8 inches. A small quantity of the fulminating powder, at the distance of a few feet, was merely flashed, by a coal of fire, but without explosion. In a manner, not easily understood, the whole quantity of powder under the large glass instantly exploded with an astounding report; *but the glass was not exploded*—it was merely thrown up a little; in its fall it was shattered, and broke a glass which it hit, but no fragment was *projected*, and none of the other contiguous tubes and glasses were even overset, nor were any of a large audience, and some of them very near, even scratched; *but the plank, one and a half inch thick, on which the powder lay, had a hole blown quite through, almost as large as the palm of one's hand.* This is a striking instance to prove that the *initial* force of this powder, when exploded, is very great, but that it extends but a very little way. If it be

strewed through a glass tube of three-fourths of an inch in diameter, and exploded by a coal of fire or hot iron, the tube may be held in the naked hand, and the powder only flashes without breaking the tube, and merely coats it over inside, and that very prettily, with the revived quicksilver.

Fulminating Silver.

Chemists are too well acquainted with the tremendous energy of this preparation, to make any comment upon its powers necessary. Unhappily, however, it is now made a subject of amusement; it is prepared for sale by those who know nothing of it, except as a nostrum, and it is bought by others who have not even this degree of knowledge. It is true it is put up in small quantities, in the little toys called torpedoes, and, if exploded one by one, they will ordinarily do no harm; but as they fall into the hands of children, we can never be secure that they will be discreetly used.

A very severe accident, from the unexpected explosion of this substance, occurred some years since in the laboratory of Yale College. (See Bruce's Journal, Vol. I. p. 163.) And, notwithstanding that this occurrence was well known in New-Haven, the same accident, only under a severer form, has again occurred in that town.

A man who had bought the secret of making fulminating silver, had prepared as much as resulted from the solution of one ounce and a half. Apparently, in a great measure, unaware of the nature of the preparation, he had placed it, unmixed with any thing, on an earthen plate, which stood on a table; his wife and children being around, he sat down to distribute the powder upon several papers which he had prepared for the purpose; sand and shot are mixed with the powder in the papers for the purpose of giving momentum, and of producing attrition when the torpedo is thrown, in order to ensure its explosion. Probably also the sand, looking not very unlike the powder, may be intended to screen it from view, and thus to preserve the secret, should the papers be opened. The unhappy man no sooner touched the fulminating silver with a

knife, than it exploded with its usual violence; the table was split in two; blood issued copiously from every part of his face, not from wounds, for it does not appear that the fragments hit him, but, according to the opinion of a competent judge, the blood was actually forced through the pores of the skin by the power of the explosion, which very nearly destroyed his eyes. He suffered immensely, but now, at the end of eight months, sees partially with one eye, but the other is nearly, if not quite, destroyed.

Should not the tampering with such dangerous substances by ignorant people be prevented by law?

In a late lecture in the laboratory of Yale College, some fulminating silver, on the point of a knife, was in the act of being put upon a copper-plate connected with one pole of a galvanic battery in active operation, the other pole was not touched by the experimenter; but it seems that the influence which was communicated through the floor of the room was sufficient instantly to explode the powder, as soon as the knife touched the copper-plate; the knifeblade was broken in two, and one half of it thrown to a distance among the audience.

Recently also, we are informed, in one of the foreign journals, that a man in England, who accidentally trod on a quantity of fulminating silver, had his foot nearly destroyed by the explosion.

USEFUL ARTS.

ART. XIX. *Account of an economical method of obtaining Gelatine from bones, as practised in Paris. Communicated to the Editor by Mr.* ISAAC DOOLITTLE.

Paris, 16th May, 1818.

MY DEAR SIR,

A FEW days since I visited the very interesting establishment of M. Robert, for the extraction of the gelatinous matter from bones.

The bones used for this purpose are those only which answered no useful purpose (except for the fabrication of phosphorus or ammoniac) before this discovery, such as those of the head, the ribs, &c. &c., the legs of sheep and calves, &c. Those formerly used by *toysmen* (*Tabletiers*) are still used for that purpose, after extracting so much of the gelantine as can be done by ebullition.

When the heads of oxen are to be operated upon, they begin by extracting the teeth, (these are reserved for the fabrication of ammoniac, as affording a greater proportion of that alkali than any of the other bones,) they then break the skull, in such manner as to preserve all the compact parts in as regular forms as possible ; these pieces present a surface of 20 to 30 square inches, and are put to soak in a mixture of muriatic acid and water. The muriatic acid used bears about twenty-three degrees of the *aeromètre*, and is diluted by water to about six degrees—four parts of the liquor is used to one part of bones. They are left in this state, in open vessels, until a complete solution of the phosphate of lime has taken place, and the gelatinous part of the bone remains in its original shape and size, and is perfectly supple. When this operation is finished, which commonly lasts six or eight days, the gelatine is put into baskets, being first drained, and immersed a short time in boiling water, in order to extract any small remains of grease, which would deteriorate the gelatine, and also to extract any of the acid which might be lodged in the pores. It is then carefully wiped with clean linen, and afterward washed in copious streams of cold water, to whiten it, and render it more transparent ; it is then put to dry in the shade.

Two ounces of this gelatine are said to be equal to three pounds of beef in making soup—that is, three pounds of beef and two ounces of gelatine will make as much soup, and of as good quality, as six pounds of beef. It is constantly used in some of the hospitals of the capital, particularly in the lying-in-hospital.

The ends of the bones, and such parts as from their porosity might still retain a portion of the acid, are separated, and used for making glue of a very superior quality.

The inside of the bones of sheep's legs furnish a sort of membranous glue, which supplies, with advantage, the place of isinglass in the fabrication of silk stuffs.

I give you these particulars, not because I think they contain any thing new to you, *in principle*, but because I may have hit upon some *details* with which you were unacquainted.

Art. XX. *Experiments made in France upon the Use of Distilled Seawater for domestic purposes, and its Effects on the Constitution, when taken as a Beverage.**

In consequence of the great want of good fresh water in many of the maritime parts of France, the government some time since ordered some experiments to be made, upon an extensive scale, in order to ascertain how far seawater, when distilled, could be used with success. Little or no use had hitherto been made of water so prepared, except in long voyages, and chiefly then only as a matter of necessity. There are above two hundred leagues of seacoast in France, where, to the breadth of many miles, the inhabitants are compelled to make use of bad and impure water, which, in many cases, is injurious to the health of themselves and their animals. In similar cases, it was the custom of the ancients to construct cisterns; but these are not only expensive in themselves, but their utility depends upon the quantity of rain that falls; while upon the shores of the most barren places, nature has supplied a variety of vegetable matter, which, when dried, would not only serve as a fuel for the purposes of distillation, but from the ashes of which might be obtained a saline substance, sufficient to repay the expense of collecting, drying, and burning. Thus the fuel for the distillation of seawater would, in reality, cost nothing, while its preparation would employ many individuals, particularly women and children. Before, how-

* Taken from the Philosophical Magazine, and by that work from the Annales de Chimie and de Physique, for January, 1818.

ever, erecting any apparatus for this purpose, it was necessary to ascertain both the utility and salubrity of the water thus prepared.

It is well known that Bougainville, Phipps, Homelin, &c. had employed this water with much success; but they, like most of the chemists of the last age, did not endeavour to imitate the process of nature in all its simplicity, but mixed various substances with the seawater, in order to take away or lessen the effect of the empyxeuma arising from the distillation, and which was so unpleasant to the smell and taste. And it is this which in general renders sailors so averse to it, and excites a prejudice very unfavourable to the salubrity of distilled seawater. One of the great objects to be ascertained was, whether this disagreeable smell and taste was peculiar to seawater, or arose from the act of distillation.

In the month of July, last year, the king ordered some experiments to be made, upon a large scale, at the three ports of Brest, Rochefort, and Toulon. The instructions given were as follows: That a sufficient quantity of seawater should be distilled to prepare, for the space of a month, bread and other food for a certain number of criminals, who were employed on the works of these ports, and also to supply them with drink, keeping from them during that period every other liquid. Ten or twelve persons at each part voluntarily came forward and offered themselves for the experiment.

The persons employed by government first distilled a sufficient quantity of seawater, without the admixture of any other substance. This produce dissolved soap, dressed vegetables, produced the same appearances, with the aerometer, as that distilled from spring water. There was no difference between the one and the other. But the distilled seawater had always that empyreumatic taste and smell, of which we have before spoken; and it was so strong, that the commission at Toulon called it *odeur de marine*, and *odeur de marecage*. But this is not peculiar to seawater, for the result of a distillation of fresh water had always the same taste and smell. Neither of these liquids immediately loses this by being filtered through char-

coal; but by being exposed for some time to the air, the distilled seawater loses this unpleasant quality, and then it does not differ from fresh water derived from the purest source; and both have equally stood every chemical test to which they have been exposed. The chemical properties of this water having thus been determined, it remains to give an account of the effects upon the individuals who underwent the experiment. These are the principal results:

Brest. During the first days, those who drank the water complained of a weight upon the stomach. This indisposition, which was the only one they experienced, soon decreased upon taking exercise, and totally went off by an additional ounce of biscuit added to their common ration. One of them, on the 29th day, had a few symptoms, but which he himself attributed to an indigestion, from some bacon he had eaten. Eight individuals drank twenty-five pints a day, rather more than three pints each.—(N. B. The French pint contains very near fifty-seven cubic inches of English measure, and is the regulation size for the claret or Bordeaux bottle; but in general the bottles are rather smaller. The French pint is therefore equal to rather more than nineteen-twentieths of an English quart, wine measure.)

Toulon. The results obtained at the arsenal of this town, were not less decisive or satisfactory. The six persons who made the experiment acquired a greater degree of freshness in their appearance, and were much fatter. Their daily consumption of distilled water was nine pounds (*poids de marc*) for drink, and eleven pounds for cooking. This is nearly the same relative quantity as those at Brest.

Rochefort. The experiments here have not been made with the same regularity; because the fifteen persons fixed upon had all agreed to say that they were very ill. The two principal ones complained of violent cholics and diarrhœas; but the plot was discovered, and upon being put upon the sick list, (*à la diète*,) they were laughed at by their companions. No one of them was really indisposed; on the contrary, many thought they experienced some good effect in regard to some infirmities under which they had long laboured.

The above are not, however, the only experiments which have been made upon this beverage. Several persons wishing to ascertain its effects by individual experience, have voluntarily confined themselves to its use; and the members of the commission of inquiry are almost in the daily practice of taking it. The captain of the Duclat has taken it every day at his meals for twenty days, and has experienced not the smallest inconvenience from its use. M. M. Vasse, and Chatelain, apothecaries to the marine at Brest, have occasionally kept the water in their mouths for four hours, by constantly renewing it, and have not found either the sharp taste, or other caustic qualities, which have been said to be peculiar to it. And here it may be proper to state, that the mouths of all the individuals who had taken the water for a length of time were examined, without the detection of any thing in them either of a swollen or inflammatory appearance. Such are the reports of commissioners employed to investigate the effects of distilled seawater, who, although separated at a great distance from each other, and having no communication, all agree in the inference, that it may be employed without any injury to the health, both as a beverage and in cookery, for the space of at least a month; and the fair presumption is, that it may be employed for a much longer time; and that in consequence, it must be considered as a very happy resource in long voyages of discovery.

FINE ARTS.

ART. XXI. *Essay on Musical Temperament. By Professor* FISHER, *of Yale College.*

[*Concluded from page* 35.]

PROPOSITION V.

To determine that position of any degree in the scale, which will render all the concords terminated by it, at a medium, the most harmonious; supposing their relative frequency given, and all the other degrees fixed.

THE best scheme of temperament for the changeable scale, on supposition that all the concords were of equally frequent occurrence, is investigated in Prop. III. But it is shown, in the last Proposition, that some chords occur in practice far more frequently than others. Hence it becomes necessary to ascertain what changes in the scale above referred to, this different frequency requires. Any given degree, as C, terminates six different concords; a Vth, IIId, and 3d above, and the same intervals below it. Let the numbers denoting the frequency of these chords below C be denoted by a, b, and c, and their temperaments, before the position of C is changed, by m, n, and p: and let the frequency of the chords above C be denoted by a', b', and c', and their temperaments by m', n, and p', respectively. If, now, we regard any two of these 6 chords, whose temperaments would be diminished by moving C opposite ways, and of which the sum of the temperaments is consequently fixed, it is manifest that the more frequent the occurrence, the less ought to be the temperament. Were we guided *only* by the consideration of making the aggregate of dissonance heard in them in a given time, the least possible, we should make the one of most frequent occurrence perfect, and throw the whole of the temperament upon the other.

Let, for example, a be greater than a', and let x be any variable distance to which C is moved, so as to diminish the temperament m, of the chord whose frequency is expressed by a. Then the temperament of a will become $=m \smile x$, and that of $a' = m' + x$. Hence, as the dissonance head in each, in a given time, is in the compound ratio of its frequency of occurrence and its temperament, their aggregate dissonance will be as $a.\overline{m \smile x} + a'.\overline{m' + x}$; a quantity which, as a is supposed greater than a', evidently becomes a minimum when $x = m$, or the chord, whose frequency is a, is made perfect. But in this way we render the harmony of the chords very unequal, which is, cæteris paribus, a disadvantage. As these considerations are heterogeneous, it must be a matter of judgment, rather than of mathematical certainty, what precise weight is to be given to each. We will give so much weight to the latter consideration, as to make the temperament of each concord *inversely as its frequency*. We have then $a : a' :: \frac{1}{m-x} : \frac{1}{m'+x}$; which gives $x = \frac{am - a'm'}{a + a'}$.

But there are six concords to be accommodated, instead of two; and it is evident that all the pairs cannot have their temperament inversely as their frequency, since the numbers a, b, &c. and m, n, &c. have no constant ratio to each other. This, however, will be the case, at a medium, if x be made such, that the *sum* of the products of the numbers expressing the frequency of those chords whose temperaments are increased by x, into their respective temperaments, shall be equal to the sum of the corresponding products belonging to those chords whose temperaments are diminished by x. Applying this principle to the system of temperament in Prop. III, which flattens all the concords, it is plain that raising any given degree by x will increase the temperaments of the concords above that degree, and diminish those of the concords below it. Hence it ought to be raised till $(m-x)\,a + (n-x)\,b + (p-x)\,c = (m'+x)\,a + (n'+x)\,b' + (p+x)\,c'$: from which x is found $= \frac{am - a'm' + bn - b'n' + cp - c'p'}{a + a' + b + b' + c + c'}$. Should either of the temperaments be sharp, the sign of that term of

the numerator, in which it occurs, must be changed; and should the total value of the expression be negative, x must be taken below C.

Proposition VI.

To determine that system of temperaments for the concords of the changeable scale, which will render it, including every consideration, the most harmonious possible.

We can scarcely expect to find any direct analytical process, which will furnish us with a solution of this complicated problem, at a single operation. We shall therefore content ourselves with a method which gradually approximates towards the desired results. The best position of any given degree, as C, supposing all the rest fixed, is determined by the last proposition. In the same manner it is evident that the constitution of the whole scale will be the best possible, when no degree in it can be elevated or depressed, without rendering the sums of the products there referred to, unequal. We can approximate to this state of the scale, by applying the theorem in Prop. V. to each of the degrees successively. It is not essential in what order the application is made; but for the sake of uniformity, in the successive approximations, we will begin with that degree which has the greatest sum $a+a'+b+$ &c. belonging to it, and proceed regularly to that in which it is least. Making the equal temperament of Prop. III., (in which the Vths, IIIds, and 3ds are flattened, 154, 77 and 77, respectively,) the standard from which to commence the alterations in the scale required by the unequal frequency of different chords, and beginning with D, the theorem gives $x=5$. Hence supposing the rest of the degrees in the scale unaltered, it will be in the most harmonious state, when D is raised $\frac{5}{540}$ of a comma. For by the last proposition, the temperament of the six concords affected by changing the place of D is best distributed, and that of the other concords is not at all affected. We will now proceed to the second degree in the scale, viz. A; in which the application of the theorem gives $x=13$. In this application, however, as D was before raised 5, m, the temperament of the Vth below A, must be taken $154+5$; and

in all the succeeding operations, when the exterior termination of any concord has been already altered, we must take its temperament, not what it was at first, but what it has become, by such previous alteration. In this manner, the scale is becoming more harmonious at every step, till we have completed the whole succession of degrees which it contains.

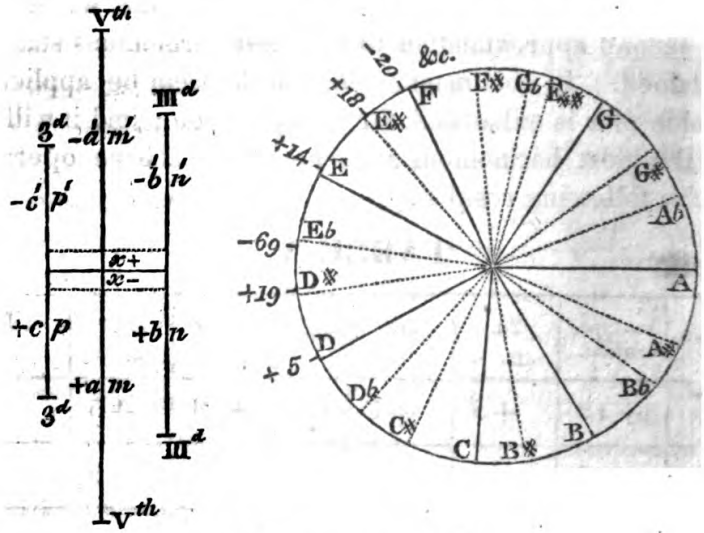
Let us now revert to D, the place where we began. As each of the outer extremities of the chords which are terminated by D has been changed, a new application of the theorem will give a second correction for the place of D; although, as the numbers a, a', b, &c. continue the same, it will be less than before. Continue the process through the whole scale, and a second approximation to the most harmonious state will be obtained. In this manner let the theorem be applied, till the value of x is exhausted, for every degree; and it will then be in the most harmonious state possible. Three operations gave the following results:

TABLE V.

Bases.	1st Operation.	2d.	3d.	Bases.	1st Operation.	2d.	3d.
F♯	+18	+5	+1	B♯	+18	+5	0
F	−20	−6	−1	B	+19	+5	0
E♯	+18	+5	0	B♭	−23	−10	−1
E	+14	+5	0	A♯	+18	+7	0
E♭	−69	−8	−1	A	+13	+4	+1
D♯	+19	+5	+1	A♭	−71	−7	−2
D	+5	+2	+1	G♯	+17	+5	0
D♭	−45	−7	−2	G	−14	0	0
C♯	+18	+2	0	F♯♯	+44	+5	0
C	−5	−5	−2	G♭	−46	−5	0

The sign *plus* denotes that the degree to which it belongs is to be raised, and *minus*, that it is to be depressed. The corrections in each succeeding operation are to be added to those in the preceding. The errors, in the 3d approximation, are so trifling, that a 4th would be wholly useless.

Note. The foregoing calculations will be rendered much more expeditious and sure, by reducing the theorem, in some sense, to a diagram, as in the first of the following figures; and by applying the successive corrections to the circumference of a circle divided into parts proportioned to the intervals of the enharmonic scale, as in the second.



Proposition VII.

To determine the temperaments and beats of all the concords, together with the values of the diatonic and chromatic intervals, and the lengths and vibrations per second of a string producing all the sounds, of the system resulting from the last proposition.

The temperaments of all the concords are easily deduced from Table V. The Vth CG, for example, has its lower extremity lowered 12, and its upper extremity 14. Hence it is flatter by 2 than at first, and consequently its temperament=156. The temperaments of all the concords, thus

calculated, will be found in the 2d, 3d, and 4th columns of Table VII.

Having ascertained the temperaments, the value of the diatonic and chromatic intervals may be found. The Vth CG being flattened 156, and the Vth FC 139, the major tone FG must be diminished 156+139, or be =4820. By thus fixing the extent of one interval after another, from the temperaments of either of the different kinds of concords, as is most convenient, the intervals in question will be found to have the values exhibited in Table VI.

Let the numbers in this table be added successively, beginning at the bottom, to the log. of 240, the number of vibrations per second of the tenor C, (see Rees's Cyc. Art. Concert Pitch,) and the numbers corresponding to these logarithms will be the vibrations in a second, of a string sounding the several degrees of the scale. They are shown in col. 6, Table VII.

Since the length of a string cæteris paribus is inversely as its number of vibrations, the lengths in col. 5 may be deduced from the vibrations in col. 6 ; or more expeditiously, by subtracting the numerical distances from C of the several degrees in Table VI. from O, and taking the corresponding numbers, from the table of logarithms. These numbers, when used as logarithms, must be brought back to the decimal form, agreeably to Scholium 2. Prop. I.

To find the number of beats made in a second by any concord, it is only necessary to take from col. 5 the numbers belonging to the degrees which terminate that concord, and to multiply them crosswise into the terms of its perfect ratio. The difference of the products will be the number of beats made in a second. The 3 last columns contain the beats made by each of the concords, in 10 seconds.

TABLE VI.

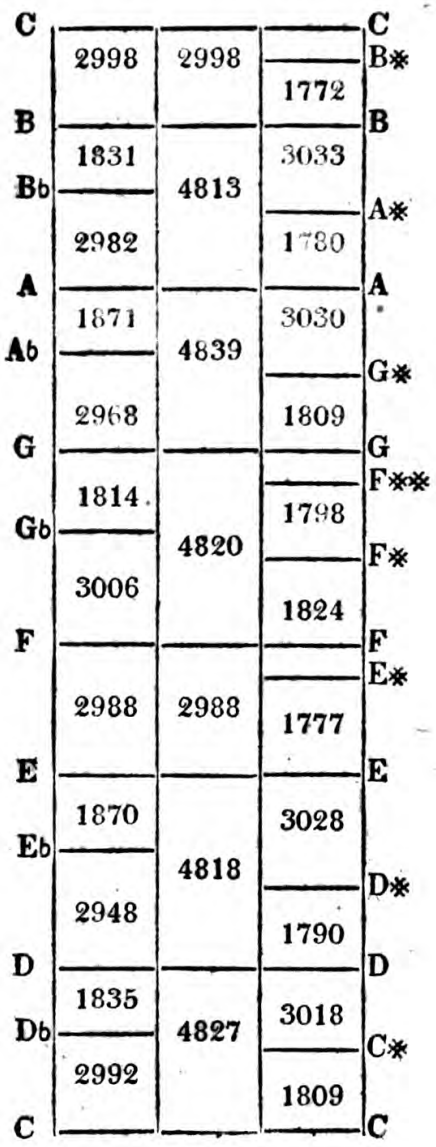

C
2998
2998
C
B♯
1772
B
B
1831
3033
B♭
4813
A♯
2982
1780
A
A
1871
3030
A♭
4839
G♯
2968
1809
G
G
F♯♯
1814
1798
G♭
4820
F♯
3006
1824
F
F
E♯
2988
2988
1777
E
E
1870
3028
E♭
4818
D♯
2948
1790
D
D
1835
3018
D♭
4827
C♯
2992
1809
C
C

TABLE VII.

Bases	Temperaments of the			Lengths of String.	Vibrations in a Second.	Beats in 10 S. of the		
	Vths b	IIIdsb	3ds b			Vths.	IIIds.	3ds.
B𝄪			77	51431	466,64			48,4
B	154	76	93	53574	447,98	47,4	39,0	57,8
Bb	147	35	97	55880	429,49	43,5	17,7	57,4
A𝄪	156		78	57448	417,77	45,1		46,2
A	153	71	107	59852	400,99	42,5	33,5	59,4
Ab	154	9		62487	384,08	40,4	4,0	
G𝄪	151	76	75	64177	373,97	39,1	32,9	39,2
G	132	39	97	66907	358,71	32,9	16,3	48,1
F𝄪𝄪			101	68778	348,95			48,5
Gb		56		69760	344,03		21,9	
F𝄪	154	76	83	71685	334,80	36,0	29,2	38,5
F	139	32	130	74760	321,03	30,9	11,9	57,8
E𝄪	154		78	76874	312,20	33,2		33,5
E	149	74	110	80085	299,68	30,8	25,2	45,3
Eb	110	13	54	83608	287,05	21,7	4,1	21,5
D𝄪	154	53	78	85868	279,50	29,6	17,0	30,0
D	144	61	112	89480	268,21	26,5	18,5	41,1
Db	180	50		93342	257,12	32,0	14,8	
C𝄪	156	78	82	95920	250,20	26,6	22,0	28,0
C	156	46	143	100000	240,00	25,8	12,8	47,5

PROPOSITION VIII.

To compare the harmoniousness of the foregoing system with that of several others, which have been most known and approved.

The aggregate of dissonance, heard in any tempered concord, is as its temperament (Prop. I.) when its frequency of occurrence is given, and as its frequency of occurrence, when its temperament is given: hence, universally, it is as the product of both. The whole amount of dissonance heard in all the concords of the same name must consequently be as the sum of the products of the numbers denoting their temperaments, each into the number in Table IV. denoting its frequency. These products, for the scale of Huygens which divides the octave into 31 equal parts, of which the tone is 5 and the semi-tone 3; for the system of mean tones, and for Dr. Smith's system of equal harmony, compared with the scale of the last proposition, (cutting off the three right-hand figures) stand as follows:

TABLE VIII.

Systems.		Huygens's.	Dr. Smith's.	Mean Tones.	New Scale.
Dissonance of the	Vths	825	945	850	786
	IIIds	121	382	0	240
	3ds	1049	629	944	683
Total		1995	1956	1794	1709

Were we to adhere to Dr. Smith's measure of equal harmony, the rows of products belonging to the Vths, IIIds, and 3ds, must be divided, respectively, by $\frac{1}{3}$, $\frac{1}{10}$, and $\frac{1}{15}$ (the reciprocals of half the products of the terms of their perfect ratios,) before they could be properly added to express the whole amount of dissonance heard in all the concords; but, according to Prop. I. the simple products ought to be added, and the sums at the bottom of the table will express the true

ratio of the aggregate dissonance of the systems under which they stand. The last has decidedly the advantage over the first, both in regard to the aggregate dissonance, and the equality of its distribution among the different classes of concords. It has nearly an equal advantage over the second in regard to the first of these considerations; although in regard to the equality of distribution, the latter has slightly the advantage. It has, in a small degree, the advantage over the third, in regard to the aggregate dissonance; while, as it respects the equality of its distribution, it has the decided preference. It is true that the temperaments of the concords of the same name, in the new scale, are not as in the others, absolutely equal; but no one of them is so large as to give any offence to the nicest ear. The largest in the whole scale exceeds the uniform temperament of Dr. Smith's Vths by only $\frac{1}{18}$ of a comma.

Scholium 1.

The above system may be put in practice on the organ, by making the successive Vths CG, GD, DE, &c. beat flat at the rate contained in Table VII., descending an octave, where necessary, and doubling the number of beats belonging to any degree in the table, when the Vth to be tuned has its base in the octave above the treble C. The tenor C must first be made to vibrate 240 in a second, the methods of doing which are detailed at length in various authors. Whenever a IIId results from the Vths tuned, its beats ought to be compared with those required in the table, and the correctness of the Vths thus proved. This system is as easy, in practice, as any other; for no one can be tuned correctly except by counting the beats, and rendering them conformable to what that system requires. The intervals of the first octave tuned ought to be adjusted with the utmost accuracy, by a table of beats. When this is done, the labour of making perfect the other octaves of the same stop, and the unisons, octaves, Vths, &c. of the other stops, is the same in every system. This last, indeed, is so much the most laborious part of the tuning of the organ, that if even much more labour were required than

actually is, in adjusting the intervals of the octave first tuned, it would occasion little difference in the whole.

Scholium 2.

The harmony of the IIIds and 3ds in any of the foregoing systems for the changeable scale is so much finer than it can possibly be in the common Douzeave, that it seems highly desirable that this scale should be introduced into general use. But the increased bulk and expense attendant on the introduction of so many new pipes or strings, together with the trouble occasioned to the performer, in rectifying the scale for music in the different keys, have hitherto prevented its becoming generally adopted. To multiply the number of finger keys would render execution on the instrument extremely difficult; and the apparatus necessary for transferring the action of the same key from one string or set of pipes to another, besides being complicated and expensive, requires such exactness that it must be continually liable to get out of order. This latter expedient, however, has been deemed the only practicable one, and has been carried into effect, under different forms, by Dr. Smith, Mr. Hawkes, M. Loeschman, and others. But Dr. Smith's plan (which is confined to stringed instruments) requires only one of the unisons to be used at once; while those of the two latter nearly double the whole number of strings or pipes. It deserves an experiment, among the makers of imperfect instruments, whether a changeable scale cannot be rendered practicable, at least on the piano forte,* without increasing the number of strings,

* A method of rendering changeable the sound of the same pipes in the organ, which had occurred to the writer, but which was not inserted above on account of the supposed difficulty of making the change sufficient in degree, he has since found to have been executed by the Rev. H. Liston, who has succeeded, by means of shaders capable of being brought before the mouths of his pipes by the action of pedals, in giving them three distinct sounds each, varying by two commas. (See the description of his Enharmonic organ, in Rees' Cyc. or Tilloch's Phil. Mag.) His scale embraces 59 intervals to the octave, and is intended to produce perfect harmony in all the keys. But as it will require the use of pedals perpetually, even on the same key, and a ready and perfect knowledge of small

and at the same time allowing both the unisons to be used together—either by an apparatus for slightly increasing the tension of the strings, or by one which shall intercept the vibrations of such a part of the string, at its extremity, as shall elevate its tone, by the diesis of the system of temperament adopted. Were only 4 degrees to the octave, furnishing the instrument with 5 sharps and 4 flats, thus rendered changeable, there is little music which could not be correctly executed upon it.

Scholium 3.

In the same general manner, may be found the best system of intervals, for a scale confined to a less number of degrees than that of the complete Enharmonic scale. In such an investigation, the numbers in Table IV. expressing the frequency of all such adjacent degrees as have but one sound in the given scale, must be united; and the temperaments m, n, &c. of the theorem, when belonging to concords whose terminating degrees are united to those adjacent, must be taken, not what they were in the complete scale, but what they become, considering them as terminated by the substituted adjacent degree.

If, for example, the best temperaments were required for a scale of 15 degrees to the octave, such as is that of some European organs, or in other words, having no Enharmonic intervals except D♯ E♭, and G♯ A♭,—the numbers in Table IV. belonging to C♯ and D♭, E♯ and F, F♯ and G♭, &c. must be united, and their sums substituted when they occur, for a, a', b, &c. in the theorem; while the temperament, for example, of the IIId on C♯ must not be reckoned 77 as in the complete scale, but 1261−77 sharp, since its upper termination has become F, instead of E♯. With these variations let

musical intervals, which practical musicians can seldom possess, there is no probability that it will ever be extensively adopted. Perhaps, however, four or five sounds, such as D♯, E♯, A♭, D♭, might be added to the common scale of 12 intervals by means of his mechanism, with advantage. An instrument thus furnished would require the use of pedals but seldom, and would contain chromatic degrees sufficient for the accurate performance of the great mass of organ music.

the same theorem be applied as before, till no value of x can be obtained, and the temperaments for that scale will be the best adjusted possible.

But as the scale which contains but 13 degrees, or 12 intervals, to the octave, is in much more general use than every other, we shall content ourselves with stating *how* the problem may be solved for scales containing any intermediate number of degrees, and proceed directly to the consideration of that which is so much the most practically important.

LEMMA.

No arrangement of the intervals in the common scale of 12 degrees, which renders none of the Vths or 3ds sharp, and none of the IIIds flat, can make any change in the aggregate temperaments of all the concords of the same name.

We will conceive the 12 Vths of the Douzeave scale to be arranged in succession, as CG, GD, DA, &c. embracing 7 octaves. Let them at first be all equal: they will each be flattened 49. I say that no change in these Vths which preserves the two extreme octaves perfect, and renders none of them sharp, can alter the sum of their temperaments. Let a, b, c, &c. be any quantities, positive or negative, by which the points C, G, D, &c. may be conceived to be raised above the corresponding points, belonging to the scheme of equal Vths. Then as the mean temperament Vth$=V-49$, the first Vth in the supposed arrangement will be $V-49+a$. The distance from C to D will be, in like manner, $2.\overline{V-49}+b$; and consequently the Vth GD will be $V-49+b-a$. In the same manner the third Vth DE will be $V-49+c-b$, &c. Hence the temperament of CG$=-49+a$, of GD$=-49+b-a$, of DA$=-49+c-b$, &c. Adding the 12 temperaments together, we find their sum $=-12\times49+a+b+$&c.$-a-b-$ &c. in which all the terms except the first destroy each other, and leave their sum $=-12\times49$ which is the aggregate temperament of the twelve equal Vths in the scheme of equal semitones.

The same reasoning holds good if we bring these Vths within the compass of an octave; since, if the octave be kept

perfect, all the Vths on the same letter, in whatever octave they are situated, must have the same temperament.

The reasoning is precisely the same for the IIIds and 3ds, considering the former as forming 4 distinct series of an octave each, beginning with C, C♯, D and E♭; and the latter as forming 3 distinct series of an octave each, beginning with C, C♯ and D. If the former be made all equal, each will be sharpened 343; if the latter be made equal, each will be flattened 392. In every system which renders none of the former flat, and none of the latter sharp, the sum of their temperaments will be 12×343, and 12×392, respectively.

Cor. The demonstration holds equally true, whatever be the magnitude of a, b, c, &c.: only if they be such that the difference $-a+b$, $-b+c$, &c. of any two successive ones be greater than the temperament of the corresponding concord in the system of equal semitones, the temperament of that chord must be reckoned negative, and the *sum*, in the enunciation of the proposition, must be considered as the excess of those temperaments which have the same sign with those of the same concords in the system of equal semitones, above those which have the contrary sign. Hence it is universally true that the excess of the flat above the sharp temperaments of the Vths is equal to 12×49; that the excess of the sharp above the flat temperaments of the IIIds is equal to 12×343; and that the excess of the flat above the sharp temperaments of the 3ds is 12×392. Hence likewise we have a very easy method of *proving* whether the temperaments of any given system have been correctly calculated. It is only to add those which have the same sign; and if the differences of the sums be equal to the products just stated, the work is right.

Proposition IX.

If all the concords of the same name, in a scale of twelve intervals to the octave, were of equally frequent occurrence, the best system of temperament would be that of equal semitones.

It is evidently best, so far as the concords of the same name are concerned, that if of equal frequency, they should be

equally tempered, unless by rendering them unequal, their medium temperament could be diminished; but this appears, from the Lemma, to be impossible. By tempering them unequally, the aggregate dissonance heard in a given time, by supposition of their equal frequency, would not be diminished, whilst the disadvantage of a transition from a better to a worse harmony would be incurred. Some advocates of irregular systems of temperament have, indeed, maintained this irregularity to be a positive advantage, as giving variety of character to the different keys. But this variety of character is obviously neither more nor less than that of greater and less degrees of dissonance. Now, what performer on a perfect instrument ever struck his intervals false, for the sake of variety? Who was ever gratified by the variety produced in vocal music by a voice slightly out of tune? If this be absurd, when applied to instruments capable of perfect harmony, it is scarcely less so to urge variety of character as being of itself a sufficient ground for introducing large temperaments into the scale. For these large temperaments will have nearly the same effect, compared with the smaller ones, that small temperaments would have, when compared with the perfect harmony of voices and perfect instruments. Possibly a discordant interval, or a concord largely tempered, might, in a few instances, add to the resources of the composer. But when an instrument is once tuned, the situation of these intervals is fixed beyond his control, and by occurring in a passage where his design required the most perfect harmony, it might as often thwart as favour the intended effect.

Since, then, the proposition is true in reference to the Vths, IIIds, and 3ds, when separately considered, it will be equally true when they are considered jointly, that is, as formed into harmonic triads, unless, by rendering the concords of the same name unequal in their temperament, the mean temperament of the Vths could be increased, and that of the IIIds and 3ds proportionally diminished. Could this be done, it might be a question whether the more equal distribution of the temperament among the concords of different names, might not justify the introduction of some inequality among those of the same

name. But it is demonstrated in the Lemma, that the sum of the temperaments of each parcel of concords, in the system of equal semitones, is the least possible. Hence no changes in the Vths can diminish the average temperaments of the IIIds and 3ds.

Cor. Hence we derive an important practical conclusion: that whatever irregularities are introduced into the scale, must be such as are demanded by the different frequency of occurrence of the several concords. If we make any alterations in the scale of equal semitones, this must be our sole criterion. A given system of temperament is eligible, in proportion to the accuracy with which it is deduced from the different frequency of the different concords. And those who maintain that the frequency of different intervals does not sensibly vary, or that it is of such a nature as not to be susceptible of calculation, must, to be consistent, adhere to the scale of equal semitones.

Proposition X.

To determine the best distribution of the temperaments of the concords in the Douzeave Scale.

As the scale of equal semitones has been demonstrated to be the best, on supposition that all the concords of the same name occurred equally often, it ought to be made the standard from which all the variations, required by their unequal frequency, are to be reckoned. To find a set of numbers expressing the relative frequency of the several concords in the common scale, we have only to unite the numbers in Table IV. standing against those adjacent degrees which have but one sound in this scale. They will then stand as in the following table:

TABLE IX.

Bases.	Vths. 4ths, and Octaves.	IIIds, 6ths, and Octaves.	3ds, VIths, and Octaves.
B	221	135	1161
B♭	418	654	34
A	870	568	1085
G♯	57	82	$365\frac{1}{5}$
G	1207	1197	$567\frac{1}{4}$
F♯	67	$29\frac{1}{2}$	1072
F	639	924	78
E	548	323	1151
E♭	$265\frac{1}{3}$	$363\frac{1}{2}$	$144\frac{1}{2}$
D	1166	943	569
C♯	26	18	581
C	816	1131	184

The general theorem of Prop. V. is equally applicable to the determination of the approximate place for any degree in this scale, considering the numbers in the above table as those to be substituted for a, a', b, &c.; and m, n, and p, in the first instance, as 49, -343 and 392, the uniform temperaments of the Vths, IIIds, and 3ds, in the scale of equal semitones. Since, however, the temperaments of the IIIds in this scale are sharp, which would require the signs of the 3d and 4th terms in the numerator of the general formula to be continually changed, it will be rendered more convenient for practice, if they are changed at first, so that it will stand thus:

$$x=\frac{am-a'm'-bn+b'n'+cp-c'p'}{a+a'+b+b'+c+c'}.$$

Three successive applications of this theorem to each degree in the scale, in the manner described Prop. VI., will bring them very near to the required position, as appears by the smallness of the corrections in the 3d column below, where the results of the several operations are exhibited at one view.

TABLE X.

Bases.	First Operation.	Second Operation.	Third Operation.
B	−140	−35	−2
B♭	+308	+33	−1
A	−8	−23	+2
G𝄪	−257	−22	−2
G	+107	+24	−8
F𝄪	−264	−7	0
F	+238	+40	+6
E	−80	−34	−4
E♭	+157	+2	−4
D	+58	+8	0
C𝄪	−352	−29	−1
C	+176	+29	+4

Cor. Hence we may deduce, in the same manner as in Prop. VII., the diatonic and chromatic intervals, the lengths of a string and their vibrations in a second, and the temperaments and beats of all the concords for the scale which results from the foregoing computations. They may be seen in the two following tables :

TABLE XI.

DIATONIC AND CHROMATIC INTERVALS.

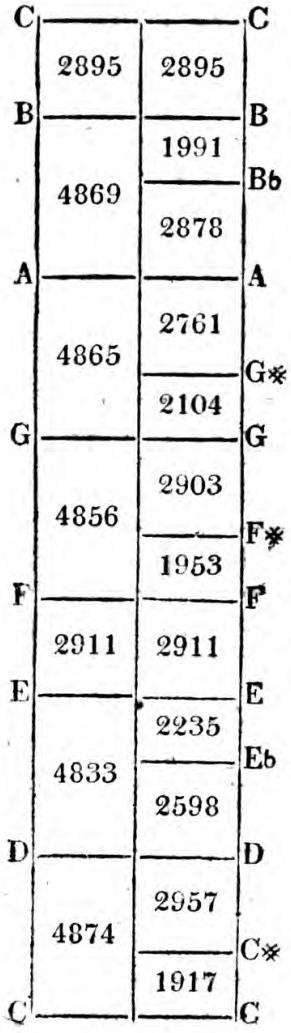


TABLE XII.

Bases.	Temperaments of the			Lengths of Strings.	Vibrations per Second.	Beats in 10 Seconds of the		
	Vths b	IIIds ♯	3ds b			Vths	IIIds	3ds
B	143	675	149	53446	449,04	44,0	352,8	92,4
B♭	105	69	1114	55954	428,92	30,8	34,0	155,2♯
A	138	10.b	154	59787	401,42	38,6	4,6b	85,2
G♯	387♯	833	288	63712	376,79	98,7♯	360,5	155,4
G	106	43	175	66874	358,88	26,4	17,6	86,8
F♯	160	954	150	71496	335,68	37,2	372,8	69,8
F	124	30	957	74786	320,92	27,6	10,8	143,0♯
E	108	180	151	79970	300,10	22,2	66,6	62,0
E♭	136♯	311	818	84194	285,06	26,6♯	102,2	186,6♯
D	144	6	174	89384	268,50	26,6	2,2	64,0
C♯	52♯	1009	128	95682	250,83	10,9♯	295,3	44,8
C	135	16	4[illegible]6	100000	240,00	22,4	4,0	147,0

Nothing in the above tables will need explanation, except the anomalous sharp beats of the 3ds, in the last column. These are derived from the perfect ratio 6 : 7, because these 3ds are, in reality, much nearer to the ratio of 6 : 7 than to that of 5 : 6; and hence could their beats be counted, they would be those of the table, and not those which would be derived from considering these 3ds as having flat temperaments of the ratio 5 : 6. But although the beats are slower, the nearer they approach the ratio 6 : 7, this ought not to be regarded as any sufficient reason for admitting so large temperaments into the scale, were it not absolutely necessary, in order to accommodate those 3ds which are of far more fre-

quent occurrence. Although the beats of these 3ds grow slower as their temperaments are increased, yet they are losing their character in melody; and become, in this respect, more and more offensive, the more they are tempered. Hence the harmony and melody of the several intervals, jointly considered, are to be judged of rather from their temperaments, in the three first columns, than from their beats, in the three last.

Scholium 1.

It will be perceived, from a comparison of the temperaments in Table XII. with the corresponding numbers in Table IX., that the harshness of the several concords, especially of the IIIds and 3ds, is, in general, nearly in the inverse ratio of their frequency. The contending claims of the different concords render it impossible that this ratio should hold exactly. Including the Vths, the harmony of the concords is much more nearly *equal*, than the principle of rendering the temperament of each inversely as its frequency, could it be carried into complete effect, would require.

Scholium 2.

The foregoing system may be put in practice, on the organ, by making the Vths beat flat, with the exception of those on C♯, E♭, and G♯, which must beat sharp, at the rate required in the table; proving the correctness of the temperaments of the Vths, by comparing the beats of the IIIds, as they rise, with those required by column two. Should less accuracy be required, the IIIds on C, D, and A, might be made perfect, without producing any essential change in the system. This would reduce the labour of counting the beats to eight degrees only.

Scholium 3.

To show that the computations of the different frequency of occurrence of the different concords, on which this system of temperament is founded, may be relied on as practically cor-

rect, for music in general, it may be proper to state, that a similar series of calculations had been before made, from an enumeration of the concords in fifty scores of music entirely different from that made use of in Prop. IV. They were not, indeed, made with the same accuracy, for the music of which the chords were counted, was too generally of the simpler kind, and the numbers corresponding to those in the two columns under each concord in Table II., and those belonging to the major and to the minor signatures, corresponding to the numbers in Table III., were added, before the products were taken, instead of keeping the modes distinct, which is necessary to perfect accuracy. Yet the resulting scheme of temperament was essentially the same throughout, with the one which has been just described. It had the same anomalous temperaments, viz. the Vths on C♯, E♭, and G♯; and the IIId on A; and these anomalies were similar in degree. The greatest difference between any two corresponding temperaments, was between those of the 3d on E♭; the first computation making it only 702, while the last has it 818.

Proposition XI.

The aggregate of dissonance, heard in a given time, in the system of temperament unfolded in the last Proposition, will be less than in either of the systems generally practised.

In order to compare the foregoing system with those which have been most generally approved, the temperaments of all the concords have been calculated, in the system of equal semitones; in that of Earl Stanhope, which has had considerable celebrity; in that of Dr. T. Young; in that of Mr. Hawkes; in that of Kirnberger, which has been extensively adopted in Germany; and in that which is described by Rousseau and D'Alembert as generally practised in France. If these temperaments be multiplied into the corresponding numbers of Table IX., agreeably to what was shown under Prop. VIII., and those products which belong to the several concords of the same name be added, the sums, after the three right-hand figures are cut off, will be as follows:

TABLE XIII.

Systems.		Mean Temp.	Young's.	Kirnberger's.	French.	Stanhope's.	Hawkes'.	New Scale.
Dissonance of the	Vths	309	494	681	561	595	665	810
	IIIds	2184	1541	1397	1346	1175	925	530
	3ds	2740	2448	2019	2121	1992	1676	1363
Total		5233	4483	4097	4028	3762	3266	2703

From an inspection of the sums at the foot of the table, it will be seen that the amount of dissonance heard in a given time is decidedly less in the new scale than in either of the others; and that it is scarcely more than half as great as in the scale of equal semitones. On the other hand, the temperament is very unequally distributed, which must be admitted, cæteris paribus, to be a disadvantage. It is even somewhat greater than in the scheme of Mr. Hawkes, although by no means in the same ratio, as the aggregate dissonance is less. It contains one Vth, which will be somewhat harsh, and four IIIds and three 3ds, which will be quite harsh. But these, as will appear from an inspection of Table IX., are, of all others, of by far the most unfrequent occurrence; so that the unpleasant effect of a transition from a better to a much worse harmony will be very seldom felt. In the six simplest keys of the major, and in the three of most frequent occurrence in the minor mode, they are *never* heard, except in occasional modulations; and even then, generally no one, and rarely more than one is heard. Now these nine keys, as will appear from Table III., comprise more than five times as much of the music examined as all the rest. The same remarks might be extended to three other minor keys, were it not that the sharp seventh is so generally used, that it deserves to be considered as an essential note of the key.

But there are two important considerations, more than counterbalancing the objection to this system, derived from the greater inequality in the distribution of its temperaments.

which have not been hitherto noticed, as not being susceptible of mathematical computation.

1st. We have gone on the supposition that tunes on the more difficult keys are as often performed, according to their number, as those on the simpler keys; and have taken for the measure of dissonance, in different systems, what would be actually heard, if the 1600 scores, whose signatures were examined, were all played in succession, and on the keys to which they are set. But the fact is, that those pieces which are set to the simpler keys are oftener played, and with fuller harmony, on account of the greater ease of execution, than those in which many of the short finger keys must be used.

2d. Pieces on the more difficult keys are often played on the adjacent easier keys, but the contrary is seldom or never done.

Giving to these two considerations no more than a reasonable weight, they will counterbalance the objection, and will render it evident that the sums under the several systems in the table may be taken as a true exhibition of their respective merits, without any injustice to the more equal systems at the left-hand of the table.

Cor. We may hence draw a comparison between the systems in common use. Their merits, when every consideration is taken into view, are nearly in the inverse ratio of the sums denoting their aggregate dissonance. That of Mr. Hawkes is the best, and, in many respects, has a remarkable analogy to the one derived from the preceding investigations.

Cor. 2. As the aggregate dissonance of the changeable scale is calculated on the same principles, in Prop. VIII., as that of the Douzeave in this, a comparison of the results in Table VIII. with those in Table XIII., will furnish us with the relative dissonance of different systems for these different scales. The relative dissonance of the two systems which form the object of this essay, is nearly as 17 : 27. Hence it appears, that by inserting eight new sounds between those of the common octave, the harshness of the music executed, at a medium of all the keys, may be diminished by more than one third of the whole, while the transition from a better to a worse harmony will never be perceived.

Art. XXII. *Notice of Colonel Trumbull's Picture of the Declaration of Independence.*

It is proper that some mention of this great national work should be made, in publications less transient than newspapers; and as the fine arts are included within the design of this Journal, it may with propriety be noticed here. This is the greatest work which the art of painting has ever produced in the United States. The picture is magnificent both in size and in execution. The dimensions of the canvass are eighteen feet by twelve.

"This picture forms one of a series long since meditated by Mr. Trumbull, in which it was intended to represent the most important events, civil and military, of the American revolution, with portraits of the most distinguished actors in the various scenes. The materials for this purpose were collected many years ago, and two plates have been engraved from paintings of the deaths of Gen. Warren and Gen. Montgomery;* but the work was suspended, in consequence of the political convulsions, which, during twenty-five years, were so fatal to the arts of peace.

"The government of the United States have ordered four of the subjects originally proposed by Mr. Trumbull, to be painted by him, and to be deposited in the capitol.

"No event in human history ever shed a more salutary influence over the destinies of so great a mass of mankind: the wisdom of no political act was ever so soon and so powerfully demonstrated, by such magnificent consequences. And justly may the nation be proud of the act itself; and of those eminent men, its authors, whose patriotism (rising above enthusiasm, and the passions which have so often bewildered mankind) was calm, dignified, persevering, and always under the guidance of reason and virtue.

* These pictures, as is well known, represent the assault on Quebec, and the battle of Bunker's Hill.

"The painting represents the congress at the moment when the committee advance to the table of the president to make their report.

"It contains faithful portraits of all those members who were living when the picture was begun, and of all others of whom any authentic representation could be obtained. Of a small number, no trace could be discovered; and nothing was admitted which was not authentic."

This picture is now, by permission of government, exhibited in the Academy of Arts in New-York, and will probably be shown in some of our other principal cities, before it receives its final location at Washington.

It exhibits the interior of the then Congress Hall at Philadelphia. Most of the members are represented as sitting in their respective chairs, or, in various instances, as standing in different parts of the room. Almost all the portraits were taken by Colonel Trumbull *from the living men*, and their accuracy may therefore be relied on.

The president, John Hancock, sitting at a table, and elevated somewhat by a low platform, is receiving the report of the committee declaring the independence of the colonies; that committee, individually illustrious, and in this august transaction collectively memorable, was composed of Franklin, Adams, Sherman, Jefferson, and Livingston. Mr. Jefferson, in the prime of life, is in the act of laying upon the table the great charter of a nation's liberties; while his companions support him by their silent but dignified presence, and the venerable Franklin, in particular, imposes new obligations on his country's gratitude.

The figures are as large as the life; and it may safely be said, that the world never beheld, on a similar occasion, a more noble assemblage. It was the native and unchartered nobility of great talent, cultivated intelligence, superior manners, high moral aim, and devoted patriotism. The crisis demanded the utmost firmness of which the human mind is capable—a firmness not produced, for the moment, by passion and enthusiasm, but resting on the most able comprehension of

both duties and dangers, and on a *principled* determination to combat the one and to fulfil the other.

This moral effect has been produced in the fullest and finest manner by this great painter; and no true American can contemplate this picture without gratitude to the men who, under God, asserted his liberties, and to the artist who has commemorated the event, and transmitted the very features and persons of the actors to posterity. Such efforts of the pencil tend powerfully also to invigorate patriotism, and to prompt the rising generation to emulate such glorious examples.

The composition and execution in this picture are in a masterly style. The grouping of so many full length portraits, in a scene in which there could scarcely be any action, and in such a manner as to dispose of them without monotony, was an attainment of no small difficulty. The painter could not even avail himself of the adventitious relief of splendid costume and furniture, and of magnificence or rich decorations in architecture; for on this occasion both were characterized by an elegant simplicity only, such however as became the actors and the crisis.

The composition has all the variety of which it is susceptible; and there is also enough of it in the style of dress and of features to relieve the eye from any danger of satiety.

It is believed, that in this picture, the United States possess a treasure to which there is no parallel in the world. In no instance, within our knowledge, is there an exhibition to an equal extent, of the actual portraits of an illustrious assembly, concerned in so momentous a transaction.

It was a great thing to assert, *in principle*, the liberties of this country; but it was also a great thing to vindicate them by arms; and we rejoice that Colonel Trumbull is still to proceed, under the sanction of government, to delineate other scenes, in which Washington and his illustrious American coadjutors, and the flower of French chivalry, were the actors. In the maturity of his experience, skill, and fame—possessed, as he is, of the portraits of most of the great men of that period, taken principally from the life, and having been him-

self largely and personally conversant with them in their great deeds, we trust that the government will promptly second what we doubt not the united voice of the nation will demand—that the illustrious artist should dedicate the evening of his life to his country's honour and glory.

INTELLIGENCE.

Art. XXIII. *An Address to the People of the Western Country.*

A NUMBER of the citizens of Cincinnati have recently instituted a society for the collection, preservation, exhibition, and illustration of natural and artificial curiosities, particularly those of the *western country*. The first efforts of the managers will be directed to the establishment of a permanent museum, on a scale so comprehensive as to receive specimens of every thing curious which they may be able to procure. In attempting to form this repository, they must of course solicit the aid of their fellow-citizens in all quarters of the extensive region, whose ancient works and natural history they propose to illustrate. The following are the classes of objects that will especially attract their attention, and to which they are desirous, at an early period, of directing the views of the community:

1. Our metals and minerals generally, including petrifactions.

2. Our indigenous animals, embracing the remains of those which are now extinct.

3. The relics of the unknown people who constructed the ancient works of the western country.

4. The various articles manufactured, for ornament or use, by the present savage tribes.

The subjects of the first class are considered by the Society as extremely interesting. Every citizen of the western coun-

try must *feel* the necessity of a speedy developement of its mineral resources. To find beneath our own soil an adequate supply of the various minerals which are now imported at an enormous expense, must be regarded by all as a matter of the first and greatest importance. The managers are anxious to be instrumental in the advancement of this useful work, and earnestly solicit the co-operation of the public. They will be thankful for specimens of all the rare or curious minerals that may be discovered in this country. To every specimen that may be transmitted, a label should be attached, stating either the kind of rock or stratum to which it belonged, or its precise locality. Whenever it is required, the managers will have a part of any specimen which is sent to them, analyzed, and a correct report made of its nature, thus affording to the discoverer a full opportunity of availing himself of all the pecuniary advantages that may attend the discovery.

As objects of scientific interest, the managers intend, as early as possible, to commence the formation of a cabinet of petrifactions. The rocks of few other countries contain a greater number and variety of these animal remains of the ancient ocean, than the limestone districts of the Ohio and Mississippi. They both astonish and confound most of the travellers through this region; and although objects of familiar examination to ourselves, they have not been collected or described by our citizens. An extensive and well arranged cabinet of these extraneous fossils would afford, both to the zoologist and geologist, an exquisite feast. It is hoped that every specimen sent to the Society will be accompanied by a label, stating the place where it was found.

It is the wish of the Society to obtain and preserve specimens of all the native animals of this country. Most of the larger quadrupeds having receded before the unceasing extension of our settlements, are now so rare as to be unknown to to all but our oldest emigrants. Measures will be taken by the managers to procure from the general retreat in the northwest, and exhibit to the people in the Ohio countries, a specimen of every quadruped which lately inhabited them; and while engaged in this enterprise, they hope to import from

the same distant wilderness, a variety of the animals which are peculiar to it.

Our native birds have not retreated, like our quadrupeds, and are, therefore, within our reach. The managers hope to see the Society, in due time, in possession of a large collection of these beautiful animals. In the accomplishment of this undertaking, it is easy to perceive that the Society may be powerfully aided by the community: and a sanguine hope is entertained, that no backwardness or indifference will be manifested by those who may fortunately have it in their power to forward specimens.

In collecting the fishes and reptiles of the Ohio, the Mississippi, and the Lakes, the managers will likewise need all the aid which their fellow-citizens may feel disposed to give them. Although not a very interesting department of zoology, no object of the Society offers so great a prospect of novelty as that which embraces these animals. The managers, therefore, flatter themselves that they will not be suffered to proceed unaided in this portion of their labours.

The obscure and neglected race of insects will not be overlooked, and any specimens sufficiently perfect to be introduced into a cabinet of entomology, will be thankfully received.

The western country, from having afforded some of the most gigantic and curious remains of land animals which have yet been discovered, seems entitled to a museum of such relics. A collection of this kind will be one of the earliest objects of the Society. Its funds will be liberally expended for the purpose; and if aided by those who may be so fortunate as to discover any of the great bones which lie buried in our alluvial or bottom lands, the managers hope, at no distant period, to repair, in some degree, the losses which have been repeatedly sustained by exportations of these interesting fossils.

The third class comprises objects of very little utility, but of extraordinary interest. Nothing, indeed, presented by the western country seems to excite in a higher degree the curiosity of strangers, than the relics and vestiges of the extinct and comparatively civilized population with which it abounds.

The managers will make every possible effort to form an extensive collection of these remains.

It is extremely unfortunate for those engaged in researches concerning the objects of this class, that so many of them have been disseminated abroad. To study them successfully, it is necessary that they should be compared, and for this purpose they must be brought together. The managers hope, therefore, that such persons as now hold, or may hereafter possess any of these antiquities, will dispose of them to the Society, instead of sending them out of the country. In this way, and in this only, can a valuable collection of these unique curiosities be formed.

The remaining class comprehends the weapons, utensils, trinkets, and other manufactures of our neighbouring Indians, of which the managers hope, in a short time, to be able to exhibit a great variety.

The curiosities of this country are the primary, but not the exclusive objects of the Society. It proposes in due time to open a gallery of paintings, and thus offer to the lovers and cultivators of the fine arts, a few of those models which are absolutely necessary to the gratification and improvement of their taste.

The managers will be happy, moreover, to receive from such of their eastern brethren as are desirous of contributing to the amelioration and advancement of a new and remote community, any of the productions of foreign countries that may be calculated to promote this object; and will, in return, cheerfully exchange any specimens of the curiosities of this country which they can spare without injury to their collection.

They will, if required, pay a reasonable price for every article which may be deemed worthy of introduction into the museum. They intend to publish, annually, a catalogue of all the more valuable donations which may be made to the museum, with the names of the donors.

ELIJAH SLACK,
JAMES FINDLAY,
WILLIAM STEELE,
JESSE EMBREE,
DANIEL DRAKE,
} *Managers.*

Cincinnati, Sept. 15, 1818.

Caleb Atwater, Esq. of Circleville, Ohio, is engaged in writing Notes on the State of Ohio, a work which is intended to embrace the most important features and interests of this new and rising State.

To this laudable effort, and to that of the Western Museum Society, whose address is published above, we cordially wish success. From the zeal, talent, and industry of the gentlemen concerned, we have every reason to expect a happy result.

We view, with much satisfaction, the efforts which have been already made, and are rapidly increasing, to bring to light the resources, and to develope the history, of the western States; and it will always give us pleasure, if through the medium of this Journal, or in any other manner, we can contribute to promote them.

Art. XXIV. *Extract of a Letter from Colonel Gibbs to the Editor.*

Sunswick, June, 1818.

Dear Sir,

Since I saw you, I have made only one experiment on magnetism. I determined the power of my magnet, as it had been shut up in the dark for a long time, and lying down. I then exposed it to the rays of the sun, also lying down, and remote from the iron support, and I found that it had gained 12 oz. power in 40 minutes, and 14 oz. power only in five hours.

Art. XXV. *A New Lamp, without Flame.*

From the Annals of Philosophy for March, 1818. Communicated by Mr. Thomas Gill.

This lamp is one of the results of the new discoveries in chemistry. It has been found, by Sir H. Davy, that a fine platina wire, heated red hot, and held in the vapour of ether,

would continue ignited for some time; but, I believe, no practical use has been made of this fact.

If a cylindrical coil of thin platina wire be placed, part of it round the cotton wick of a spirit lamp, and part of it above the wick, and the lamp be lighted, so as to heat the wire to redness; on the flame being blown out, the vapour of the alcohol will keep the upper part of the wire *red hot*, for any length of time, according to the supply of alcohol, and with little expenditure thereof; so as to be in constant readiness to kindle German fungus, or paper prepared with nitre, and, by this means, to light a sulphur match at pleasure. This lamp affords sufficient light to show the hour of the night by a watch, and to perform many other useful services; but does not hinder the repose of persons unaccustomed to keep a light burning in their bed-room, nor does it require to be snuffed.

The proper size of the platina wire is the $\frac{1}{100}$th part of an inch: a larger one will only yield a dull, red light, and a smaller one is difficult to use. About 12 turns of the wire will be sufficient, coiled around any cylindrical body suited to the size of the wick of the lamp; and four or five coils should be placed on the wick, and the remainder of the wire above it; and which will be the part ignited. A wick, composed of twelve threads of the ordinary sized lamp cotton yarn, with the platina wire coiled around it, will require about half an ounce of alcohol to keep it alight for eight hours.

An agreeable and slightly acid smell arises from this lamp during its ignition. It is perfectly safe, as nothing can fall from it; and its novel appearance, in a wick's keeping red hot for such a length of time, is very surprising to persons unacquainted with its nature.

P. S.—When the wire has become oxided, it will be necessary to uncoil it, and rub it bright again with fine glass-paper; which will cause it to act again with increased effect.

REMARK.

Such wire as is here described may, probably, be obtained in Philadelphia.

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THE

AMERICAN

JOURNAL OF SCIENCE, &c.

GEOLOGY, MINERALOGY, TOPOGRAPHY, &c.

ART. I. *Hints on some of the Outlines of Geological Arrangement, with particular Reference to the System of Werner, in a letter to the Editor, from* WILLIAM MACLURE, *Esq. dated Paris, 22d August*, 1818.

INTRODUCTORY REMARKS.

SOME years since, during Mr. Maclure's geological survey of the United States, the editor had the pleasure of passing a few days, in company with that gentleman, in exploring the geology of the vicinity of New-Haven. Near that town, junctions, on an extensive scale, between widely different formations, are to be observed. A radius of ten miles, with New-Haven for a centre, will describe a circle within which the geological student may find (with the exception of formations, unquestionably volcanic) most of the important rocks of the globe, and a radius of even six or seven miles will include the greater number of these. At, and near the terminations of the primitive ranges, there are rocks which appear to have, in a high degree, the characters of the transition class. Among them is the beautiful green marble of the Milford Hills, seven miles from New-Haven. Mr. Maclure visited that district,

and even suggested the first hint which afterward led to the discovery of the marble. Doubts being entertained concerning some of the geological relations of those rocks, a letter was addressed to Mr. Maclure (then in Philadelphia) on the subject. His answer is subjoined.

In giving it to the public, the editor takes a liberty which he hopes the respectable author will pardon, because his production, although evidently never intended for the public eye, contains statements and opinions of no small importance to the young geologist, especially of this country.

Geology, at the present day, means not a merely theoretical and usually a visionary and baseless speculation, concerning the origin of the globe; but, on the contrary, the *result of actual examination into the nature, structure, and arrangement of the materials of which it is composed.* It is therefore obvious, that the opinions of those men, who, with competent talent and science, have, with a direct reference to this subject, explored many countries, and visited different continents, are entitled to pre-eminent respect. SAUSSURE, by his scientific journeys among the Alps, (although a limited district) has given deserved celebrity to his own name, and, if it were possible, has thrown an additional charm of attraction over those romantic and sublime regions. Dolomieu has made us familiar with the productions and phenomena of volcanoes, those awful and mysterious laboratories of subterranean fire. Humboldt has surveyed the sublimest peaks of both continents, and examined the structure of the globe amidst the valleys of Mexico and the snows of Chimborazo and Pinchinca; and Werner, with opportunities much more limited, (confined indeed to his native country, Saxony) but with astonishing sagacity and perseverance, deduced from what he saw, a classification of the rocks of our globe, which, although not perfect, has done immense service to the science of Geology. In this distinguished group (to which other important names might be added) Mr. Maclure has unquestionably a right to be placed. Few men have seen so much of the structure of our globe, and few have done so much with such small pretensions. His work on American Geology is noticed with becoming respect

even in Edinburgh,* that focus of geological science. His opinions on some of the more obscure and doubtful parts of the Wernerian geology are worthy of peculiar consideration; for they are founded on a course of observations vastly more extensive than Werner ever had it in his power to make. The name of Werner will always be venerated as long as geological science shall be cultivated, for geology owes more to him than to any other man; but his pupils should not now demand that implicit and unqualified adoption of ALL his opinions, which will allow no other question to be raised, than what Werner taught or believed.

With these explanatory remarks, the following extract of Mr. Maclure's letter is now subjoined:

DEAR SIR,

Your letter of the 26th June came just as I was embarking for Europe. The information it requires concerning the primitive trap and flint slate, the transition and secondary rocks, &c. &c. is difficult to give without the aid of specimens, and frequently requires the examination of the relative position of the strata before any correct idea can be formed. I will, however, endeavour to give you the little my experience has brought me acquainted with.

Following the nomenclature of Werner, I have given a list of his rocks; but in describing them there are many of his names which I do not use; because I never met with them. Primitive trap is one instance—I do not use trap as a substantive, except in describing that kind of trap which Werner calls the newest flætz trap, the nearest to which is your trap,† which covers the oldest red sandstone.

The primitive flint slate is in the same predicament. I have always found it on the borders of the transition, between it and the secondary.

Primitive gypsum I have not found.

* Vide Edin. Review for Sept. 1818. p. 374.

† Referring to the ridges of Greenstone near New-Haven.

What Werner calls primitive trap may perhaps be compact hornblende, or perhaps the newest flætz trap, when it happens to cover the primitive; for, this species of trap, like the currents of lava, covers indiscriminately all classes of rocks, and is one reason why I consider it as the remains of ancient lava.

Transition trap is a rock that I have not met with, and may perhaps be a part of the flætz trap that happened to cover the transition, without any immediate connexion, but like a current of lava, overlying all the classes of rocks it meets with. This misapplication of names naturally arises from the system of neptunian origin, on which the nomenclature of Werner is founded.

Greywake and greywake slate are aggregates of rounded particles of rocks, evidently the detritus of more ancient formations, and differ from the aggregates of pudding and sandstone of the secondary class, in the following properties, viz.

The aggregates of transition are harder and much more compact, than the secondary; they are also cemented by argil, taking a slaty form.

This cement is in much greater quantity, in proportion to the particles cemented, and has the appearance as if the cement at the time of formation, had a consistence sufficient to prevent the particles from touching each other.

They have, in common with all the transition rocks, a regular and uniform dip from the horizon, from 10 to 40 degrees; and sometimes more. This is perhaps the strongest mark of distinction which separates them from the secondary, which are horizontal, or follow the inequalities of the surface on which they were deposited.

The transition are distinguished from the primitive in being aggregates of rounded particles, having little or no crystallization, and containing, or alternating with strata, which contain organic matter.

The oldest red sandstone, with all its accompanying strata, I should incline to put into the transition, as having many of the properties of that class, and occupying the same relative situation in the stratification of the globe. It is at a constant

dip (although small) from the horizon; the cement is in greater quantities in proportion to the particles cemented than in any of the secondary aggregates, &c. &c.

The character of the secondary is a horizontal position, that perhaps does not admit of the same facility of examining the relative situation of its stratification. The compact limestone is, probably, with reason, considered as the lowest of the secondary formation, and always under the coal formation, but it appears to me that the secondary is deposited in basins alongside of one another, and that each basin has a different order of superposition, according to the nature of the agents employed in the deposition; that it is a partial, and by no means a general deposition. The secondary aggregates of sandstone and puddings have been evidently beds of sand or gravel, and of course, in that state would be called alluvial, but when cemented together by the infiltration of water, carrying along with it lime, iron, or any other body capable of agglutinating the particles together, become rocks, and may alternate in all proportions.

I am therefore inclined to think, that in geology the best mode for the greatest part of the secondary would be to give the relative position of the strata of each valley or basin; and I am rather of opinion that they would all differ from one another.

The French and English basin having chalk for the lowest stratum, which has occupied the geologists of both countries for these 10 or 15 years, is perhaps the best known; yet they do not know the relative position of the chalk and coals, because coals have not been found in the same basin with chalk; coals occupy basins filled with different kinds of rocks, and have no resemblance to the rocks found covering the chalk.

ART. II. *On the Geology, Mineralogy, Scenery, and Curiosities of Parts of Virginia, Tennessee, and the Alabama and Mississippi Territories, &c. with Miscellaneous Remarks, in a letter to the Editor.* By the REV. ELIAS CORNELIUS.

To Benjamin Silliman, Professor, &c.

SIR,

HAVING recently returned from a tour of considerable extent in the United States, I avail myself with pleasure of the first leisure moment, to communicate, agreeably to your request, some facts, relative to the Mineralogy and Geology of that part of the country through which I passed.

INTRODUCTORY REMARKS.

Before doing this, you will permit me to premise, that in consequence of my limited acquaintance with these branches of Natural Science, and the still more limited time, which other and important concerns allowed me to devote to the subject, I can do little more than give a general description. What my eye could catch, as I travelled from one country and wilderness to another, preserving occasionally a few of the most interesting specimens, was all I could do. The specimens you have received. The narrative I am about to give, is drawn principally from the notes which were taken on the journey, and will be confined to a simple *statement of such facts* as were either observed by myself, or derived from good authority. Their application to preconceived theories, I leave to those who have more leisure and disposition for speculation than myself.

A description of a few natural and artificial curiosities which came under particular notice, will not, I trust, be thought an improper digression. The whole is committed to your disposal; and if it shall add but one mite to the treasury of Ameri-

can Natural History, I shall be gratified, and rejoice to have made even this small remuneration for your unwearied efforts, to impart to one, formerly your pupil, a love for Natural Science.

The Author's Route.

My route was in a line nearly direct from Boston to New-Orleans; passing through the principal cities to Washington; thence, diagonally, through Virginia, East Tennessee, and the northwestern angle of Georgia; in a western course through the north division of the Territory of Alabama, to the north-eastern boundary of the State of Mississippi; and thence in a line nearly southwest to Natchez. From this last place I descended the river Mississippi to New-Orleans. On my return I frequently varied from this course, and had increased opportunities for surveying the country. In both instances I passed through the countries belonging to the Cherokee, Chickesaw, and Choctaw tribes of Indians, and travelled among them, in all, about one thousand miles.

Geology of Virginia.

As others have described more minutely and accurately than I can, the country north of Virginia, I shall begin with a few remarks on the geological character of that State. It is there that the traveller, in passing from the Atlantic to the interior, crosses successively the most important formations of the earth, from the most recent alluvial to the oldest primitive. For a considerable distance from the coast, the country is alluvial. It then assumes an older secondary formation*—and sandstone and puddingstone are frequent. This is the character of the District of Columbia, and indeed of a great part of the valley of the Potomac.

Sandstone of the Capitol, &c.

In this valley, and adjacent to the river, is found the *sandstone* of which the President's house, and the Capitol are con-

* Or, according to the Wernerian Geologists, Transition? *Editor.*

structed. It is composed of fine silicious grains, is easily wrought, and from its colour, has the appearance at a small distance of white marble.

Beautiful Breccia.

It is also in the valley of this river, and not far from its famous passage through the Blue Ridge, that immense quarries of beautiful Breccia have been opened. This rock was first brought into use by Mr. Latrobe, for some years employed by the government as principal architect. It is composed of pebbles, and fragments of silicious and calcareous stones of almost every size, from a grain, to several inches in diameter, strongly and perfectly cemented. Some are angular, others rounded. Their colours are very various, and often bright. Red, white, brown, gray, and green, are alternately conspicuous, with every intermediate shade. Owing to the silicious stones which are frequently imbedded through the mass, it is wrought with much difficulty; but when finished, shows a fine polish, and is unquestionably one of the most beautifully variegated marbles, that ever ornamented any place. It would be difficult to conceive of any thing more grand than the hall of the Representatives, in the Capitol, supported as it is by twenty or thirty pillars formed of the solid rock, and placed in an amphitheatrical range; each pillar about three feet in diameter, and twenty in height. Some idea of the labour which is employed in working the marble may be formed from the fact, that the expense of each pillar is estimated at five thousand dollars. The specimens in your possession, are good examples of its general structure, but convey no adequate idea of its beauty.

Petrifaction of Wood.

It will be proper to notice in this place, a petrifaction of wood which is found on the road from Washington to Fredericksburgh, 16 miles from the latter, and four miles north of the court-house in Stafford county. It is remarkable for its size, rather than for any singularity in the composition. It was

found by digging away the earth on the side of the road, and appears to have been the trunk of a considerable tree. It is firmly fixed in the ground, and penetrates it obliquely; how far has not yet been ascertained. At the time I saw it about two feet had been exposed. The diameter is about eight inches. Its colour is white, sometimes resembling that of wood. The fibres are well preserved, and so is the general structure. It is much to be desired, that some one would clear it from its bed, and give it entire to one of our mineralogical cabinets.

Geological Features.

Next to the alluvial and secondary formations, as you pass to the west and northwest, are to be found ranges of granite and shistose, and other primitive rocks; interspersed with these may be seen sandstone, clay, slate, quartz, and limestone. Granite ranges were particularly seen in the neighbourhood of Fredericksburgh, crossing the Rappahannock; and in Orange and Albemarle counties, extending nearly to the Blue Ridge. Great quantities of quartz and quartz rock, sometimes covering with their fragments the sides of hills, are frequent. Another, and more interesting rock in the same connexion, is found in Albemarle county. For some time I doubted to what class to refer it. But from its resemblance to the rocks of the east and west mountains near New-Haven, I ventured to call it trap or whinstone. It becomes more abundant as you approach the Blue Ridge, and the granite disappears. On the sides and summit of the mountain, its appearance is more decidedly that of greenstone. In crossing the southwest mountain, the range to which Monticello belongs, and distant from the Blue Ridge about 25 miles, I observed the same rock. Whether this opinion is just, you will be able to decide from the specimens which have been forwarded.

Blue Ridge.

I have repeatedly named the Blue Ridge. It is the first of those long and parallel ranges of mountains, called the Alle-

ghany; and constitutes one of the most prominent features in the geology of the United States. Its height I cannot determine with accuracy. Probably it would not average more than one thousand feet. Its base may extend in diameter from one to two miles; and yet such is the influence it has on the climate, that vegetation on the eastern, is usually two weeks earlier than on the western side. And what is remarkable, this difference obtains, on the former side at least, until you arrive within a few hundred yards of the summit. I crossed the mountain in two places, distant from each other one hundred miles, but observed nothing essentially different in their mineralogy. At one of them called the *Rockfish-Gap*, on the road from Charlotteville to Staunton, I spent a few hours, and brought away specimens of all the varieties of minerals which I could find. These have been submitted to your inspection. Among them, you will, I think, see greenstone, epidote, and slate more or less allied to the first. These are the most common rocks, and excepting the second, are usually stratified. The epidote is generally associated with quartz, and sometimes is imbedded in it. In some instances it has a porphyritic appearance, and is very beautiful. In others, it is coated with small filaments of a greenish asbestos. Other minerals were found, whose nature I could not so easily determine. I regret exceedingly, that I cannot furnish you with a more complete description of this interesting mountain. That its character is peculiar, or different from the country on either side of it, must be obvious to the most superficial observer. Its principal rock does indeed bear a resemblance to the trap or whinstone of Albemarle county, and yet I think you will say it is not the same. One fact of importance cannot be mistaken; this mountain constitutes the great dividing line between the granite and limestone countries. For you no sooner reach its western base, than the greenstone and epidote disappear; and *limestone* pervades the country for hundreds of miles in every direction. In all the distance from this mountain to New-Orleans, I did not find a single specimen of granite, or greenstone. This may appear singular, since Mr. Maclure and Professor Cleaveland have a granite range on their maps, immediately west of the

Blue Ridge; and even that mountain is on those maps, in some parts of it, covered with the granitic tinge. This may be true. I can answer for only two points of it, and for that part of the country beyond, lying near the main road to Tennessee. In this route I descended almost the whole length of the great valley included between the Blue Ridge on the east, and the north mountain on the west. But in no instance did I meet with specimens of granite; nor west of the Blue Ridge with any prevailing rock but limestone. I know of no reason why the Blue Ridge should not be regarded as the first great dividing line between the granite and limestone countries. The change in the geological formation is so sudden and striking, that it would be difficult for the most careless traveller with his eyes open, not to observe it. The face of nature, he cannot but perceive, wears a different aspect; the air is more cool and lively; even the water which he drinks possesses new properties perceptible to his taste. The inhabitants no longer speak of their "sandstone water;" but every where he hears of "limestone water." Indeed for 800 miles in the direction which I travelled, he tastes no other water. Every spring and every rivulet, is strongly impregnated with carbonate of lime. The vessels in which it is prepared for culinary use, soon become lined with a white calcareous crust. Nor is its taste the only inconvenience experienced by the traveller unaccustomed to it. It often injures the health of a stranger, and covers the surface of the body with cutaneous eruptions.

Limestone country in inclined Strata.

The geological observer has now entered upon a very interesting field. Its great extent, and its wonderful uniformity, give new facilities to investigation. Two divisions of it seem to have been made in nature.

The *first* is that which includes the limestone lying in INCLINED STRATA. This division extends from the Blue Ridge, to the Cumberland mountain in East Tennessee, a distance in the direction of my route of 500 miles. Of course it includes all the ranges, five in number, of the Alleghany mountains. The

strata lie in a course northeast and southwest, the same as the general course of the mountains. The angle which they make with the horizon is very variable, from 25° to 45°. The colour of the rock varies from blue, and pale blue, to gray, or grayish white, frequently it presents a dull earthy appearance. The fracture is more or less conchoidal. Sometimes the rock assumes a different character, and the fracture is *uneven*, and the texture firm. This last is distinguished from the former, not only by the fracture, but by the colour. It is usually spoken of by the inhabitants as the *gray limestone*, the colour of the other being usually of a bluish cast. It differs from that also by being less brittle, and possessing the quality denominated by stonecutters, "*tough*." In consequence of this, and its enduring heat better, it is more frequently used in building than the other. This variety of limestone is not uncommon. Its colour is not always *gray*, sometimes it is a reddish brown, and sometimes white. Immense quantities of it, possessing either a grayish or reddish brown colour, are found in the vicinity of Knoxville, East Tennessee. One range of it is crossed by every road, passing to the south and east of Knoxville. Its appearance is that of some variegated marbles; white veins penetrate it, and wind through it in every direction. Whether any part of it has a texture sufficiently fine and firm to be wrought to advantage, is yet to be determined. To the eye of a superficial observer, there are many indications that it has. A specimen of very fine white marble, resembling the Italian white, was shown me in Augusta county, Virginia, which was found 15 miles from Staunton, where there is said to be a considerable quantity of it.

Limestone country in Horizontal Strata.

The *second* great division of the limestone country extends, on the route which I took, two hundred miles from the Cumberland mountain, and others associated with it southwest, as far as the Dividing Ridge, which separates the waters flowing into the Tennessee from those which proceed direct to the gulf of Mexico. The grand circumstance which distinguishes

the limestone of this division from that already described, is this, ITS STRATA ARE HORIZONTAL. Frequently immense piles may be seen forming bold precipices, but *always* in horizontal layers, differing in thickness, from a few inches to many feet. How far this arrangement extends to the west and north, I have not yet been able to learn. Travellers always speak of the limestone rocks in West Tennessee and Kentucky as *flat*, from which circumstance I conclude that the Cumberland mountain forms for a considerable distance at least, the eastern boundary. I have observed but three other particulars in which the strata of the *horizontal* differ from those of the *inclined* limestone.

1. Its colour is not so strongly marked with the bluish tinge.

2. It is not so commonly penetrated with white veins of a semicrystallized carbonate of lime; nor is it so frequently associated with the *uneven* fractured species.

3. Petrifactions are oftener found in it.

I will here take the liberty to suggest, whether in our maps of geology, some notice should not be taken of this very important division in the limestone country. Such a division exists *in fact;* nature has made it; and if geology depends on nature for its only legitimate inductions, there can be no reason why a feature so prominent as this, should be overlooked. I shall not undertake to account for their difference: but would not every geological theorist consider them as distinct formations?*

Cumberland Mountain.

The Cumberland mountain, which forms a part of this dividing line, is itself a singular formation. It belongs to the class called "Table mountains." Its width varies from a few

* The modesty of the writer has prevented him from applying to the formations which he has well described, the terms *transition* and *secondary*, which there can be little doubt do in fact belong to them. His strata of highly inclined limestone, appear to belong to the transition class of Werner, and his flat strata, to the secondary. It may be observed in this place, that the specimens alluded to in the text (passim,) appear to be correctly described by Mr. Cornelius, and to justify his geological inferences as far as hand-specimens seen at a distance from their native beds, can form a safe basis for general geological inductions. *Editor.*

miles, to more than fifty. Its height is not perceptibly different from that of the Blue Ridge. It forms a circuit, in a shape somewhat resembling a half moon. Winding to the southwest, it keeps a course north of the Tennessee river, in some places nearly parallel with it; passes a few miles to the southeast of Huntsville in the Alabama Territory, and not long after terminates. At one part, over which I crossed, the mountain is eighteen miles wide. This is about 150 miles southwest of Knoxville, a little north of the 35th degree of N. Lat. I had not ascended the mountain more than half way, before I found sandstone begin to intermingle with limestone strata. As I drew near the summit, the limestone disappeared entirely, and sandstone prevailed in abundance, with no other mineral associated until I reached the western descent, where I met bold precipices of horizontal limestone, reaching from the base to the summit. I examined several sandstone rocks while crossing the mountain, found them usually imbedded in the earth, generally with flat surfaces, of a fine grain, and strong texture. The colour is usually a reddish brown, or grayish red. The specimen which you have received is a good example. I crossed this mountain in the vicinity of Huntsville, not less than one hundred miles southwest of the place above-mentioned, and found it not wider than mountains commonly are. Its height had also become less, and horizontal limestone in regular strata prevailed in every part.

Although this mountain forms a part of the dividing line which has been mentioned, it does not exclusively so: for the Rackoon mountain, which crosses the Tennessee river, at the place so well known by the name of "the Suck," and the Look-Out mountain, which terminates abruptly about 6 miles to the left of "the Suck," form an acute angle with the Cumberland, and are composed of horizontal strata of limestone. Thus it would appear the line which divides the two kingdoms of this rock, is nearly north and south, inclining perhaps a few points to the east and west.

Scenery.

And here I cannot forbear pausing a moment to call your attention to the grand and picturesque scenery which opens to the view of the admiring spectator. The country is still possessed by the aborigines, and the hand of civilization has done but little to soften the wild aspect of nature. The Tennessee River, having concentrated into one mass, the numerous streams it has received in its course of three or four hundred miles, glides through an extended valley with a rapid and overwhelming current, half a mile in width. At this place, a group of mountains stand ready to dispute its progress. First, the "Look-Out," an independent range, commencing thirty miles below, presents, opposite the River's course, its bold and rocky termination of two thousand feet. Around its brow is a pallisade of naked rocks, from seventy to one hundred feet. The River flows upon its base, and instantly twines to the right. Passing on for six miles further it turns again, and is met by the side of the Rackoon mountain. Collecting its strength into a channel of seventy yards, it severs the mountain, and rushes tumultuously through the rocky defile, wafting the trembling navigator at the rate of a mile in two or three minutes. This passage is called "The Suck." The summit of the Look-Out mountain overlooks the whole country. And to those who can be delighted with the view of an interminable forest, penetrated by the windings of a bold river, interspersed with hundreds of verdant prairies, and broken by many ridges and mountains, furnishes in the month of May, a landscape, which yields to few others in extent, variety or beauty. Even the aborigines have not been insensible to its charms; for in the name which they have given to the Look-Out mountain we have a laconic, but very striking description of the scenery. This name in the Cherokee language, without the aspirated sounds, is "O-tullēē-ton-tannâ-tâ-kunnâ-ēē;" literally, "mountains looking at each other."

I have already remarked that the limestone of this mountain lies in horizontal strata: one mile east from its base it is

inclined. Like the Cumberland, it contains immense rocks of sandstone, but of a coarser grain, verging occasionally into pudding stone. I was told by a white man, a professed millwright, that among these sandstone rocks, he knew of many which were suitable for millstones. At the missionary establishment, called "Brainerd," eight miles east of the mountain, I saw one of them which was used for this purpose to much advantage. It is composed of fine and large grains of silicious stones, nearly white, and resembling pebbles of white quartz: the texture is firm.

Silicious Minerals, &c.

I will now notice an important fact, applicable to the whole extent of limestone country, which has come under my observation. It is its association with a description of minerals, all of which appear to be *silicious*. To describe them minutely, would require several pages. From the time I entered the limestone country till I left it this association was observed. The minerals included in it differ much in their external character. Their size varies from that of rocks to the smallest fragments. Usually they lie loose upon the earth, in angular forms, having the appearance of a stone that has been broken in pieces by the hammer. Sometimes they cover the sides of hills and mountains in such abundance as to prevent or impede vegetation. When the disintegration is minute, they are serviceable rather than otherwise; and the farmer talks of his "good black," or "white gravel land." It renders this service, I presume, not by decomposition, but by preventing the soil and its manure from being washed away. Indeed the different varieties of it are generally scattered over the surface, in pieces so small, that for convenience sake, the whole may be denominated a *silicious gravel.*

Sometimes the mineral is imbedded in limestone, in the form of nodules, thus indicating their original connexion with it.

The varieties, so far as I have observed, are quartz, hornstone, flint, jasper, and semi-opal; and several, which to me are non-descripts. *Quartz* is the most abundant. It is found of different colours; compact, and porous or cellular; of every size; simple and associated with other silicious stones; massive and crystallized. In Augusta and Rockbridge counties in Virginia, beautiful crystals of quartz, of a singular form, are found. They are six-sided prisms, with double acuminations, that is, with six-sided pyramids, mounted on the opposite ends of the prism. A specimen of two such crystals united, you have received. It was found near Lexington. A curious variety of the quartz gravel-stone occurs on both sides of Elk River, a few miles above its junction with the Tennessee, in the Alabama territory. As you travel to the west from Huntsville, it appears first in the neighbourhood of Fort Hampton, two miles east of Elk River, and may be seen for ten miles west of that river. The mineral is remarkable for containing a curious petrifaction. Its first appearance is that of a solid screw. On examination, however, you find it is not spiral; but consists of parallel concentric layers. Their diameter varies in different specimens, from that of a pin to half an inch. They stand in the centre of a hollow cylinder, extending its whole length, and occupying about one-third of its dimensions. The stone is sometimes perfectly filled with these forms. The petrifaction I could not have named, had you not pronounced it the "Entrochite."

Hornstone, next to quartz, is the most abundant of the silicious minerals associated with limestone. It is very often seen imbedded in rounded masses, both in the inclined and horizontal strata.

Flint is more rare. Several fine specimens were observed on the western declivity of the Look-Out Mountain, but in no instances in large masses or quantities.

Semi-Opal was found in one instance on the dividing ridge, which constitutes the southwestern boundary of the limestone strata.

Of the non-descripts you have several specimens. One variety strikes fire with steel, is a milk-white colour, adheres

slightly to the tongue, and has no degree of translucency on its edges. As Mr. Kain has furnished you with an interesting detail of particular minerals found in East Tennessee and Western Virginia, I need not recapitulate what he has so well said.

(*To be continued.*)

ART. III. *Notice of the Scenery, Geology, Mineralogy, Botany, &c. of Belmont County, Ohio, by* CALEB ATWATER, *Esq. of Circleville.*

BELMONT COUNTY is bounded on the north by Jefferson and Harrison, on the west by Guernsey, and south by Monroe county, and on the east by the Ohio river. It is 27 miles in length, and 21 in breadth, containing 535 square miles. Its name, *Belmont*, or beautiful mountain, indicates its situation, for it contains within its boundaries a fine body of land, rising gradually as you are travelling from the Ohio to the west, until you arrive at about the middle of it, where, from the elevation on which you stand, the eye in an eastern direction, beholds one of the most charming prospects in the state. Looking towards the east, in a pleasant morning, you behold a beautiful country of hill and dale spread out before you, divided into convenient and well-cultivated farms, intersected by glittering streams, meandering through them towards the Ohio. You hear the lowing of numerous herds around you, the shrill matin of the songsters of the forest, and the busy hum of the industrious husbandman; you see here and there a clump of trees interspersed among the cultivated parts of the country; you see the comfortable dwelling-house, the substantial barn, and hear the rumbling noise of the mill; and when you reflect that those who dwell here are industrious and enterprising, virtuous, free, and happy, you behold with pleasure, and listen with delight, while reflecting on the objects around you.

Geology and Mineralogy.

On the surface is seen a rich vegetable mould, made by the decay and putrefaction of vegetable substances. Along the Ohio, a wide intervale of the richest alluvion is found, which produces as luxuriant a growth of vegetation as any in the world. On the banks of the creeks which pass through this country the alluvial soil is not so wide as that on the river, but equally rich and productive. On the hills (and there are many of them) there are two kinds of soil, the silicious and the argillaceous, the first is formed from the decomposition of the rocks which once covered the surface, the latter from the slate which lay under them. Where these rocks are decomposed, and the country is hilly, it will readily be believed that the two kinds of soil are frequently blended together. In some places we see the best of clay for bricks; whilst in other places, and those in the vicinity of the former, we find the best of sand to mould them in when manufactured. Hard limestone of the very best quality is found in detached fragments in the sides of hills, and in strata, in abundance, along the beds of streams.

The ruins of the sandstone formation are here seen scattered about in fragments, or decomposed and intimately blended with those of other formations.

Fossil coal is every where found under the hills, of the very best quality, and in sufficient quantity not only for the fuel of the present, but many future generations, and is so easily obtained that the expense of fuel is a mere trifle. The oxide of iron, or iron ore variously combined, is recognized in many places, and water combined with muriate of soda, or common salt, is as common. Salines or licks are found in many places, where animals also, both wild and domesticated, resort in great numbers to drink the waters. These are frequently near some little water-course. Several sulphur and chalybeate springs are known to exist in this county, and some which throw out considerable quantities of petroleum.

In a country where iron and fossil coal exist, it is no wonder that copperas should be found. There are places where cop-

peras exudes in a state sufficiently pure in quality, and in quantities sufficient for several families, who collect and use it in dying. The same may be said of alum, which is collected in the same way for similar purposes.

Botany.

Though this county is very rich in the mineral, yet it is not less so in the vegetable kingdom, as may be seen by a reference to the subjoined catalogue, although numbers of trees, shrubs, and plants, are purposely omitted, which are known to exist here.

Family.	*Species.*	*Classical name.*	*Remarks.*
Oak,	White,	Quercus Alba,	Abundant.
———	Black,	——— Nigra,	Do.
———	Meadow,	——— Aquatica,	Along the streams.
———	Chesnut,	——— Prunus,	Scarce.
Maple,	Sugar,	Acer Saccharinum,	Abundant.
———	White,	——— Alba,	
Poplar or Tulip,	White,	Liriodendron,	Abundant.
———	Yellow,		
Walnut,	Black,	Juglans Nigra.	
———	White,	——— Alba.	
———	ShellbarkHickory,	——— Albaovata.	
———	Pignut,	——— Minima.	
———	Bitternut, and probably several other species.		
Beach,	Two species,	Fagus.	
———	Chesnut,	——— Americana.	
Ash,	White,	Fraxinus Alba.	
———	Blue,	——— Purpurea.	
———	Black,	——— Nigra.	
———	Swamp,	——— Aquatica.	
Elm,	Two or three species.	Ulmus.	
Buckeye,	Common,	Æsculusflava Lutea?	
———	Sweet,	——— Maxima?	
Locust,	Four species,	Robinia Pseud Acacia, &c.	
Persimmon,		Diospyros Virginica.	
Linn or Bass Wood,		Tilia Europea.	

Family.	*Species.*	*Classical name.*	*Remarks.*
Cucumber,		Cucuminis Sylvestris.	
Dog Wood, or American Box,	Two species.		
Sycamore,	Two species,	Platanus Occidentalis, &c.	
Plum,	Several species.		
Thorn,	do. do.		

The red bud; the pawpaw; grape-vines of several species, and growing to a great size; sassafras; the black willow, confined to the streams; the box elder, the common elder, of two species; the sumach, of two species; several species of gooseberries; and a great many others too numerous to be mentioned here. Among the herbaceous plants we must not omit the ginseng, the Virginia snakeroot, the columbo, and the puccoon, two or three thousand pounds of the roots of which are annually carried by the inhabitants to our Atlantic cities. Among the trees, those belonging to the oak family are the most numerous, if not the most valuable. Split into rails, the farmer builds fences with them, and sawed into plank, boards, and scantling, they furnish materials for houses and barns. The sugar maple is sufficiently abundant, so that brown sugar enough is manufactured for domestic purposes. The sycamore is the largest tree along the river, and the poplar is the largest on the hills. The latter grows by the side of the maple and the beach, and is a most valuable wood for the house-builder and the cabinetmaker. This tree is frequently four and five feet in diameter, and continues of nearly the same size as it ascends, 40, 50, and sometimes even 60 feet.

Streams.

The Ohio is the eastern boundary of this county, forming wide intervales along its banks. Indian Wheeling is a fine mill stream rising in Harrison county, and after crossing this, empties into the Ohio, opposite the town of Wheeling, which stands on the Virginia side.

Captina is another excellent mill stream, which after running about 17 or 18 miles in this county, puts into the Ohio 23 miles by water below Wheeling. These streams visit and fertilize a considerable part of Belmont.

From the view we have taken of this county, its geology, mineralogy, and botany, the reader will probably be prepared with us to conclude, that no part of the union, of equal extent, contains within it greater natural resources, or can support a more dense population.

The seat of justice is St. Clairsville, situated ten miles from the Ohio river, at Wheeling. It contains three houses for public worship, 15 stores, a printing-office, a bank, and 700 inhabitants.

Many of the inhabitants of this county are Quakers or Friends, who are charitable, humane, frugal, enterprising, industrious, and strongly opposed to slavery. From such a population, possessing such advantages, what may we not hope and expect from their exertions? Their fertile valleys will be turned into meadows, and their hills into pastures; the ox will fatten in the former, whilst the flocks of Andalusia will whiten the latter.

ART. IV. *Remarks on the Structure of the Calton Hill, near Edinburgh, Scotland; and on the Aqueous origin of Wacke; by* J. W. WEBSTER, M.D. of Boston.

THE country around Edinburgh is extremely interesting to the geologist, and presents numerous instances of the junction of rocks to which the advocates of the Neptunian system have referred in support of their opinion as to the aqueous origin of greenstone, basalt, and wacke; while the same examples have been cited by the Volcanists, and by those who hold an intermediate opinion. The structure of a portion of Calton

hill, where the most distinct alternations of substances (whose aqueous origin none can dispute,) with pure and well characterized wacke are displayed, has not, as yet, I believe been particularly described.

Edinburgh is situated nearly in the centre of an extensive coal formation, where the usual sandstones and other coal measures are connected with the newer rocks of transition. From the coal field rise in many places beds of greenstone, in general forming small conical and round-backed hills. Other eminences are composed of amygdaloid, claystone, and other porphyries; and basalt and trap tuff occur in an overlying position. Of these, it is not my intention to speak otherwise than as conveying a general idea of the geological relation of the wacke above referred to.

The structure of Calton hill has been exposed by the recent improvements, and in particular by a section made in the construction of the new road to London. The rock occurring in greatest abundance, and which is probably the fundamental bed, is a porphyry, the basis of which in general is claystone, which in many places passes into felspar, in others becomes a distinct greenstone. Numerous veins of calcareous spar traverse it in different directions, and I am lately informed, that very beautiful examples of veins of greenstone of contemporaneous formation with the rock itself, have been discovered *in* the greenstone. Upon the porphyry rests a bed of trap tuff, upon this other beds of the two rocks repose, that at the summit being porphyry. The back of the hill (as we pass from the city) is a spot of peculiar interest, consisting of alternate thin beds of bituminous shale, sandstone, wacke, and clay ironstone, disposed in a manner which will be best understood by a rough outline taken on the spot.

A Porphyry.
B Trap tuff.
C Porphyry.
D Trap tuff.
E Porphyry.
F Beds of wacke, &c. upper part concealed by vegetation.

1 Bituminous shale.
2 Wacke.
3 Sandstone.
4 Bituminous shale, with clay ironstone.
5 Wacke.
6 Bituminous shale.
7 Wacke, *with calc. spar.*
8 Bituminous shale.
9 Wacke.
10 Bituminous shale passing on both sides into
11 Wacke—and *calc. spar.*
12 Bituminous shale.
13 Wacke.
14 Bituminous shale.
15 Wacke.
16 Bituminous shale.
17 Sandstone,

The wacke has a greenish gray colour, which is pretty uniform. The fracture is nearly even and earthy, it is soft, yielding readily to the nail, and has a feebly shining streak. A slight stroke with the hammer causes the mass to separate in fragments of various size, the surfaces of which are often smooth and shining, each bed being composed of large distinct concretions, having a tendency to the prismatic form. This wacke fuses with difficulty before Brooke's blow-pipe. Specific gravity not determined, as it falls to pieces on being moistened.

The sandstone is for the most part gray, in some parts spotted red and brown, forming, as the section represents, the last stratum seen; the beds of sandstone are but a few inches in thickness, and the last (17) becomes less than an inch; it is probable, however, from the relative situation, from the dip and direction, that these strata are a continuation of others seen on the other side of the hill, where they are of sufficient thickness to have been quarried for the purposes of architecture. The *beds* of all rocks we know vary greatly in different parts, and it is not unusual for them to be some feet at one extremity, gradually decreasing till less than an inch in thickness at the other, or they may even be lost entirely, and gradually regain their former size; and it is not improbable that these beds of sandstone will be found to continue on towards the adjoining hills of Salisbury Craig and Arthur's Seat, passing under the greenstone and trap tuff.

The bituminous shale presents the usual characters; intermixed with it are numerous nodules of the common clay ironstone, the colour of which is a yellowish brown, these also frequently present characters common to the three substances, and throughout the beds, the passage from the one to the other is distinct. Whatever may be the opinions in regard to the origin of bituminous shale, there can be but one in regard to that of sandstone; and this has lately received no feeble support from the account given us by Dr. Paris, of a formation of this rock on the coast of Cornwall, where, says he, "we actually detect nature at work, and she does not refuse admit-

tance into her manufactory, nor conceal, with her accustomed reserve, the details of the operations in which she is engaged."

From the appearances which have been thus briefly noticed, no impartial geologist, we should imagine, would infer the *volcanic* origin of any portion of this formation; and if the aqueous origin of sandstone can be established, that of the wacke must be the same.

From its intimate connexion with the preceding subject, Dr. Webster subjoins the following:

Extract from a Paper on a recent formation of Sandstone, occurring in various parts of the Northern coast of Cornwall; by JOHN AYSTON PARIS, M.D. F.L.S., &c. &c. Published in the Transactions of the Geological Society of Cornwall, 1818.

"A VERY considerable portion of the northern coast of Cornwall, is covered with a calcareous sand, consisting of minute particles of comminuted shells. That part which lies between St. Ives and Padstow is more immediately the subject of the present inquiry; a tract which, with a few exceptions, is entirely covered with this species of sand; and which in some places, has accumulated in quantities so great as to have formed hills of from forty to sixty feet in elevation. A considerable area, for instance, in the parishes of Gwythian and Phillack has been thus desolated, and several churches have been inundated. In digging into these sand hills, or upon the occasional removal of some part of them by the winds, the remains of houses may be seen; and in some places, where the churchyards have been overwhelmed, a great number of human bones may be found. The sand is supposed to have been originally brought from the sea by hurricanes, probably at a remote period."——"The sand first appears in a slight but increasing state of aggregation on several parts of the shore in the bay of St. Ives; but on approaching the Gwythian river, it becomes more

extensive and indurated. On the shore opposite to Godrevy Island, an immense mass of it occurs, of more than a hundred feet in length, and from twelve to twenty feet in depth, containing entire shells and fragments of clay slate; it is singular that the whole mass assumes a striking appearance of stratification. In some places it appears that attempts have been made to separate it, probably for the purpose of building; for several old houses in Gwythian are built of it."——"It is around the promontory of New Kaye that the most extensive formation of sandstone takes place. Here it may be seen in different stages of induration; from a state in which it is too friable to be detached from the rock upon which it reposes, to a hardness so considerable, that it requires a very violent blow from a sledge to break it."——"But it is on the western side of the promontory of New Kaye, in Fistril Bay, that the geologist will be most struck with the formation; for here no other rock is in sight. The cliffs, which are high, and extend for several miles, are entirely composed of it."——"The beach is covered with disjointed fragments, which have been detached from the cliff above, many of which weigh two or three tons."

There are three modes by which Dr. Paris conceives the lapidification of calcareous sand may be effected. 1st. "By the percolation of water through a hill of calcareous sand, by which it becomes impregnated with carbonate of lime." 2d. "The percolation of water through strata containing pyritical substances, by which it becomes impregnated with sulphuric salts." 3d. "The percolation of water through decomposing slate, or any ferruginous strata, by which it becomes impregnated with iron alumina, and other mineral matter."

Art. V. *Localities of Minerals.*

To the Editor of the American Journal of Science, &c.

New-York, Dec. 21, 1818.

Dear Sir,

It is desirable that some mode should be adopted by which the public may become acquainted with all the *New American Localities of Minerals*, as they are discovered from time to time. With deference I would suggest, that in each number of your Scientific Journal, new localities might be recorded in alphabetical order, for present information and future reference.

The following localities, which have come under my observation, and which are probably not noticed in any work, are at your service.

1. *Agate.* Rolled mass: occurred near Powles Hook, New-Jersey.
2. *Apatite.* Truncated crystals of one inch, and amorphous; occurs in granite, chiefly in the felspar. Corlaer's Hook, vicinity of New-York.
3. *Brown Mammillary Hematite*, covering quartz crystals. Perkiomen lead-mine. Montgomery county, Pennsylvania
4. *Carbonate of Magnesia.* Structure earthy. Apparently a *pure* carbonate of magnesia. In mica slate, and granite; chiefly in the quartz. Roxborough, Philadelphia county.
5. *Common Jasper.* Traversed by veins of semi-opal. Small detached masses, frequently waterworn. Rhinebeck, Dutchess county, New-York.
6. *Compact Malachite.* Perkiomen lead-mine.

7. *Fetid Carbonate of Lime.* In ridges ; and strata nearly vertical, sometimes containing petrifactions. Very frequent in Dutchess county, particularly in the neighbourhood of Rhinebeck Flats, and near Hyde Park.
8. *Fibrous Talc.* In granite. Roxborough.
9. *Graphic Granite.* North River, near the city of New-York.
10. *Graphite.* In a calcareo-siliceous gangue. Corlear's Hook.
11. *Native pulverulent* (or rather granular) *Sulphur.* In pyritical quartz. Barren Hill, Montgomery county, Pennsylvania.
12. *Plumose Asbestus.* Corlaer's Hook.
13. *Semi-opal.* In common Jasper—(which see.)
14. *Scaly Talc.* In granite. Roxborough.
15. *Stellated Quartz.* Perkiomen lead-mine.
16. *Sulphate of Barytes.* In sulphuret of lead and silver. Livingston's lead-mine, Columbia county, New-York.
17. *Sulphuret of Silver.* With sulphuret of lead. Same locality.
18. *Tourmalin.* In masses of crystalline quartz. Rhinebeck.

Very respectfully,

F. C. SCHAEFFER.

The following notices were prepared before the receipt of the above letter.

Other Localities of Minerals and of ANIMAL REMAINS, *and acknowledgments of Specimens received.*

Guadaloupe.—Native sulphur, obsidian, pitchstone, native alum, basaltic hornblende, alum covered with sulphur.

Porto Rico.—Hexagonal crystals of mica.

Specimens of the above minerals are in the cabinet of Mr. John P. Brace, at Litchfield, Connecticut.

Molybdena is found in Shutesbury, Massachusetts, near Northampton, east of Connecticut River, on the land of William Eaton. It is the common sulphuret, but remarkably beautiful and well characterized. Its colour is nearly that of bright lead, very brilliant, smooth, and almost unctuous; soft, flexible, distinctly foliated, and the folia are very thin, and easily separable, almost like mica. It gives the usual greenish trace on white pottery, while a line drawn parallel on the same basis, by a piece of plumbago or black-lead, is black; this being (as pointed out by Brongniart) the easiest criterion, by which to distinguish between molybdena and plumbago, or black-lead. We have many times applied it with entire success.

This molybdena, from Shutesbury, is chiefly crystallized, and the crystals are, in some instances, very distinct; their form is that of a flat six-sided prism, or what is commonly called a table. The rock, from which they were obtained, is a granitic aggregate, (judging from the specimen sent, it may be a true granite) and the forms of the crystals are very distinctly impressed in the stone, so that when removed they leave an exact copy or crystal mould. In a letter from the proprietor of the land, it is said that the molybdena is found in a ledge of rocks, six or seven feet above the surface of the earth, and about ten or twelve feet above the level of the water; the direction of the rocks is from S. to N. E. by N.; the metal is in a vein, running E., and was discovered in small pieces in the top of the ledge. After putting in two blasts, some large pieces were obtained.

From this account, and from the specimens, (some of the crystals being an inch or more in length) this must be one of the most interesting localities of molybdena hitherto observed in this country; and it is hoped Mr. Eaton will take some pains to procure and furnish specimens.

Rose Quartz.—From Southbury, Connecticut, not far from Woodbury, and from the Honsatonuck River, two young men, of the name of Stiles, have brought us specimens of *rose quartz*, of delicate and beautiful colour. It is said to be abundant in a ledge of the same substance.

Plumbago.—In Cornwall, Litchfield county, Connecticut, plumbago is found, of a good quality, and in considerable masses, in a vein contained in a rock of gneiss, or mica-slate. It has been known a good while, and is said to have been exported anterior to the American revolutionary war.

Coal, &c. in Zanesville, Ohio. Through the kindness of the Rev. Dr. Bronson, Principal of the Cheshire Academy, we have received the following information.—In cutting a canal in the above town, in the spring of 1817, through freestone, trees, and fish, and other substances, both animal and vegetable, were taken out, alike petrified to a freestone, excepting the bark of a beach tree, which was very perfect and beautiful coal—(as we have had an opportunity of ascertaining, from an examination of the specimens.)

Coal, in the county of Muskingum, Ohio. Common stone-coal, highly bituminous, (the slaty or black coal of Werner,) is found abundantly.

South of Lake Erie, about 25 miles, in the bed of Rocky River, are found shells, and other animal remains, imbedded in argillaceous iron; the specimens were collected in 1817, by the Rev. R. Searle.

Mammoth's Tooth, from the River St. Francis, west of Mississippi. Return J. Meigs, Esq. has transmitted, through the Rev. E. Cornelius, a mammoth's tooth, apparently not mineralized. It appears to have belonged to a very old animal, as the processes, (which, it is well known, are commonly very prominent) are worn down smooth, and some of them almost obliterated.

Blue Ridge, Tennessee, and Mississippi Territory.—Through the kindness of the Rev. E. Cornelius, and of Mr. John H. Kain, we have received a considerable collection of specimens, illustrative of the mineralogy and geology, and Indian antiquities of these regions; they may be, on a future occasion, the subject of more particular remarks.

Coal, in Suffield, Connecticut, on the river of the same name. From Mr. Nathan Stedman, we have received specimens of coal, found in thin veins, in rocks of slate, and argillaceous sandstone, on the banks of the river. The veins are

thin, but considerably numerous ; the coal is very glossy and black ; breaks with a smooth and almost conchoidal fracture, and very much resembles jet. It is very much intersected by thin veins—(not thicker than a knife-blade)—of white crystallized calcareous spar. This coal is bituminous, and burns pretty freely. It has not been explored, except superficially.

Coal, in Southington, Connecticut. Beds of slate are found more or less bituminous ; and, at the bottom of some of the wells, the slate begins to exhibit thin veins of coal, distributed in great numbers through the substance of the slate, which is the shale of the miners. The coal is from the thickness of a knife-blade to that of a finger; it is highly bituminous, and burns with great freedom. Even the entire masses of the stone burn brilliantly, when ignited on a common fire ; and, after exhaustion of the coally matter, leave the slate of a grayish colour.

The locality from which the specimens were taken, is on the land of Roswell Moore, Esq. about midway between Hartford and New-Haven. The spot was lately examined by Col. Gibbs, Eli Whitney, Esq. Professor Olmstead, and others ; and arrangements are making to bore the strata, to the depth of several hundred feet, if necessary. These localities are in what may, with propriety, be called the coal formation of Connecticut. Coal has been found in several other places in that state ; and the peculiar geological features of the region in which it is contained, are very interesting, and may hereafter be described in form.

Sulphat of Barytes, with Coal, &c.—Sulphat of barytes exists abundantly in Southington, on what is called the Clark Farm. With quartz, carbonate of lime, &c. it forms the gangue of a metallic vein, containing galena, or sulphuret of lead, copper pyrites, &c. The sulphat of barytes is more or less crystallized, and principally in the form that is called the coxcomb spar. The same vein, although it is in the side of a mountain, several hundred feet above the flat country adjacent, and two or three miles from the coal strata above mentioned, contains numerous spots and patches of coal, very much resembling that at Suffield. It is of a most brilliant black, and

contrasted with the white, stony matrix, (principally quartz and sulphat of barytes) in which it is enveloped, it forms elegant specimens.

Scintillating Limestone.—In Vermont, a singular scintillating limestone is found, of which an account is given in the following extract of a letter from Mr. George Chase, dated Randolph, February 19, 1818.

"The object of the present letter is to acquaint you with a circumstance relating to the limestone that abounds in this primitive country, which to me is inexplicable. This carbonate of lime is of a pale sky-blue colour; effervesces strongly with nitric acid; and, by burning, produces lime, so that there is no question as to the identity of the mineral. But it likewise gives forth sparks with steel:—this I concluded, at first, to be an accidental circumstance; but every specimen that I have tried, from various quarters of the country, uniformly gives fire with steel. The limestone is found in layers, in blocks, and masses, disseminated among the clay-slate that covers the greatest part of the townships in this vicinity. When first taken from the earth, and exposed to the air, it is covered with an incrustation of a dark reddish-brown colour, that crumbles easily between the fingers, and is generally from one inch to a foot in thickness. This incrustation, however, hardens on a long exposure to the air. This led me to think that the incrustation was owing to the decomposition of the limestone, which was produced by the sulphuret of iron, intimately disseminated through the rock, which would also explain the singular circumstance of its striking fire. But on dissolving a small quantity of the mineral in nitric acid, and adding a drop or two of the decoction of gallnut, no discolouring of the liquor was produced."

Limpid Quartz.—West Canada Creek, a northern branch of the Mohawk, affords, in its sands, small crystals of quartz, limpid, and terminated at both ends by pyramids of six sides; we are indebted for specimens to Professor Fisher.

Fetid Primitive Limestone, &c.—From the vicinity of Williamstown College, through the kindness of Professor Dewey, we

have received specimens illustrative of the geology of that region. Among them is limestone from Stockbridge, crystallized in large plates and rhomboids, almost white, and still fetid on being rubbed, which is very different from most fetid limestones, which are dark coloured, and even black, and do not belong to primitive formation.

Molybdena.—In Pettipaug, Saybrook, Connecticut, molybdena occurs. It is mentioned in the Review of Cleaveland's Mineralogy, and is here cited again for the purpose of pointing out its locality more exactly. It is found about half a mile to the E. of the Turnpike leading from Saybrook to Middletown, on the first road on the right hand above the turnpike gate, near the house of the widow Pratt. It is not far from Pettipaug meeting-house, in a northern direction.

Beryl.—In Haddam, Connecticut, are found many beryls, and some of uncommon size; an account of one of the most remarkable localities is contained in the following memorandum from the Rev. Mr. Mather, to whom we are indebted for specimens.

"The place in which the beryls are found is in the town of Chatham, about one mile and a half north from Middle-Haddam landing; about half of a mile S. W. of a large hill, on which is the cobalt mine. The rock in which the beryls are contained is granite; the parts of which are very large, especially the felspar and the mica. Large masses of shorl are also found in these rocks. Beryls have also been found in other parts of Middle-Haddam, amongst rocks of the same description. The *greatest* diameter of the largest beryl is four inches; the *least* three inches. The beryls are numerous, and of different sizes; though few are less than an inch, or two inches in diameter. The length of the longest beryl is five inches."

Clay.—Near Delhi, New-York, a few rods from the Delaware river, are found beds of clay, of which specimens have been transmitted by Mr. John P. Foote, of New-York. We are of opinion that they are not porcelain clay.

Gypsum—Cayuga Lake. We are informed by Dr. L. Foot, that the workmen who have excavated about 20 feet on the border of the lake, in gypsum, which is generally of a dark brown, or black colour, when they come to a transparent crystallized piece, call it isinglass, and reject it as worthless: the hint should be remembered by mineralogists, that the specimens may be saved for their cabinets.

ASBESTOS IN ANTHRACITE.

Extract of a letter from Dr. I. W. Webster.

Boston, 27th Nov. 1818.

Dear Sir,

In examining some masses of the anthracite from Rhode Island, one piece attracted my attention, from the waved structure of the lamellæ into which it separated. The fragments of this were wedge-shaped, and I found the space between some of the laminæ filled up by a fibrous, silky substance, which induced me to break up other masses, in one of which I discovered an abundance of amianthus; the filaments are of a light-green colour in some parts of the mass—in others presenting different shades of brown. With a microscope, I found the fibres intermixed with the anthracite; or forming thin layers, and these sometimes parallel to, at others crossing, in different directions, the course of the laminæ. How far the presence of this mineral may influence the ignition or combustion of the coal, is a question, perhaps, worth determining. Should my engagements permit, I shall make further examination, and inform you. In the mean time, the notice of this fact may call the attention of some of your readers to the subject. At any rate, this substance has, I believe, never before been noticed in connexion with anthracite, and is highly interesting in a geological point of view.

REMARKS.

We have been familiar with the Rhode Island anthracite, and with the formation of rocks in which it is found; and, long since, observed the fact mentioned by Dr. Webster. The asbestos often is in the form of the most delicate amianthus, frequently blended also with the slate rocks, which form the roof and pavement of this coal. A specimen now lies before us, in which a complete vein of this amianthus, with fibres nearly two inches *in length, connects and pervades* a mass of slate, supposed to be of the transition class.

Similar facts are mentioned also by Dr. Meade, in his account of the Rhode Island coal.

RED PYROXENE AUGITE.

Extract of a letter to the Editor, from Dr. H. H. Hayden of Baltimore.

I have very lately discovered a couple of small specimens of the transparent *red* pyroxene, resembling fine crystals of titanium, which I, at first, mistook it for. One of them is contained in the middle of a large crystal, like the rubellite in the green tourmalins of Massachusetts, but it is not the same substance. The pyroxene, which I have reference to, is the olive-coloured epidote of some, pistazite of others, but resembles, in this instance, the sahlite; the crystals being divisible longitudinally. Some of them are five inches long, and half an inch diameter, hexaedral and double; that is, two joined together, as described by Brochant in particular.

Some other localities, of which we have received notices, may be mentioned in a future number.

BOTANY.

Art. VI. *A List of Plants found in the neighbourhood of Connasarga River, (Cherokee Country) where Springplace is situated; made by* Mrs. Gambold, *at the request of the Rev. Elias Cornelius.**

A.

Acer rubrum and Sacharium
Acanitum uncinatum
Actæa racemosa
Adianthum Capillus Veneris
Aesculus Pavia
Agave
Agrimonium Eupatorium
Aira pallens
Aletris farinosa
Alisma Plantago
Allium, 2 sp.
Amasonia latifolia
Anchusa
Andromeda arborea and other sp.
Andropogon alopecuides and ambiguum
Anemone hepatica, Thalictroides, virginiana, and pennsylvania
Angelica lucida and other sp.
Annona
Antirrhinum elatine
Apocynum cannabinum
Aquilegia canadensis
Arabis
Aralia spinosa
Arctatis caroliniana
Arethusa parviflora
Aristoloichia serpentaria, 3 sp.
Arum sagittæfolium and triphyllum
Arundo tecto
Asarum virginicum
Aselepias purpurascens, variegata, verticillata and others, tuberosa
Ascyrum
Asplenium
Aster concolor, linarifolius, and many others
Avena palustrio and spicata
Azalea viscosa, and others.

B.

Berberis canadensis
Betula alnus
Bidens pusilla N. S. Muhlenb.
Bignonia crucigera and radicans
Bucknera americana.

C.

Cacalia
Calycanthus floridus
Campanula perfoliata and divaricata
Clematis ochraleuca and virginiana
Clitoria mariana and virginiana
Collinsonia virginica

* Copied partly from Manuscripts of the late Dr. Muhlenberg, of Lancaster, Pennsylvania.

Cardumine virginica
Carduus, several sp.
Carex, N. S.
Cassia chamæcrista, marilandica, nictitans, and Tora
Ceanothus americanus
Cephalanthus occidentalis
Cerastium arvense
Cercis canadensis
Chelone glabra and Penstemon
Chenopodium ambrosioides and anthelminticum
Chionanthus virginicus
Chironia campanulata and other sp.
Chrysogonum virginicum
Cimicifuga pulmata
Circea lutetiana
Cissampelos smilacina
Claytonia virginica
Commelina erecta, longifolia virginica
Convallaria multiflora and racemosa
Conyza linifolia
Coreopsis auriculata, bidens, senifolia tripteris, alternifolia and verticillata
Cornus florida
Corylus americana
Crætægus apiifolia
Crotallaria sagittalis
Cucubalus behen
Cuscuta americana
Cynanchum
Cynoglossum officinale and virginicum
Cynosurus indicus and sparsus
——— filiformis (Muhlenb.)
Cypripedium acaule, alba and calceolus.

D.

Delphinium exaltatum
Dentaria multifida
Diodia N. S. and virginica
Dioscorea
Diospyros virginiana
Dodecatheon media
Dracocephalon virginianum

E.

Echium vulgare
Elephantopus caroliniensis
Eleusine filiformis
Epilobium coloratum
Erigeron pulchellum, and other sp.
Eryngium aquaticum ovalifolium and yuccæfolium
Erythronium dens canis
Eupatorium cœlestinum, perfoliatum, and urticæfolium
Euphorbia colorata, ipecacuanha, and other sp.
Evonymus virginicus

F.

Fagus castanea dentata sylvatica atropunicea
Festuca nutans, palustris and sylvatica
Fragaria vesca
Fumaria N. S.

G.

Galactia mollis
Galax aphylla
Galega hispidula and virginica
Galium, several sp.
Gerardia asgelia, hydrophylla, lancifolia and purpurea
Geum rivale
Gleditsia spinosa

Gaura sp.
Gentiana saponaria, and others
Geranium, 2 sp.

Glycine apios and tomentosa parabolica (Muhlenb.)
Gnaphalium germanicum, and others.

H.

Hedyotis sp.
Hedysarum prostratum and others
Helianthus angustifolius. sp. nova.
Heuchera
Hibiscus
Houstonia cœrulea, purpurea, and varians

Hydrangea glauca
Hypericum fasciculatum, nudiflorum, prolificum, and others
Hypnum sp.
Hypoxis erecta.

I.

Ilex aquifolium sp.
Impatiens noli tangere
Inula graminifolia and mariana

Ipomœa, sky blue, and other sp.
Inis, low, sweet-smelling blossoms in spring, and other sp.

J.

Jutropha stimulosa
Juglans alba acuminata
——— —— ovata

Juglans nigra
——— oblonga alba
Juncus bicornis and tenuis

K.

Kalmia latifolia

Kyllingia triceps

L.

Laurus benzoin and sassafras
Lechea minor
Lepidium sp.
Liatris graminifolia, spicata and squarrosa
Lilium martagon
Limodorum tuberosum
Linum virginicum
Liquid amber styraciflua
Liriodendrum tulipifera
Lobelia cardinalis, inflata, kalmii, puberula and siphylitica

Lonicera erecta and symphoricarpos
Ludugia alternifolia jussiæoides
Lupinus sp.
Lycopodium apodum and rupestre
Lycopsis
Lycopus virginicus
Lysimachia quadrifolia and punctata
——— sp.
Lythrum lineare and strictum.

M.

Malaxis unifolia
Marchantia polymorpha

Mimosa horridula
Mimulus ringens

Melanthium latum
——— sp.
Melica speciosa
Melissa nepeta
Menispermum carolinianum
Mespilus several sp.
Mitchella repens
Momordica sp.
Monarda punctata
Monotropa several sp.
Morus.

O.

Oenothera biennis, lineanis, and others
Ophioriza mitreola
Ophrys cernua
——— sp.
Orchis ciliaris unifolia
Orobanche uniflora
Oxalis, 2 sp.

P.

Panax ginseng
Panctratium carolinianum
Pancium nitidum
Parietaria pennsylvanica
Parnassia caroliniana
Parthenium integrifolium
Passiflora incarnata and lutea
Paspalium ciliatifolium
Pedicularis canadensis
Penstemon lævis
Penthorum sedoides
Phlox ovata, paniculata and pilosa
Phryma liptostachia
Physalis pubescens, several sp.
Phytolacca decandra
Pinus, several sp.
Plantago major and virginica
Poa nervata
Podophyllum peltatum
Polygala cruciata, incarnata and lutea
Polygonum hydropiper, and other sp.
Potentilla reptans
Prenenthes trifida
Prunella vulgaris
Prunus cerasus virginiana, and others
Psoralea melilotoides
Pyrola, 2 sp.
Pyrus malus coronarius

Q.

Quercus alba, 2 sp.
——— nigra, various sp.
——— rubra
Quercus prinus
——— Phellos
Queria canadensis

R.

Ranunculus bulbosus, and other sp.
Rhexia mariana
Rhus toxicodendron, and others
Ribes sp.
Rosa, several sp.
Rubus fruticosus, hispidus and occidentalis
Rudbeckia fulgida, hirta and purpurea
Ruellia

S.

Sagittaria sagittifolia
Salix tristis and others
Sisyrinchium Bermudiana
Sisymbrium nasturtium

Salvia lyrata and urticæfolia
Sambucus nigra
Sanicula marilandica
Sanguinaria canadensis
Saururus cernuus
Scabiosa sp.
Schisandra
Schoenus sparsus
Scirpus retrofractus
Sentellaria hyssoppifolia, parviflora, and others
Sedum, a low plant, fl. white
Senecio sp.
Serratula prælta, scariosa and spicata
Sida rhombifolia and spinosa
Silene antirrhina, and another sp.
Sium sp.
Smilax sarsaparilla and other sp.
Smyrnium aureum
Solanum nigrum
Solidago nova boracensis, rigida, virga aurea and others.
Sonchus sp.
Sophora fl. purple
Spigelia marilandica
Spiræa aruncus, apulifolia, stipulaica, tomentosa and trifoliata
Staphylæa trifoliata
Stellaria sp.
Styrax sp.
Sylphium N. S.
——— compositum.

T.

Tabernamontana latifolia
Teverium canadense
Thalictrum, various sp.
Thlaspi bursæ pastoris
Thymus virginicus
Tradescantia virginica
Tragopogon dandelion
Trichodium laxiflorum and procumbens
Trichostema dichotoma
Trifolium (Buffalo)
Trillium cernuum, luteum, sessile, and another sp.
Triosteum angustifolium

U.

Ulmus, 2 sp.
Uniola latifolia
Uvularia sessilifolia.

V.

Vaccinium, several sp.
Verbascum lychnitis
Verbena officinalis
Verbesina sp.
Veronica virginica
Viburnum, several sp.
Viola, several sp.
Viscum
Vitis, several sp.

X.

Xanthium strumarium
Xantoxylon tricarpon

Y.

Yacca filamentosa.

Acer rubrum.—The inner bark boiled to a sirup, made into pills, and these dissolved in water, is used in cases of sore eyes; the eyes washed therewith.

Actæa racemosa.—The root in spirits, these made use of in rheumatic pains.

Adianthum Capillus Verenis.—A decoction of the whole plant, used as an emetic in cases of ague and fever. A very strong medicine.

Aesculus Pavia.—The nuts pounded, are used in poultices.

Agave—The root is chewed in obstinate cases of diarrhœa with wonderful success. It is, however, a very strong medicine.

Allium.—The Indians are fond of, for culinary purposes.

Angelica,—The same.

Annona.—Of the bark they make very strong ropes.

Aralia spinosa.—A decoction of the roots roasted and pounded, (green, they are poisonous) is given as an emetic. A very strong one.

Asarum virginicum.—The leaves dried and pounded, are used for snuff; fresh, they are applied to wounds.

Bignonia crucigera.—Tea made of the leaves cleanses the blood.

Calycanthus floridus.—The *roots* are used as (though very strong) emetics. The *seeds* to poison wolves.

Carduus—various species. The roots used in poultices.

Cercis canadensis.—Children are fond of eating the blossom.

Coreopsis auriculata.—The whole plant is much used in colouring. It affords a *red* colour.

Cornus florida.—The bark of the root is used to heal wounds, and in poultices.

Ilex.—Of the wood, spoons are made. The berries of service in colics.

Juglans oblonga alba.—A kind of pills are prepared from the inner bark, and used as a cathartic.

Liquidamber styraciflua. The gum is used for a drawing plaster. Of the *inner* bark a tea is made for nervous patients.

Liriodendrum tulipifera.—Of the bark of the root a tea is made and given in fevers. It is also used in poultices.

Melanthium.—The root is a crow poison; and a sure, but severe cure for the itch.

Pinus.—Boil the root, skim off the turpentine, spread it on Deer's skin (tanned,) for a drawing plaster.

Podophyllum peltatum.—A sirup is boiled of the root, and given for a purgative, two pills at a time. A drop of the juice of the fresh root in the ear, is a cure for *deafness.* (So I have been told, I never witnessed it.)

Potentilla reptans.—A tea of it is given in fevers.

Prunus cerasus virginiana.—Of the bark a tea is made, and drunk in fevers.

Quercus alba.—The bark is used for an emetic.

Quercus nigra and *rubra.*—A die for leather.

Rosa.—The roots boiled, and drunk in cases of dysentery.

Rubus fruticosus.—The root good to chew in coughs.

Sanguinaria canadensis.—The root is used for the *red* die in basket making.

Saururus cernuus.—The roots roasted and mashed, used for poultices.

Solanum nigrum.—When young, made use of as the best relished potherb.

Solidaga virga aurea.—A tea much made use of in fevers.

Sophora.—A blue die.

Spigelia marilandica.—In cases of worms.

Spiræa stipulaica and *trifoliata.*—The whole plant a very good emetic. Of a strong tea or decoction thereof, a pint is drunk at a time.

Tradescantia virginica.—The leaves much relished greens for the table.

Yucca filamentosa.—The roots pounded and boiled, are used instead of soap to wash blankets; likewise to intoxicate fishes, by strewing them pounded on the water The same is done with *Æsculus.*

ART. VII. *Description of a new species of Asclepias.* By *Dr.* ELI IVES, *Professor, &c. in the Medical Institution of Yale College.* (*With a Plate.*)

THE plant, which is the subject of the following observations, is found growing abundantly on the sandy plains east of Cedar Hill, in New-Haven. It is locally associated with the asclepias viridiflora and verticillata. When this species of asclepias was first noticed by me, it was supposed to be a variety of viridiflora of Rafinesque and Pursh; but after examining a great number of specimens, it was found that the varieties did not blend themselves. The leaves of the viridiflora being uniformly oblong and obtuse, the leaves of the other uniformly lanceolate and acute. To this new species I purpose to give the name Lanceolata.

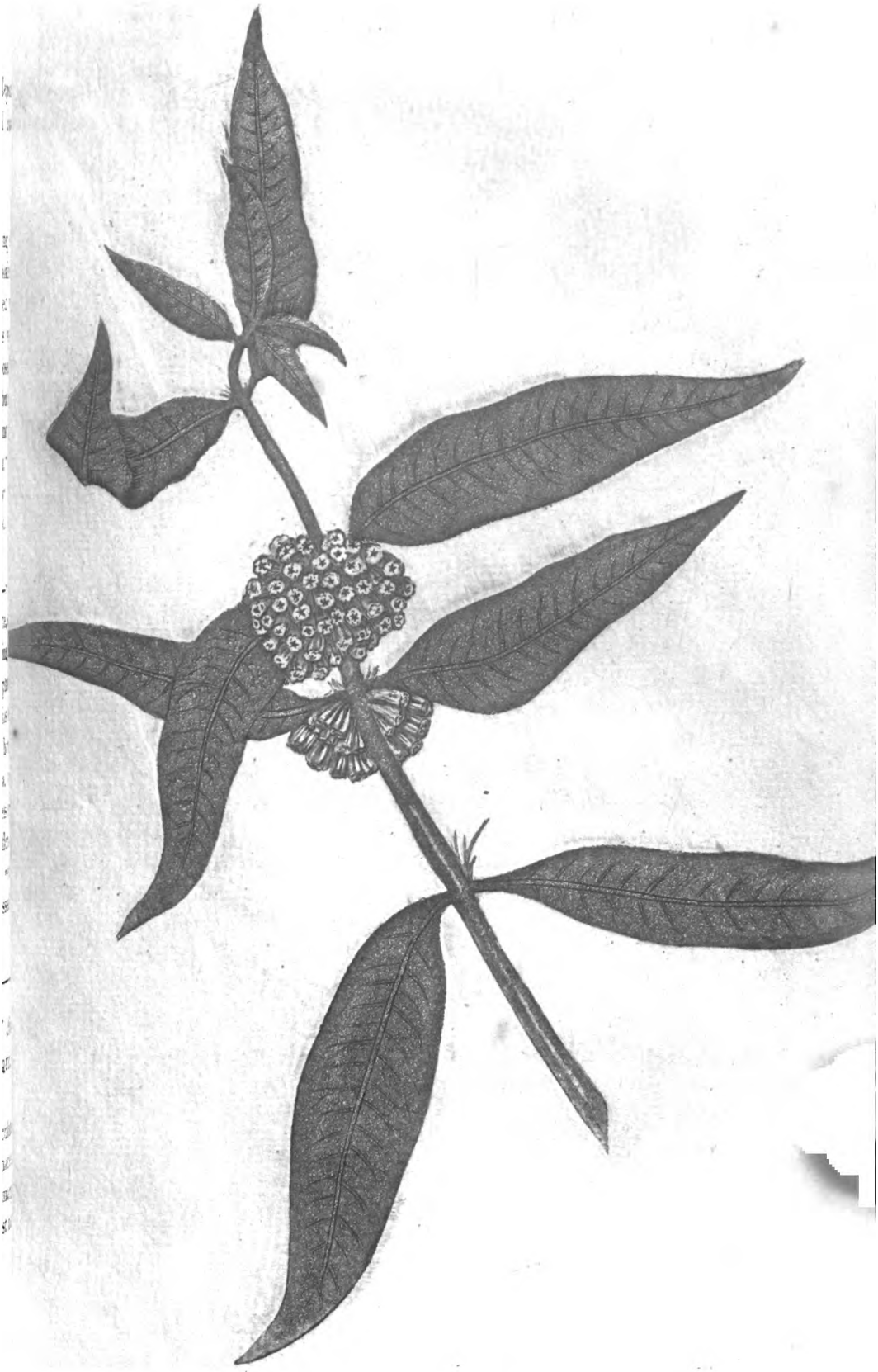
Specific character of the asclepias lanceolata:—Stem decumbent, hirsute; leaves opposite, lanceolate, acute, sub sessile, hirsuit umbels lateral, solitary, sessile, nodding, subglobose, dense-flowered; appendage none. See the plate.

The asclepias lanceolata is allied to the asclepias longifolia and viridiflora by the absence of appendage or horn of the nectary. It is distinguished from the longifolia, which is characterized by alternate linear leaves, and umbels erect.

The asclepias lanceolata and asclepias viridiflora belong to Mr. Elliott's genus acerates. In both, the nectary or stamineous crown is short concave, and oppressed to the angles of the filaments.

ART. VIII. *Description of a New Genus of American Grass.* DIPLOCEA BARBATA, by C. S. RAFINESQUE, Esq.

DIPLOCEA. *Generic definition.* Flowers paniculated monoical or polygamous. Exterior glumes membranaceous bivalve one to three flowered, valves subequal emarginated mutic. Anterior glumes bivalve unequal, the largest notched,



Asclepias lanceolata.

notch aristated, the smallest mutic entire bearded. *Additional characters.* Flowers when single sepile with a lateral jutting peduncle, when double, one sepile and one pedunculated, when three two are pedunculated and alternate. The hermaphrodite and male flowers are similar: the female are nearly clandestine, inferior. Stamens 3, styles 2. Seeds ovate oblong.

Observations. This genus is intermediate between *amphicarpon*, Raf. (*Milium amphicarpon, Pursh*) and *aira*, L. It differs from this last by its polygamy, variable number of flowers, notched valves, &c. The generic name means *double notch.* Its type is the following species, which had been ranged with the *aira*, by Walter, and considered doubtful by Pursh.

Diplocea Barbata.

Specific definition. Stems cespitose, articulations bearded; leaves rough glaucous, neck ciliated; panicles, few flowered, female axillary; largest valvet rinervate, and ciliated as well as the awl.

Latin definition. Caulibus cespetosis, geniculis barbatis, collo ciliato, foliis scabris glaucis, paniculis paucifloris, femineis axillaribus; valva majore trinerva, aristaque ciliata.

Description. Roots, annual fibrous: stems many, unequal, rather procumbent at the base, next assurgent, rising one foot at utmost; they are geniculated, slender, brittle, weak, and smooth. The knees or joints are bearded, the sheaths are split, the neck ciliated, the leaves short, stiff, rough glaucous, linear acute, obscurely striated. The panicles have few flowers, particularly the female ones, which are axillary coarctated almost hidden, while the male are terminal and divaricate: some hermaphrodite flowers are occasionally, but seldom found among both panicles; they are all similar, differing only in the want of stamina or pistils. The valves of the exterior glumes are nearly equal oblong notched obtuse, mutic and oneneroed. The valvules or valves of the glumule (corolla or interior glume) are unequal, the largest is ciliated

trinerve bifid, with a soft ciliated awl in the notch, as long as the valve: the small valve is ovate acute concave, very hairy on the back. The colour of the flower is reddish or pale red; but variable.

Observations. This plant is probably the *aira purpurea* of Walter, Pursh, Elliott, &c. but does not belong to that genus. It was found in Carolina, but I have found it on Long-Island, near Gravesend, Bath, Oyster-Bay, &c. on the sandy and gravelly sea-shore: it grows probably in the intermediate states. It blossoms in August and September, has no particular beauty, but a very singular appearance. The specific name of *purpurea* was improper, since the colour of the flowers is variable from whitish to red.

Art. IX. *Floral Calendar, &c.*

To the Editor of the American Journal of Science, &c.

Plainfield, October 17, 1818.

Sir,

Should the following calendar be thought worthy of a place in your Journal, you will please to insert it. Though very brief, it will show that vegetation is considerably later on the range of mountains, on which this place is situated, than in the level parts of our country.

Yours truly,

J. PORTER.

Floral Calendar for Plainfield, Massachusetts, 1818. By Jacob Porter.

March 13. Robins and bluebirds appear.

April 25. Claytonia in flower. A considerable part of the ground is covered with snow, which, in many places, is 2 or 3 feet deep.

April 27. Observed the claytonia, blue violet, strawberry, and a species of sedge, in blossom, at Worthington.

May 1. Hepatica, roundleaved violet, and erythronium in flower.

May 10. Chrysosplenium, or golden saxifrage, in flower.

May 15. The large trillium, or purple wakerobin, in flower.

May 18. Uvularia, or cellwort, and white violet, in flower.

May 19. A fall of snow, so that the ground at night was almost covered with it.

May 22. The beautiful coptis, or goldthread, in flower.

May 25. Ash and beech in flower.

May 26. Sugar-maple, viburnum, threeleaved arum, blue violet, small panax, prostrate mitella, fly honeysuckle, white berried gaultheria, and umbelled Solomon's seal, in flower.

June 17. Absent, since my last date, on a tour to New-York. Four other specimens of Solomon's seal, trientalis, azalea, 2 species of crowfoot, blue-eyed grass, medeola, moose-bush, and several species of vaccinium, in flower. The small trillium, or smiling wakerobin, sarsaparilla, and dentaria, blossomed during my absence.

June 22. Small enothera, 2 species of veronica, and the golden senecio, in flower.

June 23. Mountain ash, Norway potentilla, sanicle, and the lovely linnea in flower.

June 28. Prunella, and red and white clover, in flower.

June 29. Mitchella, in flower.

June 30. Yellow diervilla, in flower.

July 1. Climbing corydalis, in flower.

July 4. The fimbriate archis, and roundleaved pyrola, in flower.

July 5. Spiked epilobium, and roundleaved mallows, in flower.

July 6. Mullen, in flower.

July 7. Small geranium in flower.

July 8. Another species of epilobium, in flower.

August 18. Frost this morning.

ZOOLOGY.

Art. X. *Notes on Herpetology, by* Thomas Say, *of Philadelphia.*

(Communicated by the Author.)

ALTHOUGH I have not devoted a particular study to this department of the science of nature, yet I have been amused and instructed by casually observing many of the subjects of it, when I have been rambling in their native haunts, pursuing objects more particularly interesting to me.

But when perusing, the other day, the account of the copper-head of our country, by Mr. Rafinesque, I was impelled to ask for information on the subject, through your useful publication, in which that account appeared, and to make, at the same time, a few miscellaneous remarks or notes. These are in part included in the present essay, and if they should have a tendency to incite attention to the reptilia of the United States, at present in a state of confusion and incertitude, some portion of benefit will be rendered to the great cause of science.

I think that a moderate degree of labour and observation bestowed upon the investigation of the species already described, would prove the unity in nature of some species which have been considered as distinct by all the authors, would detect many errors in observation, expose some deceptions practised on credulity by the designing, and would enable us to fix, with some degree of accuracy, our knowledge of truth and of the species.

A work devoted particularly to this class, by some one adequate to the task, who could have in his view all the known species, is indeed a desideratum.

Scytale cupreus, Copper-head, &c. of Mr. Rafinesque. I have always considered the Copper-head to be no other than the *Cenchris mockeson* of authors, and *Boa contortrix of Linn.*

v. Latr. Lacep. Shaw, Daudin, &c. *Agkistrodon mokasen* of Beauvois; which opinion is not a little corroborated by an actual comparison of one of these animals in Peale's Museum, with the descriptions of the authors above mentioned. It may be objected to me, that the *mockeson* of those naturalists is a *Cenchris*, and not a *Scytale*, therefore generically distinguished from the Copper-head; but on the other hand, we know that the genus *Cenchris* does not exist in nature, that the individual upon which it was founded, was either a fortuitous variety, or that the illustrious naturalist was deceived by the desiccation of his specimen, giving to the basal caudal plates a bifid aspect. That the former was the case I analogically infer, from having seen, in the collection of the Academy of Natural Sciences, a *Coluber heterodon*, of which the fifth and sixth pairs of caudal scales were entire, and not as usual bifid. An additional corroboration of the truth of this inference is derived from the circumstance of the *Scytale* of Peale's Museum, having the ten or eleven apicial caudal plates bifid, precisely as in the genus *Acanthophis*, to which it seems closely affianced, and to which it would be referred if this character was a permanent one. In every other character this specimen coincides with the *S. mockeson* of authors, and in every necessary respect with the *S. cupreus* of Mr. R. with the sole exception of the calcarate termination of the tail. This caudal horn seems to approximate Mr. R's. animal to the *S. piscivorus* or true horn-snake, about which the credulous have so absurdly alarmed themselves, and which was arranged with the *Crotali* by Lacepede, in consequence of having a horn on the tail an inch long. We find sometimes a small indurated tip to the tail of *Coluber melanoleucus*,* at least upon some full grown specimens, formed by the elongation and appression of the terminal scales; a larger one on that of the European viper, and of the *Acanthophis cerastes*, and *Brownii*. Mr. Peale's specimen certainly has not the horn, but it has at the

* This large species I understand has been mistaken by a writer on Natural History for *Boa constrictor*: this is mentioned to show how remotely it is possible to diverge from accuracy in this science.

termination of the tail a scale somewhat longer and more indurated than the others, the individual had not attained his full growth. If then this species (and some others) is subject to vary in the form of its caudal plates, from which the generic characters are in part estimated, may it not also vary in the armature of the tail, which at most can only be considered as specific. The Copper-belly is a very distinct species. If the *S. cupreus* is, notwithstanding the above observations, considered a distinct species, it would gratify those who cultivate natural history, to have some good discriminative characters of it.

Much has been said and written about antidotes to the venomous bites of snakes, and Mr. Rafinesque enumerates over again several plants which have been said to be, and which he appears to believe to be specifics. If the case was my own, I would be very unwilling to rely upon either of the 20 or 30 medicinal plants, dubiously mentioned by the late Professor Barton, as reputed antidotes for this poison. It would be more prudent to resort unhesitatingly to a more certain remedy, in the ligature, and immediate excision of the part, where such an operation was practicable, or to cauterization, if the part could not be removed by the knife.

In conversation with Professor Cooper upon this subject, he informed me that in his domestic medical practice he applied common chalk to the wounds occasioned by the stings of hymenopterous insects. That in consequence of this mode of treatment, the pain was immediately allayed, and the consequent inflammation and intumescence were prevented. The experiment which led to this result was induced by the supposition that the venomous liquid might be an acid, which opinion was, in some degree, justified by the event.* Upon the same neutralizing principle it must be supposed that any alkali would be beneficial. The learned Professor supposed, that the venom of the poisonous reptilia may, in like manner, be an acid secretion, and recommends this to be ascertained by experiments upon the liquid itself.

If this inference proves correct, the same alkaline remedy may be employed to neutralize, or so modified as to stimulate,

* I have been since informed by Mr. Lesueur, that to his taste the poison was bitter.

in case, as is supposed by some, the poison produces upon the system a typhoid action.

An instance however is related in the Trans. Royal Soc. of Lond. of the unsuccessful administration of the vol. alkali in case of the bite of a Rattle-snake; and an intelligent physician of Georgia informed me, that he had applied the same stimulant in vain for the cure of the bites of poisonous snakes, but that being once stung by a Scorpion, he was instantaneously relieved by the topical use of this liquid. He further related to me a cure performed under his observation, by means of the singular antidote, which has often been resorted to in case of snake bites, that of the application of a living domestic fowl or other bird directly to the wound; three fowls were applied in this instance, of which two died in a few minutes, it was supposed, by the poison extracted from the wound. This account, from an observant medical professor, (who may nevertheless have been deceived) acquires some additional title to consideration by a similar event which lately occurred at Schooley's Mountain, New-Jersey. We are informed from a respectable source, that a boy was there bitten by a Copper-head, (Scytale mockeson.)* The part was immediately painful, became swollen and inflamed, and the sufferer had every appearance of having received a dangerous wound. A portion of the breast of a fowl was denudated of feathers, and applied to the wound; in a few minutes the fowl died, without having experienced any apparent violence or injurious pressure, from the hand of the applicant, the breast exhibiting a livid appearance. Another living fowl was then laid open by the knife, and the interior of the body placed upon the wound. The wound was subsequently scarified, and variously administered to. The boy however recovered, and his cure was generally attributed, at least in part, to the application of the birds. I am as far as any one from relying implicitly upon this mode of treatment, and would only resort to it when the part bitten could not be extirpated, and when a cautery was not at hand. Yet it must be confessed,

* The terminal caudal plates of this individual were bifid, as in the one of Peale's Museum.

that from the numerous attestations to its efficacy we should be almost led to suppose a very strong affinity to exist between the venom and the animal thus applied.

That so numerous a catalogue of plants have gained credit with the uninformed as specifics, will not be surprising, when we know that the reservoir of the venom is very readily exhausted and slowly replenished. When this reservoir is vacated, the reptile is of course innoxious, and the most inert plant would then stand a good chance of gaining reputation with the credulous as a specific.

For a similar reason we have so many cures for the bite of a rabid animal; and it may be for a similar reason that the body of an animal has acquired repute as an antidote, against the venom of a serpent.

Coluber trivittata of Mr. R. p. 80, of this work. Judging from the descriptive name and the locality, is the *C. sirtalis* of authors, or possibly the *C. saurita* or *C. ordinatus*. These serpents have each the three vittæ, though in the two former this trait is much more striking. I know of no other serpent in our vicinity to which the name can be characteristically applied. The *ordinatus* has been called *bipunctatus* and *ibibe* by the French school. What is the difference between *sirtalis* and *saurita?* they must be very closely allied, if not synonymous.

Coluber getulus, Lin. This species attains to a more considerable magnitude than authors have stated. I saw a specimen on Cumberland Island, Georgia, at least five feet long. The ground colour, by the direction of light in which I viewed him, was deep glaucous or livid, he was much more robust than *C. Constrictor.** He permitted my near approach, without agitating his tail in the menacing manner of the serpent just mentioned, and of the crotali, or manifesting any signs of

* This last is the animal, beyond a doubt, judging from the detailed description and plate, which has lately been erected into a new genus, under the name of Scoliophus.....the identity is immediately obvious, to any one acquainted with the specific characters of the above-mentioned coluber. And I presume it can be made apparent, to any one tolerably versed in the science, should proof be thought necessary.

fear. In my anxiety to secure him, he eluded my grasp, and by a sudden and rapid exertion, disappeared, with all the rapidity of movement so remarkable in the *constrictor*. This last, from his celerity, is known in many districts by the name of *Racer*.

Coluber heterodon. This viperine species, of which Latreille has formed a genus under the name of *Heterodon*, varies considerably in its markings, and like most of our serpents, is not constant in the number of its plates and scales, (126, 48—138, 42—141, 42, &c.) perhaps too much reliance has been placed upon colour, and upon the number of the plates and scales beneath the body, of the Ophidiœ generally. In the form of the anterior termination of the head, the *heterodon* is remarkable, and a good specific character may be obtained from the orbital scales, which are eleven or twelve in number; the parabolic curve which passes through the eyes, and terminates at the maxillary angles, is also generally present. This same serpent was figured in Deterville's ed. of Buffon, under the name of *Coleuvre cannelee*. The *heterodon* abounds in many sandy situations, and near the sea-shore. Several persons pursuing a pathway, passed within a few inches of one of them without his betraying any emotion, but the moment he perceived me advancing with my eye fixed upon him, he with a sudden exertion assumed a defensive attitude, by elevating the anterior portion of his body, flattening his head, and 3 or 4 inches length of his neck; these he waved with a steady and oblique motion from side to side, uttering at the same time an audible sibilation, he made no attempt to escape, and seemed absolutely fearless until taken. They have the habit of the vipera, but not the fangs. It seems to be synonymous with *Coluber simus*. This species is often called *mockeson*. Dr. Shaw's description of *Boa contortrix* seems to indicate this species. Was he deceived by an erroneous reference to Catesby's figure of this Hog-nose? or by Forster's catalogue?

Coluber punctatus. A good diagnostic character of this species, in addition to the cervical cestus, rests in the tripple series of abdominal dots; but these are often wanting or obsolete in the young specimen, in which state it is probably the *torquatus* of

Shaw. Sometimes the dots are wanting on the neck and near the cloaca; and in one aged individual, the intermediate line occurred double, and confluent on the throat.

Coluber fulvius, this species is said by Daudin to be closely allied to his *C. coccineus*, notwithstanding the difference in plates and scales. But it is certainly very distinct by other characters, and strikingly so in its perfectly annular black and red bands; the latter are margined with yellowish and spotted with black. A specimen has 224 plates and 32 scales, total length 21 inches, length of the tail $1\frac{9}{10}$ inch. The *coccineus* has the under part of the body whitish, immaculate. The *fulvius* seems to belong to the genus *vipera*; it has the fangs, but not the orifice behind the nostril, which communicates with the reservoir of venom, so conspicuous in the *crotali*, &c.

Ophisaurus ventralis. The tail of this snake not only breaks in pieces when struck with a weapon, but portions of it are thrown off at the will of the serpent. This singular fact I witnessed in Georgia. This is one of the many which are called horn-snakes. A tip of the tail of one of them was once brought to me as having been taken from a recently withered tree, which the bearer assured me was destroyed by the insertion of this formidable instrument, and it was not without considerable difficulty he was convinced of the innocence of the tail, and of having been the dupe of a knave. There seems to be a peculiar character in the mode of imbrication of the scales of this species, each one of these at the lateral edges, passes beneath the lateral scale on one side, and over the edge of the opposite one. It has been described under five different generic names, and four different specific ones.

The *Crotali* do not gain a single joint only to the rattle annually, as is generally supposed. They gain more than one each year, the exact number being probably regulated in a great measure by the quantity of nourishment the animal has received. Rattle-snakes in Peale's Museum have been observed to produce 3 or 4 in a year, and to lose as many from the extremity during the same time. Hence it is obvious, that the growth of these curious appendages is irregular, and that

the age of an individual cannot be determined from their number. Mr. Rubens Peale informed me, that a female of *Crotalus horridus*, Beauv. *durissus*, Daud. which lived in his Museum more than fourteen years, had eleven joints to her rattle when first in his possession; that several joints were acquired and lost annually, and that at her death, which occurred last year, she had the same number as when brought to the Museum; she had, however, during that time received an accession of four inches to her length. Her death was occasioned by an abortion.

The *C. adamanteus*, Beauvois. *Rhombifer*, Daud. is by much the largest of our North American serpents, and doubtless is the species which Catesby saw a specimen of, eight feet long.

Crotalus miliarius varies in some characters from those laid down by authors. A specimen within my view has five dorsal series, of alternate, irregularly orbicular black spots, those of the intermediate series are obsolete, and slightly connected across the back, those of the vertebral series have not red centres, and are edged with a white line; the ventral spots are disposed adventitiously, so as not to be traced into longitudinal series; they are large, black, irregularly orbicular, and occupy about one half of the surface, which is white. Ventral plates 140; subcaudal, 33, of which the six terminal ones are bifid. Joints of the rattle with but one transverse contraction on the middle of each, besides the terminal contraction. Total length 1 foot $4\frac{1}{4}$ inches, tail two inches. It appears to be more vindictive than the two species before mentioned. The individual here noticed we encountered in East Florida; he struck at Mr. W. Maclure and myself successively as we passed by him, without any previous intimation of his presence, owing to the inaudible smallness of his rattle, and its having but three joints; he was killed by Mr. T. Peale, (whom we preceded) while preparing for another assault. This incident is noted as a contrast to the anecdote of the *Coluber heterodon*.

Salamandra alleganiensis, Daud. appears to be synonymous with *S. gigantea* of Dr. Barton. It was first described by Mr. Latreille in Deterv. Ed. of Buffon, tom. 11. The name *allega-*

niensis, although defective, as it indicates no character, has however the unalienable right of priority.

Salamandra subviolacea, Barton. This name has been rejected by Mr. Daudin, and substituted by that of *venenosa*, I do not know for what reason, as none is assigned.

Salamandra punctata, Gmel. This appellation was originally given and restricted to the *stelio* of Catesby. tab. 10. (represented in the bill of Ardea Herodias) and was adopted by many subsequent authors, but was finally rejected by Daudin, who considered the species the same as Barton's *subviolacea*. He concurred with Mr. Latreille in appropriating the name thus rejected to *var.* β of *Lacerta*, *aquatica* of Gmel. Notwithstanding this high authority I cannot but coincide with Professor Barton in this instance, in believing it altogether distinct. The single character of the subocellate spots, though not remarked by this author, is a sufficiently discriminative one; these ocellæ are always present, and in no one of the varieties I have seen has the approximation to the *subviolacea* been so considerable as to render a specific discrepance equivocal. Catesby's variety with the ocellæ on the tail seems to be the least common; in general these spots, or epupillate ocellæ, are exclusively confined to a line on each side of the back, about six in each, extending from the base of the head to the origin of the tail, though there are sometimes scattered smaller ones on each side of the body, and upon the vertex of the head, they are of a beautiful reddish colour, enclosed by a definite black areola; the upper part of the body is brownish, with numerous, distant black points, and a slight vertebral, obtuse carina, the inferior surface of the body of a fine yellow or orange, with distant black points, the tail* is compressed, ancipital, attenuated to an obtuse tip, longer than the body, and punctured with black in like manner. The younger specimens vary considerably, in being, on many parts of the body, destitute of black punctures, and in having the dorsal and

* Dr. Barton remarked that this part is rounded, (cauda teres,) this observation was not autoptical, but dictated most probably by the appearance of Catesby's figure. In the young animal the tail is less compressed than in the old one.

ventral colour, of the same pale orange. It is decidedly aquatic. Several specimens are preserved in the collection of the Academy of Natural Sciences, and from these it is evident that the reddish colour of the subocellate spots is destroyed by the action of the antiseptic liquid; to this circumstance it is probably owing that these spots have been hitherto described as white.

After stating these differential traits, it may be proper to observe, that the *S. maculata* of Shaw is synonymous with the above. But I think it most proper to restore Gmelin's name *punctata*, which will afford an opportunity to do justice to the memory of Laurenti, by reviving the original name by which he distinguished the *Var. β. of Lacerta, aquatica*, Gmel., that of *parisinus*.

Bufo cornuta. This animal, which has been stigmatized as the most prodigiously deformed *creature* known to exist!! is generally supposed to inhabit North America as well as Surinam. I do not think it has ever been found in North America. Shaw, in Nodder's Nat. Misc. says it is principally found in Virginia, but in his General Zoology, I think he says that Seba was in error when he represented its native country to be North America. Two other species of *Bufo* have been correctly stated to inhabit this country, viz. *B. musicus*, and *Crapaud rougeâtre*, Daud. (B. rubidus) first noticed as distinct by Mr. William Bartram. I discovered a third species on the banks of St. John's river, East Florida, which, as I am not at present prepared to describe, I shall not surreptitiously name.

It is, I conceive, an incumbent duty on the describer of a natural object, to deposit his specimen, or a duplicate, when practicable, in some cabinet or museum, to which he should refer, in order that subsequent writers may be satisfied with the accuracy of his observations, by examining for themselves. By such reference, and by the re-examination of the same objects by others, the plethoric redundance of synonyma, that prolific source of accumulating error, will be banished or elucidated, and naturalists will most readily arrive at the knowledge of truth, which is, or ought to be, the grand leading object of their labours.

PHYSICS AND CHEMISTRY.

Art. XI. *Outline of a Theory of Meteors.*

By Wm. G. Reynolds, M.D. *Middletown Point, New-Jersey.*

SHOULD the progress of science, for a century to come, keep pace with its rapid advancement for the last fifty years, many appearances in the physical world, now enveloped in obscurity, will then admit of as easy solution as the combustion of inflammable substances, or any familiar process in chemistry does at this day. Among the many subjects from which the veil of mystery would thus be raised, we may include those luminous appearances, in the aerial regions, called meteors, which I am about to consider in the following essay; and which seem to constitute a distinct class of bodies of considerable variety.

Meteors were regarded by the ancients as the sure prognostics of great and awful events in the moral and physical world; and were divided by them into several species, receiving names characteristic of the various forms and appearances they assumed; but of their opinions, as to the physical cause of these phenomena, the ancients have left us nothing solid or instructive. The moderns, more enlightened, have ceased to regard these bodies with the superstitious awe of former ages; but in respect to the cause thereof, are perhaps but little in advance of their predecessors, having, I believe, produced nothing yet that will bear the test of philosophical investigation.

Doctor Blagden (Philosophical Transactions, 1784,) considers electricity as the general cause of these phenomena; Doctor Gregory, and others, think they depend upon collections of highly inflammable matter, as phosphorus, phosphorated hydrogen, &c. being volatilized and congregated in the upper re-

gions of the air. Doctor Halley ascribes them to a fortuitous concourse of atoms, which the earth meets in her annual track through the ecliptic ; and Sir John Pringle seems to regard them as bodies of a celestial character, revolving round centres, and intended by the Creator for wise and beneficent purposes, perhaps to our atmosphere, to free it of noxious qualities, or supply such as are salutary. Many other theories, as ingenious as fanciful, might be enumerated ; but without commenting on their comparative merit, I must acknowledge that none of them have yet impressed my mind with a conviction of their truth. A series of observations, however, have enabled the moderns to ascertain, with apparent accuracy, several particulars relative to these stupendous bodies, which add much to our knowledge of their general character :—their velocity, equal to 30, and even 40 miles in a second of time ; their altitude, from 20 to 100 miles ; and their diameter, in some instances, more than a mile, are facts we derive from respectable authority, and may aid us, essentially, in forming just conceptions of their nature and properties.

I believe meteoric stones to result from all meteoric explosions ; limiting, however, the term meteor to those phenomena, in the higher regions of the air, denominated fire-balls, shooting-stars, &c. That these bodies move in a resisting medium, must be evident to every attentive observer ; and that this medium is our atmosphere, is pretty certain, 1st. Because we know of no other resisting medium round the earth ; 2dly. Because the same kind of resistance is apparent at every intermediate altitude, from their greatest to their least, which last we know to be far within our atmospheric bounds ; and, 3dly. Calculation has, in no instance, assigned them an elevation beyond the probable height of the atmosphere.

That meteors proceed from the earth, that they arise from certain combinations of its elements with heat, and that meteoric stones are the necessary result of the decompositions of these combinations, are opinions I will endeavour to support, by the following considerations.

1st. The properties and habitudes of matter, under certain conditions and combinations.

2dly. The situation of the earth's surface in respect to the sun, the influence of his rays thereon, and the nature of the elements or compounds on which these rays act:

And 3dly. The identity that exists between the component parts of meteoric stones, and the elements that enter abundantly into the composition of our globe; and, by several other facts and arguments.

Under my first general specification, I will select such principles from the established doctrines of philosophy, as have an immediate bearing on the subject; without engaging in any of those subtle speculations in which certain recondite properties of matter, or the identities of quality and body are affirmed or denied.

Thus, 1st. Heat is the universal cause of fluidity and volatility in bodies; hence no solid can assume the state of gas, until it absorbs, or unites with, a certain portion of caloric; and the subtilty and volatility of compounds thus formed, will be in a due ratio to the quantity of caloric they employ.

2dly. The heat employed to maintain a body in the gaseous state, is said to be latent or fixed, and may be regarded as an ocean or atmosphere of fire, holding the ultimate particles of the body in a state of extreme division, and wide separation, from which they can be driven only by some change in the affinities or condition of the compound.

3dly. If the latent heat in a gaseous compound be suddenly abstracted, as in explosion, its escape is attended with the emission of light and sensible heat, when the volatilized particles held in solution being no longer able to maintain the state of gas, suffer approximation in a due proportion to the quantity of caloric they have lost.

4thly. Caloric, in reducing solids to the state of gas, lessens, but cannot in any case, as far as we know, totally destroy their gravitating force; the diminution of this force, however, being in a direct proportion to the quantity of heat employed.—Hence the following inferences may be fairly drawn, as they seem to be in unison with the relative dependence and harmony existing between the material elements of this globe, and, I believe, are contradicted by no direct experiments; viz. that

the expansion of volume, specific levity, and subtilty of artificial gases, are in a direct proportion to the absolute quantity of caloric they employ; and the caloric is in the same proportion to the insolubility of the substance with which it unites.

5thly. When the specific gravity of bodies on the surface of the earth, is reduced below that of the superincumbent atmosphere, they ascend to media of their own density, in obedience to the laws of Aerostatics; thus we raise balloons by filling them with light air, and the carbon of pit coal and common wood exposed to combustion, and water to the sun's rays, will rise until they reach a medium of like specific gravity with themselves.

6thly. Mechanical agitation and division assist the solution of solids, by bringing fresh portions of the menstruum into successive contact with their fragments, and thus exposing a larger surface.

Under the second head I proceed to notice the situation of of the earth's surface in respect to the sun, &c. The atmosphere is a thin, elastic, gravitating fluid, that completely envelopes the earth, to which it may be considered a kind of appendage or external covering; its base resting on the earth's surface, is of an uniform density, growing rare as it recedes therefrom, in a due ratio to the diminution of its gravitating force, until it is lost in empty space. The atmosphere is estimated on certain data to be about 44 or 45 miles high, but we have good reasons to believe it fills a much wider circle, though too thin to reflect the rays of light above its reputed height.

The earth presents one whole hemisphere to the sun in unerring daily succession; and those parts of it which have the least protection against his rays, will, cæteris paribus, suffer the greatest intensity of their action. Within the tropics, the atmosphere opposes less resistance to the sun's rays than in the temperate zones; and in both large tracts of cultivated land, the summits and sides of great ranges of mountains, margin of oceans, rivers, &c. present an almost naked surface to

their influence.* The exterior strata of the earth, and especially the more exposed parts thereof, envelope in their compounds, elements of an identity of character with those composing meteoric stones.

The atmosphere is the great recipient of all volatilized bodies; it possesses but feebly the powers of a solvent, unaided by heat or moisture, but when these are adjuvants, no body in nature can totally resist their action for a long time.

Now if the above principles are admitted, we have in their application a reasonable solution of most meteoric phenomena. Thus, the rays of the sun darting through the atmosphere reach the surface of the earth, where, by accumulation, they produce sensible heat, which though not intense, is steady and uniform, for many hours every day; minute portions of the earthy and metallic compounds exposed to the sun's influence, will be volatilized by the absorption of heat, and thereby assuming the state of elastic fluids, will ascend until they arrive at media of their own density. The atmosphere in contact, will have some of its particles blended in these compounds, will ascend with them, and to supply the vacuum, new portions of air will rush in and ascend, and the process will continue until the sun's rays are withdrawn, or interrupted by some of the common occurrences of nature.

The utmost height to which these elastic fluids ascend, may be estimated at something more than one hundred miles; and they float at every intermediate distance between their greatest elevation and the clouds, but rarely below the latter, except their course is directed towards the earth in their explosions. They probably ascend at first in small daily detached portions of gaseous clouds, and are diffused over wide regions; but having no sensible resistance opposed to their mutual attraction, they will by the laws of their affinities congregate into immense volumes of highly concentrated elastic fluids, which on exploding will exhibit all the phenomena of bursting mete-

* Here we might properly enough notice the high-ways, streets, and pavements of cities, &c. on which the materials being minutely divided by attrition, are in a better state for the sun to act freely on, and will consequently yield greater products than equal areas of undisturbed surface, under like circumstances of heat.

ors in the following manner, viz. the latent heat on escaping will manifest itself in the form of fire and light, the force with which it strikes the atmosphere, or the rebound of the latter to fill the vacuum, or both, will occasion sound more or less detonating or hissing, as the escape is more sudden, or the atmosphere more dense ; the earthy and metallic particles on the escape of caloric, will obey the laws of cohesive attraction, clash together, recover their gravity, and descend to the earth in masses, or shattered fragments.

Meteoric stones frequently bear the marks of violence, which is doubtless owing to the conflict sustained at the moment of explosion ; their difference in size depends on the difference of magnitude in the disploding volumes ; something like regular arrangement is frequently perceived in the structure of these stones, because in all productions of solid from fluid matter, the consolidating particles possess a tendency to arrange themselves in the order of their affinities. It is thus the various arrangements in saline crystallization, the freezing of water, and cooling of melted metals, may be accounted for. There is a real, as well as an apparent difference in the velocity of meteoric bodies ; the first arising from their difference of magnitude and the violence of the explosion, as well as from the resistance they meet ; the latter, from the different distances at which they are seen. The gradation of colour, from a bright silvery hue to a dusky red, is owing, in a certain degree, to the state of the atmosphere refracting different coloured rays, and also to the materials in the compound, similar to the different hues in artificial fireworks. Reddish and white nebicula are sometimes left in the tracks of meteors, which are nothing but ignited vapours, or the particles brushed off the burning body by the resisting atmosphere. The velocity or motion and direction of meteors, depend upon principles well known and daily practised by engineers, and the constructors of fireworks.

The immediate cause of these explosions is a little obscure, and merits a fuller detail than is compatible with my present limits ; their analogy to the electric phenomena in the clouds, leaves room to suppose they are effected by certain modifica-

tions of electricity. Clouds of opposite electricities will approach each other and explode, by the positive imparting as much electrical fire to the negative cloud as will make them equal, when just as much water as the imparted fire held in solution, will be set at liberty and descend to the earth. If, however, this solution be deemed inapplicable, perhaps the following may be admitted. Thus, when heat is urged upon incombustible* bodies with a force that overcomes the cohesive property by which their particles are tied together, it unites with them in large quantities, and becomes latent, by which union they are reduced to the state of elastic fluids; and as it is a universal property of heat to counteract the gravitating force of bodies, these compounds must necessarily become volant, and ascend as above stated. It is only thermometrical or sensible heat, that destroys the attraction of cohesion existing between the particles of bodies, the repulsive power of latent heat being barely able to counteract this property, when the elements under its dominion are removed beyond a certain distance from each other; now the very reduced temperature in the high regions to which these gaseous clouds will ascend, may admit their earthy and metallic particles within the sphere of cohesive or aggregative attraction, when the caloric will be expelled like water from a sponge, accompanied by all the phenomena above stated.

The third general head of my subject leads me to inquire into the constituent principles of meteoric stones: sundry papers on the analysis of these productions, have been furnished us by chemists of acknowledged reputation and ability, and in none of these that I have seen, was there any element described that had not been previously known. But should it hereafter be found that air stones contain matters not found on our globe, the fact will afford no absolute proof of the foreign origin of these stones, as we are successively discovering earthy and metallic principles of distinct characters from those already known.

* Perhaps there is no body in nature absolutely incombustible, but I use the term here in common acceptation.

A portion of one of these stones that fell in the town of Weston, (Connecticut) examined by the late Dr. Woodhouse, gave the following results in a hundred parts, viz.

Silex	50	
Iron	27	
Sulphur . . .	7	
Magnesia . . .	10	
Nickel	1	inferred from chemical tests.
Loss	5	
	100	

"The sulphur was seen by the naked eye distributed through the silex in round globules the size of a pin's head, after dissolving the powdered stone in diluted nitric acid."

All specimens of these stones do not afford precisely similar results, but differ in their constituent elements and relative proportions; their component parts, however, are to be found abundantly in schist, schorl, pyrites, pebble, granite, &c. on which the sun must daily act.

The following facts go to strengthen the above theory, viz. Meteors are most frequent and stupendous in tropical countries, where the heat of the sun is most intense; and less frequent in our climate in the winter and spring, while, and after the earth has been covered with snow for many weeks in succession; and they are most frequent in the higher latitudes towards autumn, after a continuation of hot dry weather: out of the whole number (179) of shooting stars I have noted during the last twelve years, 149 appeared between June and December, inclusive.

If it be said that the specific gravity of meteoric stones being several times that of water, it is absurd to suppose they can rise, (if even reduced to the state of gas) to the elevated stations here assigned them, seeing the vapours of water can ascend only one or two miles above the earth. To this I reply, that the doctrine of heat is not yet so thoroughly understood, as to acquaint us with all its habitudes with natural bodies, but we infer from analogy, that the more refractory a body is in

the fire, the greater in a due ratio is the absolute quantity of heat required to reduce it to, and retain it in, the state of gas, and the greater, in a corresponding degree, will be the dilatation of its particles and decrease of its specific gravity. Hence, if water reduced to vapour by heat, be capable of assuming an altitude of two miles, it follows that more refractory substances reduced to a similar state, will suffer expansion and fugacity in a due proportion to the quantity of caloric they employ, and will assume a corresponding elevation, as already inferred under my first head.

Another objection may be, that though high degrees of heat affect certain solids as above stated, yet these cannot be sensibly acted on by such feeble agents as atmospheric air and the rays of the sun. I answer, if it be admitted that sensible heat acts on solids in an increasing ratio to its intensity, *it follows* that lower degrees, though acting in an inverse ratio to higher, must affect the same bodies in a conceivable degree at any temperature above their natural zero :* and though the heat of the sun beating on a plane surface for several hours is feeble, compared with that produced by a burning lens, or air furnace, yet if it be sufficient to detach from one square foot of the earth's surface the 104023 part of a grain in twenty-four hours, the quantity taken from 100 square miles, in the same time and proportion, would amount to ten pounds, which is abundantly sufficient for all meteoric phenomena; and the loss to each square foot, supposing the process to be uninterrupted, would be no more than one grain in 284 years. When we advert to the intense heat produced by concentrating a few of the sun's rays in a burning lens, the whole quantity daily sent to the earth must strike us forcibly. If collected in a lens of

* It may be easily proved that water evaporates (though slowly) at a temperature many degrees below its freezing point; and these vapours are more subtle and elastic than those formed at the boiling point of that fluid

REMARK.

It is indeed proved that vapour is formed from water at the lowest temperatures, but is *less elastic*, the lower the temperature, as appears from its sustaining a continually decreasing column of mercury, the lower the temperature at which the vapour is formed. Vide Dalton's and Gay Lussac's experiments. *Editor.*

sufficient magnitude, they might volatilize a space equal to the state of New-York in a moment of time! As all bodies possess a limited capacity for heat, does it not follow that there must be some outlet to its perpetual accession to our globe, or the earth would soon become so highly ignited as to glow with the fulgour of a meteor? And may not this outlet be found in the above described compounds? which serve as conductors of the surplus of heat from the earth to the higher regions of the air, where on being freed by displosion, from the grosser matters incumbering it, it finds a rapid passage to its great archetype and parent, the SUN. Thus his daily waste may be restored, and an equilibrium, by the return of his own emanated particles, preserved, between the sun and the earth, and probably all the planets of our system.

The last consideration I shall offer in favour of the domestic or earthly origin of meteoric phenomena, is the difficulties that present to our granting them a foreign one. Though I am well aware of the respectability of the names which the theory of moonstones can summon to its support, yet I have always regarded it as unfounded and unphilosophical for the following reasons, viz. 1st. Whether the moon has an atmosphere or not, we will all admit that she has attraction, which must extend to many thousands of miles from her surface. No projectile force that we are acquainted with can throw a heavy body 100 miles, even though no atmospheric, or other resistance than its own gravity, were present; hence the idea of that force extending to thousands of miles from the moon's surface, is gratuitous and nugatory. 2dly. The products of volcanoes bear no similarity of origin, or kindred resemblance to meteoric stones; those are lavas of different kinds, pumicestone, scoria, ashes, &c. these solid masses of matter, with some degree of regularity in the arrangement of their constituent particles. 3dly. The descent of these stones has no coincidence in point of time with the position of the moon. She is as often in their nadir as their zenith. We also witness in all cases, explosion and light in our own atmosphere, at the time of the descent of these stones. This could not be the case if they proceeded from the moon, for obvious reasons. 4thly. The heat ade-

quate to such projectile force as would carry a body from the moon's surface beyond the sphere of her attraction, would volatilize the matter of meteoric stones in a moment ; hence they would not be projected from the Lunarian crater in solid masses, but in elastic vapour.

In conclusion, although the theory which I have endeavoured to elucidate and establish, be subject to some difficulties and objections which science may hereafter remove, it appears to me perfectly consonant with the relative dependence and harmony of our system, and by no means at variance with the infinite wisdom and power by which it was originated.

ART. XII. *Observations upon the prevailing Currents of Air in the State of Ohio and the Regions of the West, by* CALEB ATWATER, *Esq. of Circleville, Ohio; in Letters addressed to His Excellency De Witt Clinton, LL. D. Governor of the State of New-York, and President of the Literary and Philosophical Society.*

(Communicated for the American Journal of Science, &c.)

Circleville, Ohio, July 23, 1818.

DEAR SIR,

WITH pleasure, I acknowledge the receipt of the circular letter bearing date the 5th instant, which you addressed to me, for which you will be pleased to accept my warmest acknowledgments for yourself personally, and the Philosophical Society of which you are the president. To answer all the questions which are put to me in that letter, is not at present within my limited means, either as it respects the leisure or the ability. I shall therefore, at this time, confine myself to "observations upon the prevailing currents of air in the state of Ohio."* These observations will be wholly founded on personal experience, during the four years in which I have traversed this state, from Lake Erie to the Ohio river, whilst at-

* We have taken the liberty to give Mr. Atwater's Memoir a more extensive Title, for reason's that will be obvious on a perusal of the piece.

tending on the several courts, in all seasons and in all the changes of weather.

The prevailing currents of air, one of which generally obtains in Ohio, are three.

The first comes from the Mexican Gulf, ascending the Mississippi and its larger tributary branches quite to their very sources.

The second proceeds from the back of mountains to the west, descends the Missouri to its mouth, and then spreads over a vast extent of country.

The third comes down the great northern and north-western lakes to the south end of Lake Michigan and the southern shore of Lake Erie, where it spreads over the region of country lying to the south of them.

That current of air which comes from the Mexican Gulf, is warmer, and perhaps more moist, than any other which prevails here. After a few days prevalence, it uniformly brings rain along with it. That this current of air should be very warm may be readily conceived, when we reflect that it comes from a hot tropical region; and that it should be very moist, excites no surprise, when it is considered, that in its passage upwards it passes wholly over water, and through the warm mists and fogs constantly ascending from the Mississippi and its tributaries. This current prevails much more along the Ohio river than it does at any considerable distance from it. One consequence is, that the climate in the immediate vicinity of the Ohio river is warmer, than it is either north or south of it, unless you go to the southward a considerable distance. Other causes may, and probably do, in a greater or less degree, contribute to produce this result, and I will here state them:

First, The Ohio runs on a surface less elevated above the sea than the country, either north or south of it, but this difference is trifling through the whole of the sandstone formation. This formation prevails from the head of the Ohio to Aberdeen, which is opposite to Marysville in Kentucky, at least two-thirds of the distance which that river washes the southern shore of this state. The reason is obvious, because there are no falls in a sandstone formation.

Another cause which contributes to produce a warmer climate, especially in the winter season, in the valley of the Ohio, is, that several considerable streams which empty themselves into the Ohio, have their sources on the high lands, a great distance to the south of it; for instance, the Great and Little Sandy, and the Great and Little Kenhawa, which descending from a warm region of country, their waters contribute to keep the Ohio open in winter.

But these causes are by no means sufficient to produce the one half of the comparative warmth of climate observable in the immediate vicinity of this invaluable river. To prove that the climate is much milder in the southern than in the northern part of this state, I will proceed to mention several facts, which have fallen under my own observation.

In the latter part of last February I was at the town of Delaware, on the Whetstone Branch of the Scioto river, between eighty and ninety miles south of Lake Erie, and twenty-five miles north of Columbus, the seat of government, which is near the centre of the state, where I saw a number of gentlemen direct from Detroit, by the way of Lower Sandusky, who informed me that the snow at that time was eighteen inches in depth and upwards all along the lake shore, but gradually decreased as they came south until they arrived at Delaware. At that place it was then about twelve inches deep in the open fields, and somewhat deeper in the woods. I descended the road along the Whetstone to Columbus, the snow decreasing in depth all the way as I proceeded. At Columbus it wholly disappeared in the fields, and only ice was found in the road, which also decreased until I came to the Big Walnut Creek, thirteen miles south of Columbus, where it disappeared, and the road began to be muddy. As I still proceeded south, the mud increased in depth until I came to Chillicothe, about thirty-two miles south of Big Walnut, where the frost was entirely out of the ground, and the roads were almost impassable. As I still descended southward, along the Scioto, I found that at Piketon, on the Scioto, nineteen miles south of Chillicothe, the road had considerably improved. I proceeded onwards to Portsmouth on the Ohio

river, at the mouth of the Scioto, about twenty-six miles south of Piketon, where the ground was entirely settled, and the innkeeper, where I lodged, was making his garden, sowing his sallad seed, and planting his peas. This journey was performed in three days, and in travelling only one hundred and fifteen miles from north to south, this extraordinary difference of climate was observed.

A traveller may leave Portsmouth when the farmer is beginning to hoe his corn the first time, and travel with good speed to Delaware, and find the husbandman just beginning to plant.

Instances which have fallen within my own personal observation might be multiplied to a great extent, but a few may suffice.

Generally speaking, there is a difference in the beginning and ending of the warm season of about two weeks between Portsmouth and Delaware, or of three weeks between the former place and Lower Sandusky.

In relation to the warmth of the climate, I will state two other facts, originating, as I believe, in the prevalence of the southern current of air from the Mexican Gulf along the Ohio river.

First, In the summer months the paroquet ascends the Scioto more than one hundred miles from its mouth, and until within a few years past, wintered at Miller's Bottom, and at other places along the banks of the Ohio, near its great southern bend in latitude 38° north, in Gallia and Lawrence counties, in the state of Ohio. I have seen them there in all the winter months in considerable numbers, but few however now winter there ; and probably if the cold northwestern current of air from the great lakes becomes more and more prevalent in the winter months, these birds will migrate altogether to a more southern clime.

Are these birds found as far to the north on the east side of the Alleghany by at least three degrees ? Monsieur Volney, Mr. Jefferson, and otbers, say not. It has been denied that this fact proves any thing more than that this bird frequented these parts in quest of its favourite food. This food is grass

and other vegetable matter in summer, and the cockle bur, and the balls of button-wood, or, as by a perversion of language, it is called in this country sycamore.* But this bird may find its favourite food as well east as west of the Alleghanies. The grasses and trees alluded to, flourish as I have observed in forty-five degrees of north latitude, and I am credibly informed that they are abundant as far north as Quebec, and even around Hudson's Bay. Wherever waters run and trees grow on their banks, (if low and wet,) on the American continent, even as high as eighty degrees of north latitude, there the paroquet may find its food in abundance.

Another fact tending to establish the same point is, that the reed cane, before this country was much settled, grew in a higher latitude by several degrees on this than it did on the other side of the Alleghany mountains. It has indeed been said, that this cane was never found north of the Ohio, nor above the mouth of the Big Sandy River, which empties into the Ohio, on the line which separates Virginia and Kentucky. This however is incorrect; for within a few years it was growing in abundance at Miller's Bottom, twenty-six miles above the mouth of Big Sandy. It grew at Lancaster, on the Hockhocking, northward of the mouth of the Big Sandy, in a direct line, at least one hundred and fifty miles, and it now grows on the Whetstone branch of the Scioto, more than two degrees of latitude above the lowest bend of the Ohio, which is at the mouth of the Big Sandy. Before the white people settled there, I have every reason to believe, that the cane grew in great plenty at Delaware, where there are more signs of buffaloes than at any other place within my knowledge. It has been conjectured, that the seed of the cane was brought down and scattered by the Big Sandy; but granting this, in what way could that stream carry this seed up the Hockhocking and Scioto to their sources? to places several hundred feet above the highest freshes ever known in this country? With a knowledge of these facts, cast your eye at the map of Ohio. Proofs

* Genus, *platanus*—species, *occidentalis*, popular name, sycamore, or button-wood.

within my reach might be multiplied to a much greater extent, but they are probably unnecessary.

But another current of air prevails here, especially in the cold months, coming from the mouth of the Missouri, which is a little to the south of west of this place. This current is colder than the preceding one, and though moist, yet not as much so as the one already described. It prevails generally in October and November, before our warm weather is over, and produces frosts and a chilly dampness, and what I have observed nowhere else, especially on the east side of the Alleghanies, it produces a kind of faintness at the breast.

People of delicate habits, coming here from the northern and eastern states, uniformly complain of this faintness. It is not perhaps extraordinary that this current of air should be cold, proceeding as it does from a high northern latitude, along the great chain of rocky mountains in the northwest; that it should be moist, and perhaps also that it should affect the animal economy unpleasantly, may possibly be attributed to its passing such a length of way over the waters of the Missouri, and the wet prairies and barrens lying so extensively between us and the head waters of that stream. The luxuriant vegetation which covers these prairies and barrens at that season of the year, begins to putrefy, and fills with unhealthy exhalations every gale of wind which passes over them.

At the mouth of this river (Missouri,) which is in about latitude 38° north, this current of air is extremely cold in the winter months. It diverges from this point, and produces extreme cold at a considerable distance to the south of it on the Mississippi river. General Rector, the present surveyor general of the United States, who keeps his office at St. Louis, informs, that he has known the Mississippi at St. Genevieve, in latitude about 37°, so firmly covered with ice in one night, as to be able to bear horses and cattle the ensuing day. This circumstance must have been owing to the sudden change of the current of air from south to the northwest, descending the Missouri river from the cold regions at its sources.

From several gentlemen, residents for many years in Illinois and Missouri Territories, I have been informed, that changes of weather in that region of country are, especially in winter, very frequent and great; that one day the moist south wind from the Mexican gulf will prevail, and produce quite warm and mild weather for the season; on the very next, or frequently in the latter part of the same, the current of air from the sources of the Missouri will prevail, and block up the streams with ice.

There is a third current of air which prevails during our winter months, more and more, annually, as the country becomes cleared of its forests in the direction alluded to; it proceeds from the great lakes to the northwest of us, and even beyond them. Proceeding as it does from the north and northwest of lake Superior, and crossing the great expanse of water in this direction, it rushes down these great lakes to the south end of lake Michigan in latitude about 41° north, diverges from that point, and spreads over the immense regions lying to the south, where the air is more rarefied by reason of its warmer climate. This current of air brings along with it intense cold, and extended last winter even to New-Orleans, where the snow fell to such a depth, that sleighs were seen passing in every part of the city. The more the forests are cleared away between any place in this country and the northern lakes, the more this cold current of air will prevail. This current also diverges from the southern shore of Lake Erie, but is not so strong as that part of it which diverges from the south end of Michigan, and of course does not extend as far to the south. When this part of this state was first settled, this current of air was hardly felt at this place, and then only for a short time in the winter months, and hardly ever reached the Ohio river; but last winter it continued three weeks at one time, and produced good sleighing; and also caused rheumatisms, pleurisies, peripneumonies, &c. which proved mortal to some. In this place, which is in latitude about 39° 20′ north, the thermometer of Fahrenheit, hanging in an entry of a dwelling-house with closed doors, sunk to 24 degrees below zero. This extreme cold may

be attributed to general, rather than to local causes, and it may be said that the winters all over the world have been colder of late years than formerly. But on the very day, when it was thus cold, (if newspapers can be believed) a great number of vessels put to sea from Reedy Island in the Delaware below Philadelphia, and about thirty sail of vessels went to sea from New-York harbour.

All our streams were at the same time bridged with ice of great firmness as well as thickness, and continued to be so for a considerable time afterward, until the warmer current of air from the south prevailed over the current from the lakes. It will be proper, and may be necessary, here to state, that the latitudes of several places in this country are very different from what you would be led to believe from examining any map or chart now or ever in existence. For instance, Lake Michigan extends farther south than Fort Wayne, which place by actual survey is in this state; St. Louis is not 38°, and the most southern point or bend of the Ohio river, is not more than latitude 38° north. I state merely what I am informed of by those who have ascertained these facts by actual observation and survey. The place opposite the mouth of the Big Sandy, is nearly as far south as Lexington in Kentucky. The south end of Michigan lake ought to be laid down on the map 41° north. Prevailing currents of air (not every breath of air which moves over the surface) I have attempted to describe. It may be well enough, however, to mention some other currents which sometimes prevail for a few days. And here I will mention what our oldest settlers along the Ohio have observed, that is, that whenever in a dry time, there is a current of air proceeding down the river for three or four days in succession, the current from the Gulf of Mexico is sure to drive it back with redoubled force, and after blowing a day or two, it is equally sure to bring rain with it. It is easy to assign a cause for it; for meeting the trade winds in the Gulf, it is driven back with redoubled violence to the sources of the larger streams which empty themselves into the Gulf.

When a thunder storm, proceeding in either a western or eastern direction, as the case may be, happens to strike a large

water-course running either north or south, and when also there happens to be a large branch emptying into the stream, within a few miles either above or below the point where the storm approaches it, I have uniformly observed the storm to cross the large stream at the point where the large branch unites with it, and ascend the branch. Where there are two large tributaries about equi-distant from the point of approach, the storm frequently divides and follows each of them. The reason why it should be so, this is not the place to discuss; but the Wisdom and Goodness which so ordered it, are too apparent to every rational mind to be overlooked. It may be asked if the difference in latitude and elevation between the Ohio and lake regions of country, does not produce a great difference in the climates of those respective regions? These causes certainly produce some difference, but not all. It is my object to establish facts, rather than any favourite theory. The difference of latitude between the Ohio river at the mouth of the Scioto, and lake Erie at the mouth of the Maume or Sandusky, is nearly three degrees, and the difference of elevation above the sea is trifling, if any. From the mouth of the Scioto to Columbus, about 90 miles in a direct line, the water, where there is what is commonly called a *ripple*, runs briskly, and these ripples happen, perhaps, one to a mile; but they are in a sandstone region, and the fall of course is trifling.

Let us suppose then, that the river Scioto descends one hundred feet from the mouth of the Whetstone, which empties into that river at Columbus, to the Ohio, and that the Whetstone which runs through a limestone formation, descends another hundred feet, which would make Upper Sandusky two hundred feet higher than the Ohio river. From this highest ground between the Ohio and the lake, it is a well-known fact, that the land descends towards the north much more in a given distance, than it does towards the south, and the distance is not half as far. The Maume and other streams putting into the lake, are full of rapids. Admitting for argument's sake, that the Sandusky or Maume descend only 100 feet, then the surface of the lake is 100 feet higher than the Ohio river. Would

three degrees of latitude, and 100 feet greater elevation produce three weeks difference in the seasons? Is there that difference between Baltimore in Maryland, and Wilkesbarre in Pennsylvania? Is there that difference between New-York and Fort Edward on the Hudson? It is believed that there is not one half that difference.

I have referred but little to thermometers, because they are kept in so many different situations by their owners, that I have known no less than 8 degrees of difference between several of them kept in one town, within almost a stone's throw of each other, at one and the same moment of time.

Every allowance being made for other causes, I am still of the opinion that the difference in the climates of the Ohio and lake regions of country, is to be attributed chiefly to the prevalence of different currents of air. The southern current rarely, if ever, reaches the northern lakes, and the northern, until lately, never reached the Gulf of Mexico. But as the country is cleared of its native forests, we may reasonably conclude this cold current of air will prevail more and more, until we shall have snow enough for sleighs, at least two months in every winter; the summers will be shorter, the extremes of heat and cold will be greater than at present, and those clouds which formerly obscured the sun almost continually during the summer months, will be chased away, and with them the pale cheek, the sallow hue, the oppression at the breast, and the difficulty of respiration, the headache, and the thousand ills which many of the first emigrants have experienced in our climate. We shall probably then have fewer diseases, and more acute ones. The storms will probably be fewer, more severe, and not continue as long as at present. There are still further views which might be taken of this subject, but they are left to abler pens and future observations.

Thus I have endeavoured to give my opinion on a subject of some interest to the present, as well as future generations; in doing which, I have not sought for flowers which might have been gathered by stepping out of my path, but the *fruit* rather of my own observatiou and experience: I have not wandered

through the fields of imagination, invoking the poetic muse, but addressed myself chiefly

> "To him who soars on golden wing,
> Guiding his fiery-wheeled throne,
> The cherub contemplation."

ART. XIII. *On a singular Disruption of the Ground, apparently by Frost, in Letters from* EDWARD HITCHCOCK, A.M. *Principal of Deerfield Academy.*

(With a Plate.)

To the Editor of the American Journal of Science, &c.

SIR,

I HAVE lately examined a singular disruption in the earth, discovered a few days since in the northerly part of an extensive meadow in this town, about ten rods from Deerfield river.

The soil on the spot is alluvial, consisting of a dry, rich, vegetable mould, with a large intermixture of sand; and the field, elevated 14 feet above the bed of the river, is annually mowed. A valley encircles the ruptured spot on the east, south, and west, only five feet lower, yet so marshy and soft, as to render draining necessary to make it passable; and immediately back of this valley, on the south, rises a hill 100 feet high, at whose foot are several springs. North of the rupture, also, between it and the river, is a gradual descent of three feet: indeed, the ground slopes from it on every side except the northwest.

A fissure one inch wide and fourteen deep, forming an almost perfect ellipsis, whose diameters are 9 and 5½ rods, marks the exterior limit of the convulsion. Within this curve are several others nearly concentric to it, some forming a quarter, and some half an ellipsis, and near the longer axis are others, running in various directions. On this transverse diameter, which lies

near the highest part of the swell above described, and in its longest direction, or parallel to the river, the greatest effect of the convulsion appears. The earth, to the depth it has frozen the past winter, 14 inches, is broken on a straight line above 6 rods, and the south edge of the fissure, having been forced up, overlaps the other, three feet. Where one edge does not thus overreach, the tables of earth, which at a small distance resemble masses of ice, are raised up so that their faces form an isosceles triangle, leaving a cavity beneath. About the extremities of the transverse axis, is also an overlapping of two feet, which continues nearly two rods on the curve each way from the axis, and in most places is double, overreaching internally and externally, exhibiting likewise, some irregularity where the compressing forces acted at right angles to each other. The edges of these elevated masses of earth, which are yet frozen, are quite smooth, and the angles but little fractured. I have dug into the earth about four feet underneath the longer axis of the ellipsis, and thrust down a bar in other places, but cannot perceive that the soil has been moved below where it was frozen. It is, however, not the most favourable season for ascertaining this fact.

Every appearance on the spot will justify this conclusion, that the frozen surface of the earth around, has pressed with great force *from every direction* to this ellipsis as a centre; for, were every fissure in the ellipsis to be filled by replacing the earth, there must remain on its longer axis and at the extremities of this, an overplus of surface two feet wide.

The month of February last has been unusually cold. Its mean temperature in Deerfield, by Fahrenheit's scales, is as follows.

7*h.* A. M.	1½*h.* P. M.	10*h.* P. M.
6°	24°	11°

The extremes were 25° below, and 49° above zero. On the last day but one of the month, the cold suddenly relaxed; and on the 1st and 2d of March, a heavy and warm rain succeeded. This produced an uncommon rise in Deerfield river,

and on the 3d of March, it had overflowed the ground where the above described phenomenon occurred, and did not recede from it for 24 hours. Its greatest depth there, was five feet. The snow was nearly one foot deep when the flood happened, and being a nonconductor of heat, the temperature of the surface of the ground was not probably much changed from its state in February, until the water came in contact with it. It may not be amiss to give the state of the thermometer on the last of February and beginning of March.

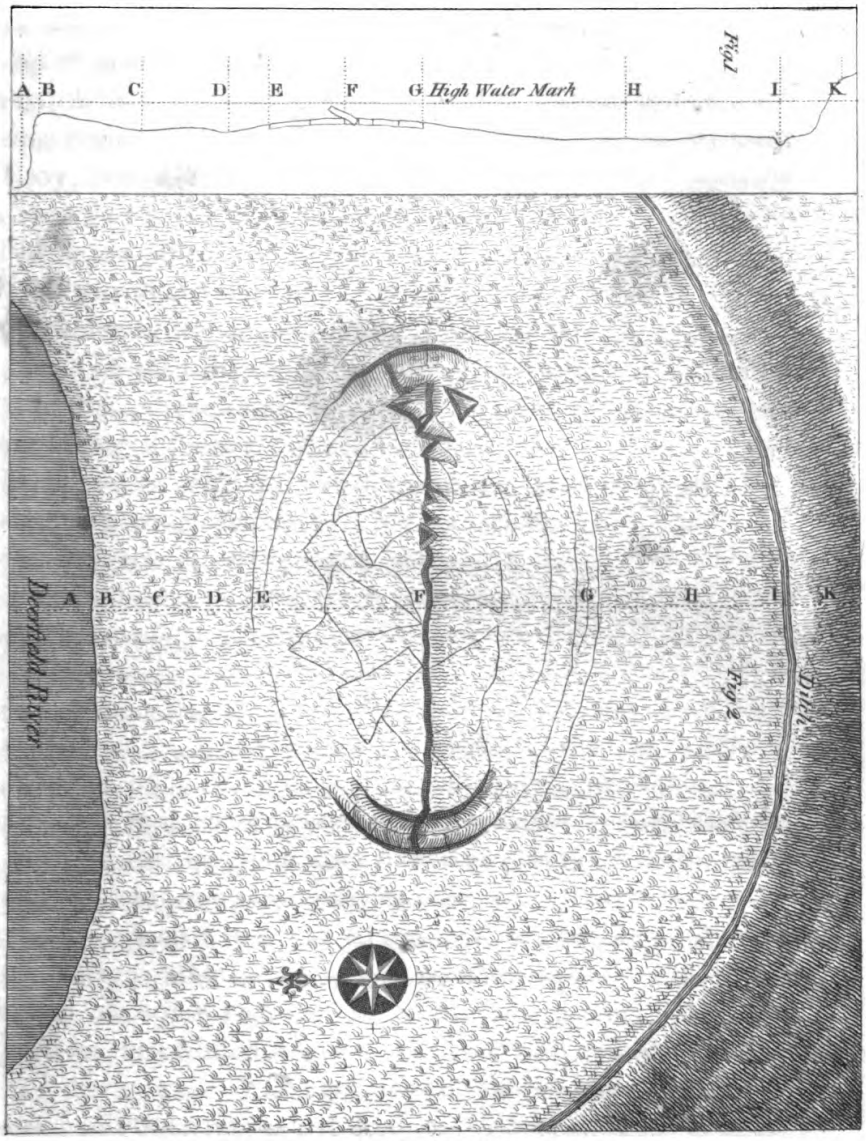
	7*h*. A. M.	1½*h*. P. M.	10*h*. P. M.	Wind, weather, &c.
Feb. 27th,	15° below 0.	28° above 0.	32° above 0.	South, clear.
28th,	31 above	45 ——	31 ——	do. do.
March 1st,	29 ——	46 ——	37 ——	N. E. rain.
2d,	46 ——	49 ——	37 ——	do. do.
3d,	30 ——	35 ——	29 ——	do. rain & clear.

On the third of March, about sunset, some lads were sailing near the spot where the disruption appears, and saw the water in considerable agitation, with much bubbling, and at short intervals it was thrown up in several places to the height of 3 or 4 feet. They saw no rupture in the earth, although they came within two or three rods of the spot, and state the water to have been two feet deep. About one o'clock on the morning of March 4th, Mr. Seth Sheldon and family, living one mile south from this spot, and being awake, were alarmed by a loud report from the north, by which their house and furniture were much shaken. They compared the sound, though louder by far than they had ever heard from this cause, to that of a cracking in the earth by frost in severe weather. Some others living rather nearer the spot, were awakened by the same report. That the rupture in the earth was made at that time is probable, though not certain.

It may be proper to state, that during the flood, no ice, except a few loose masses, was carried over, or near the spot where the disruption appears. This, therefore, could not have produced it.

Fig. 1. is a transverse section, taken with a theodolite, from Deerfield river 28 rods south, crossing the longer axis of the

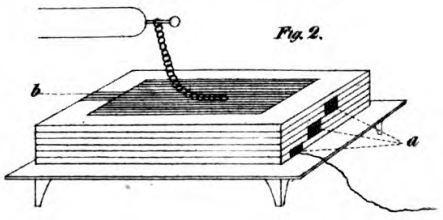
Disruption of the Ground by Frost.

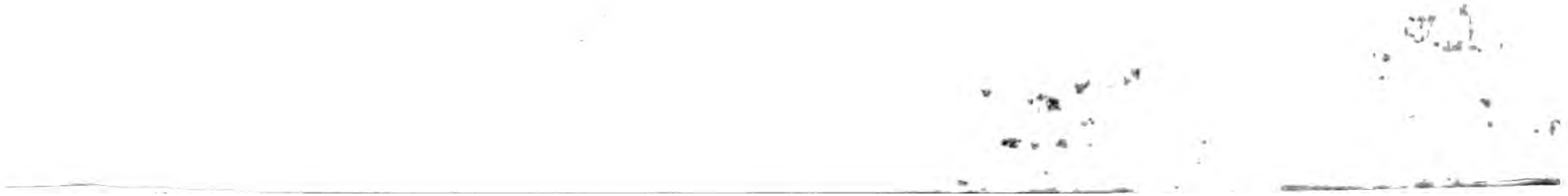


N.L.S.S. Jocelyn Sc. NH.

Dr. J.F. Dana's Electrical Battery.







disruption at right angles. The scale is 4 rods to an inch, although in laying off the heights and levels, the exact proportion was a little varied, to render the irregularities of surface more distinct. The letters of reference correspond to those on fig. 2, and need no explanation.

Fig. 2. is a bird's-eye view of the disruption and the adjoining region, very obligingly sketched by Mr. Derick Barnard of Troy, New-York. The surrounding country is somewhat contracted to bring more of it into view.

These are all the facts I am able at present to collect concerning this phenomenon. I have been particular as to the temperature of the air, and the situation of the adjacent country, from an idea that frost was a principal agent in producing it; and that, therefore, these circumstances would be important in fixing a theory. I will not, however, hazard any hypothesis on the subject; but if you deem the fact of sufficient importance, your opinion, Sir, is respectfully solicited.

Your humble Servant,

EDWARD HITCHCOCK.

Deerfield, Mass. March 26th, 1818.

Deerfield, June 3d, 1818.

SIR,

SINCE I sent you a description of a singular disruption in the earth in this town, another has been observed in the same meadows, about one mile from the former. This is less than the one of which I sent you an account, but its situation is almost exactly similar; it being on a small elevation, on the sides of which, at a few rods distant, is low wet ground. Indeed, the *general* description which I sent you will answer for this smaller disruption. The diameters of this last, are only 7 and 8 paces, and the curve is not perfect. There appears to have been an expansion of the earth's surface around both these spots, or disruptions, by which it was forced to give way at the point where there was the least resistance, which, of course, would be on the highest ground. The more I observe of this phenomenon, the more I am inclined to impute it to the agency of frost.

It may be proper to observe, that in neither of these disruptions has the general mass of the hills sunk in the least. Had this been the case, it might perhaps have accounted for them. It is also certain, that the soil below where it was frozen the past winter, has not been moved. I mentioned this fact in my first communication, though with some suggested doubt.

REMARKS.

An opinion having been requested by Mr. Hitchcock on the above facts, it may be observed, that there appears in the statement sufficient evidence that the phenomenon (as the author has suggested) is attributable to frost.

It is a fact, established equally by common experience and by numerous experiments, that water, in freezing, expands. It is generally estimated that 8 cubic inches of water, become 9 by the act of congealing. The expansion is attributed, with sufficient evidence, to a crystalline arrangement arising from a kind of polarity in the particles of water exerted when they are near congealing, by which they attract one another in certain points, and not in others. Dr. Black, with his usual felicity, has illustrated this tendency, by supposing a great number of small magnetized needles, thrust through corks, so that they will float parallel to the surface of water, to be thrown promiscuously into a vessel of that fluid. They will not remain in the situation in which they are thrown in, but, in consequence of their polarity, attractions and repulsions will be immediately exerted; they will rush together, with a force equal to the overcoming of a certain resistance; they will arrange themselves in pairs and groups, and finally, in a connected assemblage.

The particles of water attract each other with a prodigious force, when resistance is opposed; for it is well known that domestic utensils, trees, rocks, and even cannon, and bomb-shells, are burst with explosion, when water confined within them is frozen.

There is force enough then exerted by the expansion of freezing water, to produce all the mechanical violence, whose effects were so striking in the instance at Deerfield.

In the common cracking of the ground by frost, so extensively observed in cold climates, the effect appears to result in

the following manner. The water contained in the ground, (that is, in that part which is within the reach of a freezing temperature) by congealing, expands and demands more space; a movement must necessarily take place in the direction where there is the least resistance; this will evidently be upward, because the atmosphere, the only counteracting power in this direction, cannot resist the expansion of the freezing water as much as it is resisted by the earth below the freezing stratum. Consequently, the freezing earth is forced upward, but being of unequal strength in different places, it cracks at the weakest spot; and the earth, for some distance on the sides of the fissure, is thrown into the position of two planes gently inclined, their relative position resembling that of a very flat roof, and the more they are lifted by frost, the more they will decline from one another, and the wider will be the fissure.

But why, in the instance which Mr. Hitchcock has related, did they overlap? The explanation appears to result from the circumstances of the case, as far as they can be understood without ocular inspection of the ground.

The elevated spot which cracked in so remarkable a manner, being nearly surrounded by a *belt* of low wet ground, the congelation of the water in this ground by the intense cold, would of course produce a very great expansive effort towards the elevated ground. This, not only on account of its elevation, but from its containing less water, would not be able to exert an equal counteracting effort. The surface of the ground, therefore, (without at all disturbing the unfrozen earth below,) was, by the expansive effort of the freezing water, *pushed along* towards the elevated spot. This spot being possessed of a certain power of resistance derived from its gravity, and from the freezing of the water in it, would not immediately give way; but the whole surface, it is probable, gradually rose for some time, while the expansion was going on and increasing. A cavity would thus be produced between that superficial layer of frozen ground which was rising, and the unfrozen ground below. This cavity would of course be filled with air derived from the atmosphere, and from the porousness of the ground below. When the place came to be overflowed, water

would immediately rush in through any fissure, and this hydraulic and hydrostatic effort would force the air out at any orifice, and thus blow the water up with it. This was probably the cause of the agitation of the water, and of the bubbling of air, and of the throwing up of the water at intervals, observed by the boys on the 3d of March.

The effect of the water covering the ground, would be to weaken its cohesion derived from frost, and as there were probably hundreds of tons of pressure, the vaulted ground, when sufficiently weakened, gave way with a loud explosion and a violent concussion, as heard by Mr. Sheldon's family, a few hours after the facts observed by the boys. The parts of the arch now fallen in, (so to speak) necessarily either overlapped, or rose in ridges, piece being pressed against piece, as described and figured by Mr. Hitchcock.

We are indebted to this gentleman for his delineation of this singular case.

The freezing of water, and its attendant expansion, are productive of multiplied and very diversified phenomena upon our globe, whether we contemplate them in the delicate spiculæ of hoarfrost, the six-rayed stars of snow, or in the stupendous glaciers of the Alps, and the awful icebergs of Greenland.

Cambridge, January 25, 1819.

PROFESSOR SILLIMAN.

Dear Sir,

IF the following observations are worthy of a place in your valuable Journal, please to insert them, and oblige yours, with real esteem, J. F. DANA.

ART. XIV. *On a New Form of the Electrical Battery, by* J. F. DANA, M. D. *Chemical Assistant in Harvard University, and Lecturer on Chemistry and Pharmacy in Dartmouth College.*

THE Electrical Battery in its common form is an unmanageable and inconvenient apparatus. When the coated surface is

comparatively small, the instrument occupies a large space, and it cannot be readily removed from place to place without much trouble and risk; the apparatus is, moreover, very expensive, and when one of the jars is broken, another of the same dimensions cannot readily be found to supply its place.

It occurred to me, that a Battery might be constructed of plates of glass and sheets of tinfoil, in which the same extent of coated surface should occupy a much smaller space, and consequently that the apparatus would be more convenient and more portable. I selected several panes of glass, the surfaces of which coincided closely with each other, and then arranged them with sheets of tinfoil in this order, viz. pane of glass, sheet of tinfoil, then another pane of glass, then a second sheet of tinfoil, and so on; the sheets of foil being smaller than the plates of glass by two inches all around; the glass being 10 by 12, and the foil 6 by 8. This apparatus contained six plates of tinfoil, and the lowest plate being numbered *one*, was connected with the ground, and by slips of tinfoil passing over the edges, with the *third* plate, and this, in like manner with the *fifth*. The *second* plate was connected with the *fourth*, and this with the *sixth*, which communicated with the conductor of the machine; in this manner each plate positively electrified will be opposed by one negatively electrified, and vice versa; the 6th, 4th, and 2d plates positive, and the 5th, 3d, and 1st, negative. Into this apparatus I could introduce a powerful charge, but not possessing a battery of the common form, could not make comparative experiments. The annexed figures will explain the construction of this apparatus.

(See Plate.)

Fig. 1. *a* 1, *a* 2, &c. the tinfoil.
b b b, plates of glass.
c, the intermediate slips connecting the plates 6, 4, and 2.
d, the slips connecting 5, 3, 1, and the ground.

Fig. 2. *a*, the intermediate slips passing over the edges of the glass and connecting plates, 1, 3, and 5.
b, the slip which connects the upper sheet of foil with the 4th, &c.

In a battery of the ordinary form, it is evident that a much less surface is coated than in one of the above construction;

in a battery of the common form, two feet long, one foot wide, and ten inches high, and containing 18 coated jars, there will be no more than 3500 square inches of coated surface, while in a battery of the same dimensions on the proposed construction, there will be no less than 8000 square inches covered with tinfoil, allowing the sheet of glass and of foil to be $\frac{1}{4}$ inch thick.

When plate glass is employed for making this battery, the ring of glass exterior to the tinfoil may be covered with varnish, and then the next plate laid over it; the tinfoil will then be shut out for ever from the access of moisture, and the insulation will remain perfect. This form of the Electrical Battery is very portable, may be packed in a case with the machine, and indeed a powerful battery occupies no greater space than a quarto volume. It is cheap and easily constructed.

ART. XV. *Chemical Examination of the Berries of the Myrica Cerifera, or Wax Myrtle, by* J. F. DANA, *M. D. Chemical Assistant in Harvard University, and Lecturer on Chemistry and Pharmacy in Dartmouth College.*

(Communicated for this Journal.)

THE myrtle wax of commerce has been examined by Dr. Bostock and by M. Cadet; the entire berry not having been made the subject of analysis, I have been induced to examine it, with a view to ascertain the proportion of wax.

I. Fifty grains of the most perfect berries were digested in repeated portions of warm alcohol, until the fluid appeared to exert no further action. The first portions of alcohol were tinged of a green colour, but the last portions remained colourless.

II. The alcoholic solutions were poured into a small retort of known weight; the alcohol was carefully distilled off, and the residuum dried; deducting the weight of the retort, there remained 18.5 grs. for the weight of the matter dissolved by the alcohol.

III. The substances which had been dissolved by the alcohol consisted of two portions, viz. the wax, which was of an apple-green colour, and a reddish brown substance; this substance was supposed to be resinous, and the contents of the retort were therefore digested in acetic acid; the acid soon became of a reddish brown colour, and dissolved nearly the whole of the matter in the retort, leaving the wax. The acid solution, together with a small portion of insoluble reddish matter, were carefully separated from the wax. The wax being dried and melted, weighed 16 grains.

IV. The acetic acid solution was evaporated to dryness, and a dark brown matter was obtained; it was almost totally soluble in warm alcohol, from which it was precipitated by water; it was supposed therefore to consist chiefly of resin, with a small portion of extractive matter, and may be called resino-extractive; it weighed 2.5 grains.

V. The matter insoluble in alcohol consisted of two parts, viz. the kernels and a fine-grained black powder, having very much the appearance of fine gunpowder; the powder was carefully separated from the kernels by a wire sieve, and weighed 7.5 grains. The kernels were found to weigh 23.75 grains.

From this analysis it appears that the entire berries consist of

Wax	32.00
Resino-extractive . . .	5.00
Black powder	15.00
Kernels	47.00
	99.50
Loss	.50
	100.00

The chemical properties of the wax and of the black powder may be made the subject of another communication.

Earthy phosphate of iron has recently been found at Hopkinton, Mass. It exists there in large quantities, and is employed as a pigment. The gentleman on whose grounds it was found sent me several pounds of it.

J. F. D.

Art. XVI. *Analysis of Wacke, by Dr.* J. W. Webster, *of Boston.*

One hundred parts exposed to a red heat in a platina crucible lost 18.5, acquired an umber brown colour, and a degree of hardness sufficient to scratch glass.

One hundred parts reduced to fine powder were mixed with four times the weight of soda, and exposed to heat, gradually increased for three quarters of an hour; at the expiration of which time, the whole had acquired a pasty consistence. The crucible was now removed from the fire, its outer surface carefully wiped. Muriatic acid was poured on till all effervescence ceased. The solution obtained was evaporated to dryness, gradually assuming an orange red colour. Water was now poured upon the mass, after which it was filtered, and the powder remaining carefully dried; after ignition, and while warm, it weighed 28 parts. This powder was insoluble in muriatic acid, and of a white colour.

To the filtered solution, reduced by evaporation, carbonate of potash was added, the precipitate was collected on a filter, washed and dried; it weighed 23. parts. This powder was redissolved in sulphuric acid, sulphate of potash added, and crystals of alum finally obtained; hence this powder was alumine. To the liquor from which the silex and alumine had thus been separated, acetic acid was added; the whole evaporated to dryness; the excess of acid being removed, a small quantity of water was poured on, and after strong ignition, the precipitate weighed 4.5.

Into a very small tubulated retort I introduced a portion from the same mass, whence the piece submitted to analysis

was broken, and obtained over mercury the carbonic acid in the usual manner. This was equal to 2.32; by deducting this from 18.5 the loss during exposure to red heat, we shall have 16.18, the proportion of water. The oxide of iron was separated from the solutions after the addition of acetic acid, by ammonia, and weighed 26 parts.

Silex	28.
Alumine	23.
Lime	4.5
Carbonic acid . .	2.32
Water	16.18
Oxide of iron . .	26.
	100

AGRICULTURE AND ECONOMICS.

ART. XVII. *On the Comparative Quantity of Nutritious Matter which may be obtained from an Acre of Land when cultivated with Potatoes or Wheat, by Dr.* ELI IVES, *Professor of Materia Medica and Botany in Yale College.*

IN a good season an acre of suitable land well cultivated will produce 400 bushels of potatoes. In Woodbridge, a town adjoining New-Haven, a crop of 600 bushels of potatoes has been obtained from a single acre. A bushel of potatoes weighs 56 pounds. Multiply 400, the number of bushels, by 56, the weight of a single bushel, gives 22400, the number of pounds of potatoes produced upon one acre.

Thirty bushels of wheat are considered a good crop as the product of one acre of land. About $\frac{5}{6}$ of wheat may be considered as nutritious matter.

According to the experiments of Dr. Pearson and Einhoff, about one-third of the potato is nutritious matter. From the

analysis of Einhoff, 7680 parts of potatoes afforded 1153 parts of starch—fibrous matter analogous to starch 540 parts—albumen 107 parts—mucilage 312 parts. The sum of these products amounts to about one-third of the potatoes subject to the experiment.

Sir Humphry Davy observes, that one-fourth of the weight of potatoes at least may be considered nutritious matter.

One-fourth of 22400, the product of an acre of ground, cultivated with potatoes, is 5600. The whole weight of a crop of wheat calculated at 30 bushels to the acre, and at 60 pounds to the bushel, gives 1800. Deducting one-sixth from the wheat as matter not nutritious, and the weight is reduced to 1500.

The nutritious matter of the crop of potatoes to that of wheat is as 5600 to 1500, or as 56 to 15.

The starch might be obtained by a very simple machine, recommended by Parmentier; and in seasons when potatoes are abundant, the potatoes might be converted to starch, and the starch preserved for any length of time, and used as a substitute for wheaten flour.

The machine alluded to is a cylinder of wood about three feet long and six inches in diameter, covered with sheet tin, punched outward so as to form a coarse grater, and turned by a crank. This cylinder is placed in a box of boards whose sides slope a little inward upon the principle of a hopper, and a tub of water is placed beneath. The potatoes are thrown into this box, and as the crank is turned they are crushed, and the starch or fecula subsides to the bottom of the water. It is well known, that potatoes are largely used in England mixed with flour to form a very good bread; the *starch* of the potato would of course answer much better.



ARCHIBALD BRUCE M.D.

MISCELLANEOUS.

ART. XVIII. *Biographical Notice of the late* ARCHIBALD BRUCE, M. D. *Professor of Materia Medica and Mineralogy in the Medical Institution of the State of New-York, and Queen's College, New-Jersey; and Member of various Learned Societies in America and Europe.* With a Portrait.

(Communicated.)

DOCTOR Archibald Bruce, (the subject of this Memoir) was a native of the city of New-York, in North America. He was born in the month of February, in the year seventeen hundred and seventy-seven. His father was, at that time, at the head of the medical department of the British army, (then stationed at New-York) to which he had been attached from his youth, having been many years previously resident at New-York, as surgeon to the artillery department; where he was married, in or about the year seventeen hundred and sixty-seven, to Judith, a daughter of Nicholas Bayard, formerly of the same city, at that time the widow of Jeremiah Van Rensselaer of Greenbush; by whom he had another son, (who died an officer in the British army in Ireland) and a daughter, who died while a child.

William Bruce, (the father above-mentioned,) and his brother Archibald, together with a sister, were natives of the town of Dumfries in Scotland, where their father was many years resident as the parochial clergyman; and so continued until his decease, much respected.

Both sons applied themselves to the science of medicine and surgery. William, as above stated, became a physician in the British army, and died, in that station, of the yellow fever, in the island of Barbadoes. And Archibald received a commission of surgeon in the British navy, in which he continued until

were eminently active, and by their united exertion and perseverance, (opposed by much professional talent) they obtained a charter from the regents. In this new institution, as professor of the materia medica, and of his favourite pursuit, mineralogy, he exhibited the fruits of arduous study, with a dignity of character, and urbanity of manner, which commanded the respect of the profession, and the regard of the students.

The ruling passion in Dr. Bruce's mind, was a love of natural science, and especially of mineralogy. Towards the study of this science, he produced in his own country a strong impulse, and he gave it no small degree of eclat. His cabinet, composed of very select and well characterized specimens; purchased by himself, or collected in his own pedestrian and other tours in Europe, or, in many instances, presented to him by distinguished mineralogists abroad; and both in its extent, and in relation to the then state of this country, very valuable, soon became an object of much attention. That of the late B. D. Perkins, which, at about the same time, had been formed by Mr. Perkins in Europe, and imported by him into this country, was also placed in New-York, and both cabinets (for both were freely shown to the curious, by their liberal and courteous proprietors) contributed more than any causes had ever done before, to excite in the public mind an active interest in the science of mineralogy.*

Dr. Bruce, while abroad, had been personally and intimately conversant with the Hon. Mr. Greville, of Paddington Green, near London, a descendant of the noble house of Warwick, the possessor of one of the finest private cabinets in Europe, and a zealous cultivator of mineralogy. Count Bournon, one of those loyal French exiles, who found a home in England, during the storm of the French revolution, was almost domesticated at Mr. Greville's, and was hardly second to any man in mineralogical, and particularly in crystallographical knowledge.

* The collection of Mr. Perkins became, in 1807, (partly by the liberality of its possessor, and partly by purchase,) the property of Yale College, and is now in the cabinet of that institution. It is believed that few cabinets of equal extent, ever contained more instructive and beautiful specimens, with less that is unmeaning or superfluous. The cabinet of Dr. Bruce has, since his death, been purchased by a gentleman in New-York, for 5000 dollars. *Editor.*

His connexions with men of science on the continent, were of the first order, and to be familiar at Mr. Greville's, and with Count Bournon, was to have access to every thing connected with science in England and France. Dr. Bruce was also at home at Sir Joseph Banks's, the common resort of learned and illustrious men. Thus he enjoyed every advantage in England, and when he went to the continent, the abundant means of introduction which he possessed, brought him into contact with the distinguished men of Paris, and of other cities which he visited. The learned and estimable Abbé Haüy was among his personal friends and correspondents; and many others might be mentioned in the same character, whose names are among the first in the ranks of science, in various countries of Europe.

Returned to his own country, after being so long familiar with the fine collections in natural history, and especially in mineralogy, in various countries in Europe, Dr. Bruce manifested a strong desire to aid in bringing to light the neglected mineral treasures of the United States. He soon became a focus of information on these subjects. Specimens were sent to him from many and distant parts of the country, both as donations and for his opinion respecting their nature. In relation to mineralogy he conversed, he corresponded extensively, both with Europe and America; he performed mineralogical tours; he kindly sought out and encouraged the young mineralogists of his own country, and often expressed a wish to see a journal of American mineralogy upon the plan of that of the School of Mines at Paris. This object, it is well known, he accomplished, and in 1810, published the first number of this work. Owing to extraneous causes, it was never carried beyond one volume; but it demonstrated the possibility of sustaining such a work in the United States, and will always be mentioned in the history of American science, as the earliest original purely scientific journal of America.

Dr. Bruce had, in a high degree, the feelings of a man of science. He was ever forward to promote its interests, and both at home and abroad, was considered as one of its most distinguished American friends.

Many strangers of distinction came introduced to him, and his urbanity and hospitality rarely left him without guests at his board. During the latter part of his life, he seems to have been less interested in science. His journal had been so long suspended, that it was considered as virtually relinquished; his health was undermined by repeated attacks of illness, and science and society had to lament his sudden departure, when he had scarcely attained the meridian of life.

He died in his native place on the 22d of February, 1818, of an apoplexy, in the 41st year of his age.

INTELLIGENCE.

Art. XIX. 1. *Dr.* J. W. Webster's *Lectures.*

Dr. J. W. Webster, some months since, commenced a course of Lectures in the town of Boston, on Geology and Mineralogy. Having finished his first course, he is now occupied with a second on the same subjects, and we understand receives the patronage of some of the most respectable citizens of Boston and its vicinity. He makes Geology the groundwork of his plan, and fills up by describing the metals and minerals met with in each class of rocks, after the rock has been noticed. A pretty full account is given of the coal formations, (several of which Dr. W. has visited) and of the modes of searching and boring. A view is given of the formations of Paris and the Isle of Wight, with specimens from those districts.

In the volcanic part, a description (from personal observation) is given of St. Michael's. The structure of veins; the forming and destroying effects of water; the physiognomy of the dry land and submarine; the origin of islands and coral reefs, and a view of the principal mountain ranges throughout the world conclude the course.

2. *Dr. Webster's Cabinet.*

Dr. Webster, having spent two or three years in Europe, in professional studies, during which time he devoted much attention to mineralogy and geology, with the ample aids afforded by the cabinets and distinguished teachers in Scotland, France, and England, has recently returned to his own country, and has brought with him a very select and considerably extensive cabinet of minerals, with which, and with American specimens, he illustrates his lectures. We understand that the collection contains some thousand specimens, and is good in the English and Scotch minerals; also in the Siberian coppers; it contains a suite of three hundred geological specimens from Freyberg, from granite to gravel. The geological part is extensive, and was increased by numerous pedestrian tours in England and Scotland; most of the geological specimens have been examined, in company with Professor Jameson. The volcanic part is good, from the extensive opportunities which Dr. Webster enjoyed in the Azores, in which, on his return to this country, he spent some time, and found much to interest him. His observations will soon be given to the public, in a work entitled *Remarks on the Azores or Western Islands.*

It is well known that they are volcanic, and of course afford the usual volcanic substances. The most interesting part is that occupied by the boiling fountains, in many respects similar to the Geysers of Iceland, excepting that the water is not ejected to any considerable height; but the incrustations, the sinter, and sulphur, are every way equal to any specimens which Dr. Webster saw in Sir G. Mackenzie's collection.

We are much gratified in noticing both what Dr. Webster has done and is still doing. We are persuaded that he will do much towards promoting the cultivation of American mineralogy and geology, and especially in the enlightened community in which he resides.

We cordially wish him success, and trust that it will be ensured by the patronage of the citizens of Boston.

3. *Supposed identity of Copal and Amber.*

A correspondent, whose paper is withheld from publication till some additional experiments can be made, conceives that copal and amber are originally the same substance, and the product of the same tree.

4. The Negronite.—(*A supposed new mineral.*)

Extract of a letter from Dr. H. H. Hayden of Baltimore, to the Editor, dated January 5, 1819.

" It (the necronite) occurs in a primitive marble, or limestone, which is obtained 21 miles from Baltimore, and a small distance from the York and Lancaster road. It was first noticed by myself at Washington's monument, in which this marble is principally employed.

" It occurs, for the most part, in isolated masses in the blocks, or slabs, both in an amorphous and crystallized state. It is most commonly associated with a beautiful brown mica, of the colour of titanium; small but regular crystals of sulphuret of iron, tremolite, and small prismatic crystals of titanium, which are rare. The form of the crystals is a rhomboid, approximating very much to that of the felspar, and which has inclined some to consider it as such. Also, the hexaedral prism, resembling that of the beryl. This form is rare, and has not, as yet, I believe, been found complete. Its colour is a bluish white, and clear white. Its structure much resembles felspar, being lamellar; sometimes opaque, semi-transparent and transparent, at least in moderately thin pieces. It scratches glass, carbonat of lime, and even felspar, in a *slight* degree. In all our efforts, it has been found infusible, per se, or with borate of soda, and even from all the force of heat that could be excited in a smith's furnace, it came out unchanged in any degree. The acids seem to have no sensible effect upon it, either cold or hot. This is all that I can say of it at present, except that it possesses a most *horrid* smell.*

* On account of its peculiar cadaverous odour Dr. Hayden proposes to call this mineral (should it prove to be a new one) Necronite, from the Greek Νεκρος.

I have since found in a marble of the same kind, but from a different quarry, and a few miles distant from the first, a quartz almost as fetid as the necronite, and likewise associated with *small* prisms of titanium.

" These substances carry with them a degree of interest in another point of view. They seem to invalidate the opinion that the fetid smell of secondary limestone, slate, &c. is derived from the decomposition of animal matter. As their gangue is *decidedly* a rock of primitive formation."

Another new mineral observed by Dr. Hayden.

" Exclusive of the interest which the necronite has excited with me and several others, I have besides stumbled upon another substance, if possible still more interesting. I discovered it in an imperfect state, about 4 years since, but not until recently have I been able to find it perfect, in beautiful garnet coloured cubic crystals $\frac{1}{4}$ of an inch square or nearly. These crystals are very liable or subject to decomposition, in which state they present a perfect but spongy cube. Although they resemble the cubic zeolite, yet they have nothing of its character with them besides."

Remark.

Dr. Hayden without doubt alludes to the *chabasie* of the Abbé Haüy, formerly but inaccurately called the cubic zeolite; for it is really a rhomboid very nearly approaching a cube—its angles being 93° 48′, and 86° 12′.

5. Preservation of Dead Bodies.

From Thenard's Chemistry, vol. iii. *Paris ed. p.* 713.

The author declines describing the methods of embalming commonly employed, and proceeds to describe the mode which was for the first time employed by Dr. Chaussier.

" This process consists in placing the dead body thoroughly emptied and washed, in water kept constantly saturated with corrosive sublimate. This salt gradually combines with the flesh, gives it firmness, renders it imputrescible, and incapable of being attacked by insects and worms.

"I have seen, (adds the author) a head thus prepared, which had been exposed alternately to the sun and rain during several years, without having suffered the slightest change. It was very little deformed, and easily recognized, although the flesh had become as hard as wood."

6. Matches kindling without fire.

(*From Thenard's Chemistry, Vol.* ii. *p.* 525.)

This match is prepared by mingling two parts of the oxymuriate of potash and one of sulphur, which by means of a little gum is attached to a common sulphur match. This match on being dipped into, or rather slightly wet with, strong sulphuric acid, (oil of vitriol) immediately catches fire.

The author has not added the caution that the sulphur and salt should be pulverized separately; if rubbed together in a mortar, they will explode with some danger to the operator, provided the quantity be over a few grains.

Matches made upon this principle, have been for sometime made and sold in this country. They are sometimes put up in little japanned cases with a small phial, from which when inverted with the mouth open, nothing will drop, and yet the match kindles on being thrust in quite to the bottom. The truth is, these bottles contain a little amianthus moistened with sulphuric acid, which thus kindles the match, but as the acid soon weakens by attracting water from the air, it is better to use a phial of the acid in the liquid state. A few drops answer the purpose, and when this is weakened, it is easily renewed.

7. *Cleaveland's Mineralogy.*

Our opinion of this work was fully expressed in the review of it in our first number. In the Edinburgh Review for September, 1818, this work is again reviewed, and in a manner which must gratify every friend to American science. It will be necessary to cite only a single sentence. After commending the condensed and *honest* manner in which the work is printed, (for they say, that the same matter which here fills one volume would in England have been spread over three,) the re-

viewer adds, " We should be glad to see it reprinted exactly upon the plan of the original; and we have no doubt that it would be found *the most useful work on mineralogy in our language.*" More need not be—more scarcely could be said.

8. *A new Alkali.*

A new alkali has recently been discovered in Sweden, by M. Arfwedson. It is found in the petalite, a mineral from Utoen, in Sweden, in a proportion not over 5 pr. ct.; also in the triphane or spodumene, in the proportion of 8 per cent. and in what is called crystallized lepidolite, in the proportion of 4 per cent. In its general properties it very nearly resembles the other alkalies. When heated in contact with platinum it acts on it. In the galvanic circuit it was decomposed " with bright scintillations, and the reduced metal being separated, afterward burnt." This metal resembles sodium. The new alkali has been called lithia. (*Jour. of Science of the Roy. Inst.*)

9. *Ignited Platinum Wire.*

In our last we mentioned the lamp without flame, the ignition of platinum wire being sustained by means of the vapour of alcohol.

Sir H. Davy has discovered that the vapour of camphor answers the same purpose: " If a piece of camphor, or a few small fragments in a heap, be placed in any convenient situation, as on a shilling, the bottom of a glass, &c. and a piece of platinum wire, either coiled or pressed up together, be heated and laid upon it, the platinum will glow as long as any camphor remains, and will frequently light it up into a flame."

Jour. Roy. Inst.

10. *Red Rain.*

A red rain fell in Naples, (March 14, 1818,) the common people were much alarmed, and called it *blood* or *fire.*

An earthy powder was collected, which when dry was yellow, unctuous, and of an earthy taste; its specific gravity 2.07.

Its analysis presented silex 33—alumine 15.5—chrome 1. iron 14.5—carbonic acid 9., and a combustible substance of a carbonaceous nature.

It is thought that this powder had not a volcanic origin, and that the presence of chrome assimilates it with meteoric stones. *Ibid.*

11. *Gnephalium.*

Professor Ives has discovered a new species of gnephalium with decurrent leaves, of which a plate and description will appear in our next number.

12. *Augite.*

M. Haüy has united the fassalite and the bakalite with the sahlite, a sub-species of augite. (See Mem. of the Museum of Nat. Hist. vol. 3.)

13. *A New Vegetable Alkali,*

Has been found by Messrs. Pelletier and Caventon in the Feve St. Ignace and the Nux Vomica. It has been named the vaucquelin, in honour of M. Vaucquelin. (Journal de Physique, for Aug. 1818.)

14. *New Minerals.*

Two new mineral species have been discovered, the scorrodite from Schnuburg in Saxony, and the tungstate of lead from Zinnwald in Bohemia. *Ibid.*

15. *New Metal.*

A new metal has been discovered by Berzilius, in the mines of Fahlun in Sweden, to which he has given the name of Selenium. *Ibid.*

16. *Pure Alumine.*

A large bed of this substance, perfectly pure, has been found at Argenton, Department de L'Endre. *Ibid.*

17. *Collections of American Minerals.*

We are informed that under the auspices of Col. Gibbs, a collection of American minerals by states, according to the arrangement of the minerals of the departments of France, in the cabinet of the school of mines at Paris, was begun some time since, at the rooms of the Hist. Society in New-York; and recently in the University of Cambridge. In the arrange-

ment of the latter, he has been assisted by Dr. J. W. Webster, lecturer on mineralogy and geology in Boston.

18. *C. S. Rafinesque, Esq.*

We are requested to announce that a Journal of this gentleman's "Travels and Discoveries in the West, will be published this year by Cramer and Spear of Pittsburg, and that the results of his zoological and botanical labours consist in the discovery of about 15 new genera, and 180 new species of plants; about 75 new genera, and 600 new species of animals, whereof nearly 70 are new fishes, 20 new quadrupeds, 30 new reptiles, 112 new shells, 250 new fossils, &c." "He has inquired how the deep valleys have been excavated, where lakes existed, where the old falls of the Ohio were, the extent and origin of the coal region, &c."

19. *Medical College of Ohio.*

Extract of a letter from Cincinnati, Jan. 10th, 1819.

The legislature of the state of Ohio have just established a medical college in this city, and have by an unanimous vote passed a law incorporating the Faculty. In the act, Dr. Samuel Brown of Alabama is named as Professor of Anatomy, Dr. Daniel Drake of Cincinnati, Professor of the Institutes and Practice of Medicine, Dr. Coleman Rogers, Professor of Surgery, and Dr. Slack, Professor of Chemistry. The other Professors are to be appointed by the Faculty, and it is believed that Dr. Richardson of Lexington, Kentucky, will be called to the Obstetrical chair. Very high expectations are entertained of the importance of this institution in the west.

20. *Notes on Ohio.*

Caleb Atwater, Esq. of Circleville, Ohio, has issued proposals for publishing the above work, (mentioned in our last number) with a prospectus exhibiting its principal features. We doubt not it will contain valuable information concerning a very interesting portion of the United States, and every effort on the part of men of intelligence and enlarged views, to make the western and southwestern states better known, deserves, and it is believed will receive, adequate support.

21. *Discovery of American Tungsten and Tellurium.*

Neither of these metals, so far as we are informed, has been announced as existing in either of the Americas. It is well known to mineralogists, that tungsten is very rare, and that tellurium is found only in Transylvania.

We have now the pleasure to state that both these metals exist in the Bismuth mine, in the town of Huntington, parish of New Stratford, in Connecticut, 20 miles west of New-Haven.

During the examination of some ores, brought to us by Mr. Ephraim Lane, the proprietor of this mine, we obtained the tungsten in the state of yellow oxid, and the tellurium in the metallic state.

The tungsten is stated to be abundant in the mine; it is the ferruginous species, known to mineralogists by the name of wolfram.

We cannot yet say whether the tellurium is abundant, having obtained it from only two pieces; from these we extracted also tungsten, so that it may possibly constitute a new mineral species. Further particulars will be given in our next Number.

22. *Mr. Sheldon's Application of Chesnut Wood to the Arts of Tanning and Dying.*

REMARKS.

A considerable time since, we were confidentially made acquainted with the discovery detailed in the following letter. We have repeated the most important of Mr. Sheldon's experiments, both in relation to tanning and dying, and are well satisfied that the discoverer has not overrated, or erroneously estimated, the value of his own results. We are persuaded that the highly *useful* arts alluded to, will derive important aid from the use of a material so abundant and cheap as chesnut wood.

To Professor Silliman.

Springfield, Mass. Feb. 27, 1819.

DEAR SIR,

I SEND you a more particular account of the newly discovered properties of the chesnut.

This tree, *Fagus Castanea*, Linn. is very abundant in New-England and the middle states ; and occurs in the mountainous districts, as far southward as South-Carolina, or perhaps even Georgia. It is one of the stateliest trees of the forest ; scarcely less distinguished by the beauty of its foliage, than by the durability of its wood.

By repeated analyses, conducted with the minutest attention to every circumstance which could ensure accuracy, it appears, incredible as it may seem, that the chesnut *wood* contains twice as much tannin as ross'd* *oak bark*, and six-sevenths as much colouring matter (which gives a black with iron,) as logwood. I am aware that nothing could be farther from the common apprehension than such results ; but the uniform success of a great variety of experiments in tanning and dying, in addition to the other kind of evidence, should satisfy the most incredulous.

The leather tanned with it, has, in every instance, been superior to that tanned in a comparative experiment, with oak bark ; being firmer, less porous, and at the same time more pliable. The reason for this difference, will probably be found in the *high state of oxygenizement* of the bark, particularly of the epidermis, by which it is rendered to a certain degree acrid and corrosive. Dr. Bancroft was perhaps the first who noticed the oxygenizement of barks. He attributes the dark brown colour of the epidermis of *his* quercitron, to this cause ; and as a confirmation of the idea, I have observed that ink made of the epidermis of another kind of bark, though at first not to be distinguished by the colour from that made of the cellular and cortical parts, is incomparably less permanent.

As a material for making ink, the wood of the chesnut is probably unrivalled. Combined with iron in any proportion, it gives, as it is dilute or concentrated, a pure blue or blue-black ; while galls, sumach, &c. &c. unless combined with a greater proportion than is consistent with the highest degree of permanency, afford a *black* more or less inclining to a red-

* That is, the inner bark deprived of the epidermis or outer bark, by the shaving knife.

dish brown. The lake of the chesnut is indeed a blue, and not to be distinguished by the eye from indigo; but when diffused on paper, this same substance becomes an intense shining black. In dying, little difference is observable between the chesnut and galls, and sumach, except that the former has a rather greater affinity for wool, &c. than the latter, and of course requires less boiling. Its permanency has been completely tested by long exposure to the sun and the weather; but no doubt can exist on this head, if the position of Berthollet be true, that permanent blacks are formed only by the combination of iron and tannin.

To prepare the chesnut wood for the purposes of tanning, a mode has been devised for reducing it to a suitable degree of fineness. This method consists in the application of knives, either in the direction of, or transversely to the grain, by a rotatory motion. This mode obviously involves the greatest possible economy of moving power. Messrs. B. and M. Stebbins, of West-Springfield, who are making arrangements for going largely into the exportation of the article, have in construction a machine upon this plan.

As might be expected, the inspissated aqueous extract of the chesnut, bears a near resemblance in many particulars, to catechu. Professor Dewey, of William's College, who at my request, has gone through an extensive and elaborate course of experiments, informed me that he obtained a quarter more of the gelatinous precipitate from the former, than from the latter. By the taste, the two substances are not to be distinguished, except that the former is more pungent. It leaves upon the tongue, the same permanent and refreshing sweetness, for which the other is so much prized in the east; where it is used as an article of luxury, with betel nut. Might not the extract be advantageously substituted for catechu, in the celebrated life preserving composition of Dr. Pearson; the object being to concentrate the greatest possible quantity of nutritious and tonic substances in the smallest weight.

The colouring properties of the two substances, are entirely different. After the discovery, twelve or fifteen years since, of the composition of the *terra japonica*, attempts were made in England to introduce it into the materia tingentia, as a substi-

tute for galls; but unfortunately, like the extract of quercitron, it affords with iron nothing but a meagre olive; and Dr. Bancroft states, that in a great number of trials, he was unable, by the greatest accumulation, to produce any thing like a black, even upon wool, much less upon cotton and silk.

A singular fact, which I observed in the course of my experiments, is worthy of notice. I had prepared for a certain purpose, solutions from the wood of the trunk of a tree, about three feet, and from that of a limb about three inches in diameter. The same quantity of wood and of the solvent was employed in both cases. On adding to each the same quantity of the solution of gelatine, abundant precipitates immediately appeared, as usual, apparently much the same in quantity; but to my astonishment, the size of the several congeries in each, bore a near proportion to that of the sticks from which they were obtained, not differing much from that of middling and of very small flakes of snow. Is not this an extraordinary fact, evincive of a complication in the arrangement of these bodies hitherto unsuspected. May it not at some future period, lead to a *nomenclature of precipitates;* affording, like the chrystallography of Haüy, a new and accurate mode of determining the compositions of substances; and perhaps throwing light upon the obscure subject of chemical, or if you please, electrochemical affinities. The *size* of a stick might probably be ascertained with almost as much precision, as by actual admeasurement. The solutions in this experiment, were formed by maceration in cold weather. When hot water was employed, and the process was completed in two or three hours, the appearance of the precipitate was very different, the congeries being smaller, irregular, and not well defined.

I have only to add, that having taken measures to secure the discovery, both in this country and Europe, it is my wish to bring it into general use as speedily as possible.

I am, Sir, very respectfully,
Your obedient servant,
WILLIAM SHELDON.

P. S. In a short article for some future number, I may send you an account of the operation of the machine, and of some other particulars.

23. *Additional note concerning the Tungsten and Tellurium.*

We have not room to insert in the present number, a description and a chemical examination of the ores of tungsten and tellurium recently discovered in Connecticut; they will appear in our next.

In the mean time it may be stated, that the tungsten and tellurium are found blended in the same pieces, but whether in mere mixture, or in chemical combination, is not yet quite determined. Many specimens of the tungsten exist without the tellurium, but every piece which has afforded tellurium has also afforded tungsten, and in greater abundance. Even in well defined crystals, both metals have been found in the same crystal, and where the external appearance was homogeneous. In other specimens a difference seems to be apparent, and a proper ore of tellurium appears to be blended with a proper ore of tungsten. This latter ore is the wolfram, composed of oxid of tungsten, or as some choose to say, tungstic oxid combined with iron and manganese. The crystals, however, are octahedral, a fact which we believe is not mentioned of this species by authors, although this form is found in the calcareous tungsten.

The Bismuth mine in which these ores are found is the property of Mr. Ephraim Lane. Letters addressed, post paid, to him at New Stratford, town of Huntington, Connecticut, will find him through the Post Office; and he will, for a reasonable compensation, pack boxes more or less extensive, for mineralogists and others. As Mr. Lane is by occupation a farmer, and is obliged to blast a quartz gangue in order to obtain his specimens, he cannot be expected to transmit them gratis. His mine, which has been sunk only ten feet, affords native bismuth, native silver, magnetical and common iron pyrytes, and copper pyrites, (the two latter crystallized) galena, blende, tungsten, tellurium, &c.

It is expected that the shaft will soon be sunk deeper, when probably a more abundant supply of good specimens will be obtained.

N. B. The silver and galena are the least abundant.

March 8th, 1819.

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THE

AMERICAN

JOURNAL OF SCIENCE, &c.

GEOLOGY, TOPOGRAPHY, AND MINERALOGY.

ART. I. *On the Geology, Mineralogy, Scenery, and Curiosities of Parts of Virginia, Tennessee, and of the Alabama and Mississippi Territories, &c. with Miscellaneous Remarks, &c. In a Letter to the Editor.* By the REV. ELIAS CORNELIUS.

(Concluded from page 226.)

I WILL conclude this part of the narrative with a brief notice of a few curiosities occurring in the region which has been described.

Caves.

1. It is well known that it furnishes a great number of interesting *caves*. They are found alike in the inclined and horizontal strata. Some of them are several miles in extent, and afford fine specimens of earthy and alkaline salts.

Wier's cave in Virginia has been described by Mr. Kain. I have in my possession a map of its most important apartments, including its whole length, copied from a survey made by Mr. J. Pack in Oct. 1806; also the notes of another survey made in May 1816, by the Rev. Conrad Speece of Augusta

county, and Mr. Robert Grattan; which, with an explanation, and particular description, I hope to be able to transmit to you at a future time.

From these surveys, it appears that the whole extent of the cave, hitherto discovered, does not exceed eight hundred yards. This was the length stated to me by the guide, when I visited it in August, 1817. I cannot but think there is some mistake in Mr. Kain's remark, that "it is a mile and a half in extent." I spent four hours in examining every accessible part, and by permission of Mr. Henry Bingham, the owner, made a large collection of specimens, which were transmitted for the Cabinet of Yale College.*

The Natural Bridge.

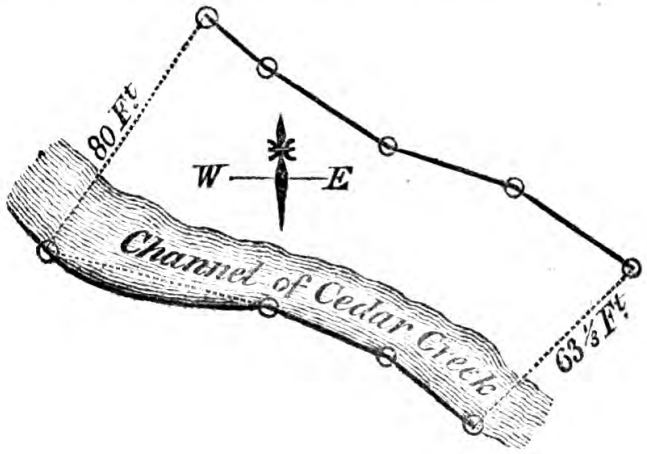
2. My object in naming this celebrated curiosity, is not to give a new description of it, but merely to furnish a correct account of its dimensions. I visited it in company with the Rev. Mr. Huson, who had previously found its height by a cord, to be two hundred and ten feet. We now found it by the quadrant, to be two hundred and eleven feet, and the arch through the centre about forty feet.

Some have attempted to account for this great curiosity, by supposing that a convulsion in nature may have rent the hill, in which it stands, asunder; thus forming the deep and narrow defile, over which the rocky strata were left, which constitute its magnificent arch. If so, the sides should have corresponding parts. At a distance from the base, no such correspondence is perceptible. At the base, the rocks are more or less craggy and irregular. This led me to take the courses and distances of each side. The following was the result.

Eastern side presents 4 angular points.				Western side presents 3 angular points.			
1.	N. 55°	W. 1 chain.	09 links.	1.	N. 50°	W. 0 chain.	45 links.
2.	N. 72	W. 1 ——	05½ ——	2.	N. 67	W. 1 ——	12½ ——
3.	N. 57	W. 1 ——	12½ ——	3.	N. 77	W. 1 ——	44 ——
4.	N. 50	W. 0 ——	33 ——				

* See Number 1. page 59.

The chain used contained 50 links, equal to 33 feet and $\frac{1}{3}$. The distance between the abutments at the north end of their bases, is 80 feet; at the south end, 66 feet. As they ascend, the distance is greater. These data give the following diagram.



Although considerable resemblance appears at the base, yet as no such correspondence is visible 40 feet above it, and the sides for the whole remaining distance to the arch, one hundred and thirty feet, lose their craggy appearance entirely, and present the smooth, irregular surface of the oldest rocks. I am led to think that the natural bridge is coeval with a very remote period of time. Nor is there any difficulty even in supposing it to have proceeded from the hand of the Almighty, as it is; for great and marvellous are all his works!

The following anecdote will evince the effect which the sight of the natural bridge produced on a servant, who, without having received any definite or adequate ideas of what he was to see, attended his master to this spot.

On the summit of the hill, or from the top of the Bridge, the view is not more awful than that which is seen from the brink of a hundred other precipices. The grand prospect is from below. To reach it you must descend the hill by a blind path, which winds through a thicket of trees, and terminates at the instant when the whole bridge with its broad sides and

lofty arch, all of solid rock, appears perfectly in sight. Not one in a thousand can forbear to make an involuntary pause: but the servant, who had hitherto followed his master, without meeting with any thing particularly to arrest his attention, had no sooner arrived at this point, and caught a glance of the object which burst upon his vision, than he fell upon his knees, fixed in wonder and admiration.

A River flowing from a Cave.

3. I will next mention *a singular cave*, which I do not remember ever to have seen described. It is situated in the Cherokee country, at Nicojack, the north-western angle in the map of Georgia, and is known by the name of the Nicojack cave. It is 20 miles S. W. of the Look-Out mountain, and half a mile from the south bank of the Tennessee River. The Rackoon mountain in which it is situated, here fronts to the northeast. Immense layers of horizontal limestone form a precipice of considerable height. In this precipice the cave commences; not however with an opening of a few feet, as is common; but with a mouth fifty feet high, and one hundred and sixty wide. Its roof is formed by a solid and regular layer of limestone, having no support but the sides of the cave, and as level as the floor of a house. The entrance is partly obstructed by piles of fallen rocks, which appear to have been dislodged by some great convulsion. From its entrance, the cave consists chiefly of one grand excavation through the rocks, preserving for a great distance the same dimensions as at its mouth.

What is more remarkable than all, it forms for the whole distance it has yet been explored, a walled and vaulted passage, for a stream of cool and limpid water, which, where it leaves the cave, is six feet deep and sixty feet wide. A few years since, Col. James Ore of Tennessee, commencing early in the morning, followed the course of this creek in a canoe, for three miles. He then came to a fall of water, and was obliged to return, without making any further discovery. Whether he penetrated three miles up the cave or not, it is a fact he did not return till the evening, having been busily engaged in his

subterranean voyage for twelve hours. He stated that the course of the cave after proceeding some way to the southwest, became south ; and southeast by south, the remaining distance.

Natural Nitre.

The sides of the principal excavation present a few apartments which are interesting, principally because they furnish large quantities of the earth from which the nitrate of potash is obtained. This is a circumstance very common to the caves of the western country. In that at Nicojack, it abounds, and is found covering the surfaces of fallen rocks, but in more abundance beneath them. There are two kinds, one is called the "clay dirt," the other the "black dirt ;" the last is much more strongly impregnated than the first. For several years there has been a considerable manufacture of saltpetre from this earth. The process is by lixiviation and crystallization, and is very simple. The earth is thrown into a hopper, and the fluid obtained, passed through another of ashes, the alkali of which decomposes the earthy nitrate, and uniting with its acid, which contains chiefly nitrate of lime, turns it into nitrate of potash. The precipitated lime gives the mass a whitish colour, and the consistence of curdled milk. By allowing it to stand in a large trough, the precipitate, which is principally lime, subsides, and the superincumbent fluid, now an alkaline, instead of an earthy nitrate, is carefully removed and boiled for some time in iron kettles, till it is ready to crystallize. It is then removed again to a large trough, in which it shoots into crystals. It is now called "rough shot-petre." In this state it is sent to market, and sells usually for sixteen dollars per hundred weight. Sometimes it is dissolved in water, reboiled, and recrystallized, when it is called refined, and sells for twenty dollars per hundred. One bushel of the clay dirt yields from 3 to 5lbs. and the black dirt from 7 to 10lbs. of the rough shot-petre. The same dirt, if returned to the cave, and scattered on the rocks, or mingled with the new earth, becomes impregnated with the nitrate again, and in a few months may be thrown into the hopper, and be subjected to a new process.

The causes which have produced the nitric salts of these caves, may not yet have been fully developed. But it is highly probable, they are to be ascribed to the decomposition of animal substances.

It is reasonable to suppose, that in an uncultivated country they would become the abodes of wild animals, and even of savage men. That they have been used by the natives as burial places, is certain. In one which I entered, I counted a hundred human skulls, in the space of twenty feet square. All the lesser and more corruptible parts of each skeleton had mouldered to dust, and the whole lay in the greatest confusion. I have heard of many such caves, and to this day some of the Indians are known to deposit their dead in them. From the decomposition of such substances, it is well known the acid of the nitric salts arises, and it would of course unite with the lime every where present, and form nitrate of lime.

Mounds.

4. I have but one more article of curiosity to mention under this division. It is one of those artificial *mounds* which occur so frequently in the western country. I have seen many of them, and read of more. But never of one of such dimensions as that which I am now to describe.

It is situated in the interior of the Cherokee nation, on the north side of the Etowee, vulgarly called Hightower River, one of the branches of the Koosee. It stands upon a strip of alluvial land, called *River Bottom.* I visited it in company with eight Indian chiefs. The first object which excited attention was an excavation about twenty feet wide, and in some parts ten feet deep. Its course is nearly that of a semicircle; the extremities extending towards the river, which forms a small elbow. I had not time to examine it minutely. An Indian said it extended each way to the river, and had several unexcavated parts, which served for passages to the area which it encloses. To my surprise, I found no embankment on either side of it. But I did not long doubt to what place the earth had been removed; for I had scarcely proceeded two hundred

yards, when, through the thick forest trees, a stupendous pile met the eye, whose dimensions were in full proportion to the intrenchment. I had at the time no means of taking an accurate admeasurement. To supply my deficiency, I cut a long vine, which was preserved until I had an opportunity of ascertaining its exact length. In this manner I found the distance from the margin of the summit to the base, to be one hundred and eleven feet. And judging from the degree of its declivity, the *perpendicular height* cannot be less than seventy-five feet. The circumference of the base, including the feet of three parapets, measured one thousand one hundred and fourteen feet. One of these parapets extends from the base to the summit, and can be ascended, though with difficulty, on horseback. The other two, after rising thirty or forty feet, terminate in a kind of triangular platform. Its top is level, and at the time I visited it, was so completely covered with weeds, bushes, and trees of most luxuriant growth, that I could not examine it as well as I wished. Its diameter, I judged, must be one hundred and fifty feet. On its sides and summit, are many large trees of the same description, and of equal dimensions with those around it. One beach-tree, near the top, measured ten feet nine inches in circumference. The earth on one side of the tree, was three and a half feet lower than on the opposite side. This fact will give a good idea of the degree of the mound's declivity. An oak, which was lying down on one of the parapets, measured at the distance of six feet from the butt, without the bark, twelve feet four inches in circumference. At a short distance to the southeast is another mound, in ascending which I took thirty steps. Its top is encircled by a breastwork three feet high, intersected through the middle with another elevation of a similar kind. A little farther is another mound, which I had not time to examine.

On these great works of art, the Indians gazed with as much curiosity as any white man. I inquired of the oldest chief, if the natives had any tradition respecting them; to which he answered in the negative. I then requested each to say what he supposed was their origin. Neither could tell: though all agreed in saying; "they were never put up by our people."

It seems probable they were erected by another race, who once inhabited the country. That such a race existed, is now generally admitted. Who they were, and what were the causes of their degeneracy, or of their extermination, no circumstances have yet explained. But this is no reason why we should not, as in a hundred other instances, infer the existence of the cause from its effects, without any previous knowledge of its history.

In regard to the objects which these mounds were designed to answer, it is obvious they were not always the same. Some were intended as receptacles for the dead. These are small, and are distinguished by containing human bones. Some may have been designed as sites for public buildings, whether of a civil or religious kind; and others no doubt were constructed for the purposes of war. Of this last description, is the Etowee mound. In proof of its suitableness for such a purpose, I need only mention that the Cherokees in their late war with the Creeks, secured its summit by pickets, and occupied it as a place of protection for hundreds of their women and children. Gladly would I have spent a day in examining it more minutely; but my companions, unable to appreciate my motives, grew impatient, and I was obliged to withdraw, and leave a more perfect observation and description to some one else.

Alluvial Formation.

I will now call your attention to the last geological division which came under my observation. It is the alluvial tract, extending from the Dividing Ridge already mentioned, to the Gulf of Mexico. This Ridge is the last range of high land which I crossed on the journey to New Orleans, and lies about six hundred miles north of the Gulf of Mexico. Its course at the place I crossed it, is a little south of west. It divides the waters of the Tennessee from those which proceed directly to the gulf. Travellers always observe it. They often mentioned it to me as the southern boundary of the *stony country*. After crossing it, you see no more limestone; and, which excites more joy in the traveller, no more of the silicious gravel,

with which it is associated, and which is so troublesome to the feet of horses. The soil consists of a soft clay, or light sand, on which you seldom meet with a stone of any kind. The surface of the earth is undulating and hilly, but not mountainous. The water-courses do not move rapidly and tumultuously, as in the limestone country; but form in the soft earth, deep trenches, through which they glide smoothly and silently along. The smallest rivulet often has a trench ten feet deep; and the earth over which it passes, is continually yielding to its gentle attrition.

The only minerals which I observed, are sandstone, common and ferruginous; silicious pebbles in beds of creeks, and occasionally on the uplands; earthy ores of iron, particularly red oxides, and petrifactions of shells, wood, &c. In addition to these, it may here be mentioned that galena has been found in small quantities at Gibson's Port, and at Ellis's Cliffs, in the State of Mississippi: a crystal of amethyst, in the same state, by Mr. Blannerhassett; and a great variety of useful ochres, in many places on the banks of the Mississippi.

In the geological map attached to Professor Cleaveland's Mineralogy, the alluvial country bordering on the Gulf of Mexico, is represented as terminating at Natchez. But why its termination is placed here, I am unable to understand. The country above and below Natchez, so far as it has come under my observation, presents no difference of appearance in its geology, or mineralogy. I am aware that at Natchez, when the water of the Mississippi is lowest, a soft rock is seen, from which lime has been obtained. But this rock is two hundred feet below the surface of the adjoining country; and admitting that it is a limestone rock, there is no difficulty in supposing it may constitute the basis of the alluvial deposit which rests upon it. That the incumbent earth is *alluvial*, can be doubted, I think, by no one who has had an opportunity of examining it. By means of a road, which has been cut obliquely down the side of the bluff, distinct layers of clay, sand, and pebbles, have been exposed for the whole distance from the summit to the base. The same character is observed at a distance from the river, where the earth has been excavated by washing, or dig-

ging. In the vicinity of the town, there is a curious exhibition of the fact. A stream of water has worn away the earth to the depth of fifteen or twenty feet, and is continually lengthening the chasm, in the direction opposite to its own course. Thus, as the water flows from the town, the chasm approaches it. In examining the cause of this fact, I perceived it was owing chiefly to the difference of cohesion in the alluvial deposits, of which the earth is formed. That at the surface, being a thick loam, wears away with more difficulty than the deposit below it, which consists of a loose sand. The consequence is, that the water, which has once obtained a perpendicular passage of a few inches through the first, washes away the second with such rapidity, that it is constantly undermining it. This occasions a perpetual caving in of the surface, in a direction opposite to the course of the stream. The same fact is observed in many parts of the country for a great distance above Natchez. If there be wanting any other fact to prove that the earth on which the town of Natchez stands, is alluvial, it is found in the effect which the Mississippi has upon the base of the Natchez bluff. In consequence of a bend in the river, the whole force of its current is thrown against this base. If it consisted of solid rock, the river would probably have no effect upon it; but of such loose and friable materials is it composed, that the river is continually undermining it, and producing effects not less to be dreaded than those of an earthquake. Several years ago, a great number of acres sunk fifty feet or more below the general surface of the hill; and in 1805, there was another caving of that part directly over the small village at the landing. Several houses were buried in consequence of it, and strong fears are entertained by the inhabitants, that the same cause will yet submerge in the Mississippi, the whole of the present landing-place.

These facts, I think you will say, furnish satisfactory evidence of the alluvial character of the country at Natchez. The same character belongs to the whole extent south of the Dividing Ridge. This may be safely inferred from the general features of the country. But I have two facts, of a geological kind, to mention, both of which go to confirm the opinion.

1. A well was dug in the Choctaw nation, at the agency of the United States, in the year 1812 or 1813, under the direction of Silas Dinsmore, Esq. the agent. The excavation was continued to the depth of one hundred and seventy-two feet. No water was found. At no great distance from the surface, marine exuviæ were found in abundance. The shells were small, and imbedded in a soft clay, similar to marine earth. This formation continued till the excavation ceased. Dispersed through it, were found lumps of selenite, or foliated gypsum, some of which were half as large as a man's fist. Specimens of the earth, the exuviæ, and the selenite, have been transmitted for your examination. This excavation was made one hundred and twenty miles north northeast of Natchez. The Pearl River is four miles to the east of the place, and is the only considerable stream in this part of the country.

2. In the Chickasaw nation, one hundred and seventy miles north of the Choctaw agency, commence beds of oyster-shells, which continue to be seen at intervals for twelve miles. Four miles from the first bed, you come to what is called "Chickasaw Old Town," where they are observed in great abundance. They are imbedded in low ridges of a white marl. They appear to be of two kinds. Specimens of each, and also of the marl, you have received. "Chickasaw Old Town," is a name now appropriated to a prairie, on a part of which there formerly stood a small village of Chickasaws. The prairie is twenty miles long, and four wide. The shells occur in three places as you cross it, and again, on two contiguous hills to the east of it, at the distance of four miles. They do not cover the surface merely. They form a constituent part of the hills or plains in which they are found. Wherever the earth has been washed so as to produce deep gutters, they are seen in greatest abundance. Nor are they petrifactions, such as are found in rocks. They have the same appearance as common oyster-shells, they lie loose in the earth, and thus indicate a comparatively recent origin. They occur *three hundred miles* northeast of Natchez, and but *sixty* miles south of the Dividing Ridge.

If the country north of Natchez is alluvial, no one will doubt it is so from this place to the Gulf of Mexico. At Baton Rouge, one hundred and forty miles north of New Orleans, you meet the first elevated land in ascending from the gulf. The banks of the Mississippi are higher than the interior, and would be annually overflowed by the river, but for a narrow embankment of earth about six feet high, called the Levee. By means of this, a narrow strip of land, from half a mile to a mile in width, is redeemed, and cultivated with cotton and the sugar cane, to the great advantage of the planter. Generally, within one mile from the river, there is an impenetrable morass. The country has every where the appearance of an origin comparatively recent. Not a rock on which you can stand, and no mountain to gladden the eye ; you seem to have left the older parts of creation to witness the encroachments which the earth is continually making upon the empire of the sea ; and on arriving at the mouth of the Mississippi, you find the grand instruments of nature in active operation, producing with slow, but certain gradations, the same results.

A destructive Insect.

But I will not enlarge on a fact already familiar. I will ask your further indulgence only, while I communicate an authentic and curious fact for the information of the zoologist.

In the Choctaw country, one hundred and thirty miles northeast of Natchez, a part of the public road is rendered famous on account of the periodical return of a poisonous and destructive fly. Contrary to the custom of other insects, it always *appears* when the *cold weather* commences in December, and as invariably *disappears* on the approach of *warm weather*, which is about the first of April. It is said to have been remarked first in the winter of 1807, *during a snow-storm ;* when its effects upon cattle and horses were observed to be similar to those of the gnat and musqueto, in summer, except that they were more severe. It continued to return at the same season of the year, without producing extensive

mischief, until the winter of 1816, when it began to be generally fatal to the horses of travellers. So far as I recollect, it was stated, that from thirty to forty travelling horses were destroyed during this winter. The consequences were alarming. In the wilderness, where a man's horse is his chief dependence, the traveller was surprised and distressed to see the beast sicken and die in convulsions, sometimes within three hours after encountering this little insect. Or if the animal were fortunate enough to live, a sickness followed, commonly attended with the sudden and entire shedding of the hair, which rendered the brute unfit for use. Unwilling to believe that effects so dreadful could be produced by a cause apparently trifling, travellers began to suspect that the Indians, or others, of whom they obtained food for their horses, had, for some base and selfish end, mingled poison with it. The greatest precaution was observed. They refused to stop at any house on the way, and carried, for the distance of forty or fifty miles, their own provision; but after all suffered the same calamities. This excited a serious inquiry into the true cause of their distress. The fly, which has been mentioned, was known to be a most singular insect, and peculiarly troublesome to horses. At length it was admitted by all, that the cause of the evils complained of could be no other than this insect. Other precautions have since been observed, particularly that of riding over the road infested with it *in the night;* and now it happens that comparatively few horses are destroyed. I am unable to describe it from my own observation. I passed over the same road in April last, only two weeks after it disappeared, and was obliged to take the description from others. Its colour is a dark brown; it has an elongated head, with a small and sharp proboscis; and is in size between the gnat and musqueto. When it alights upon a horse, it darts through the hair, much like a gnat, and never quits its hold until removed by force. When a horse stops to drink, swarms fly about the head, and crowd into the mouth, nostrils, and ears; hence it is supposed the poison is communicated inwardly. Whether this be true or not, the most fatal consequences result. It is singular,

that from the time of its first appearance, it has never extended for a greater distance than forty miles, in one direction, and usually, it is confined to fifteen miles. In no other part of the country has it ever been seen. From this fact, it would seem probable that the cause of its existence is local. But what it is, none can tell. After the warm weather commences, it disappears as effectually from human observation, as if it were annihilated. Towards the close of December it springs up all at once into being again, and resumes the work of destruction. A fact, so singular, I could not have ventured to state, without the best evidence of its reality. All the circumstances here related, are familiar to hundreds, and were in almost every man's mouth, when I passed through the country. In addition to this, they were confirmed by the account which I received from Col. John M'Kee, a gentleman of much intelligence and respectability, who is the present agent of the general government for the Choctaw nation. He has consented to obtain specimens of the insect for your examination, when it returns again; and will, I hope, accompany the transmission with a more perfect description than it has been possible for me to communicate.

In concluding this narrative of facts, I should be glad to take a comprehensive view of the whole. The bold features in the geology of the United States, as they are drawn by the Blue Ridge, the Cumberland with its associated mountains, and the Dividing Ridge, deserve to be distinctly and strongly impressed upon the mind. Such is the order and regularity of their arrangement, that they can hardly fail to conduct the attentive observer to important results. What has now been said of them, is but an epitome of the whole. I trust the public will soon read, in the pages of your Journal, a detail more perfect and more interesting. And allow me to suggest, whether, under the auspices of our learned societies, some men of science might not be employed and supported in exploring the country, with the prospect of greatly enlarging the science of our country, and of enriching our Journals and Cabinets of Natural History. Tours of discovery have often been made for other objects, and with success. Our country

yields to no other in the variety, or the value of its natural productions. We owe it to ourselves and to the world, to search them out with diligence and without delay.

Somers, (N. Y.) Oct. 1818.

Art. II. *On the Origin of Prairies.*

St. Louis, (Missouri Ter.) March 3, 1819.

Sir,

THE probable cause of the origin and continuance of *prairies* has been the subject of much speculation among the learned and curious. The inquiry is interesting; and many theories have arisen; but although plausible and ingenious, they are, in my opinion, unfounded in fact.

I should be glad to see the following remarks, which were called forth more particularly by the speculations of Caleb Atwater, Esq. (See No. 2. p. 116. of this work) appear in your valuable Journal of Science; and they are, for that purpose, at your service.

With high respect, I am, Sir, your's,

R. W. WELLS.

Benjamin Silliman, Esq.

Mr. Atwater, after describing the prairies and barrens, says, that according to the common opinion, they "were occasioned entirely by the burning of the woods," but, "erroneous information first propagated such an opinion, and blind credulity has extended it down to us." Mr. A. goes on to affirm that, "wherever prairies and barrens are found, there, for a long space of time, water once stood, but was gradually drained off." The writer of this having often visited and observed with attention the nature and appearance of the prairies on the Alleghany mountains, in the states of Ohio, Indiana, and Illinois, and having long been employed by the United States as a surveyor in the prairie country of the

Missouri and Missisippi, thinks he may venture to oppose these speculations without being thought presumptuous. He is of opinion, that the vast prairies and barrens, extending over the greater part of the western states, and over nearly all Louisiana, were primitively occasioned, and have been since continued, by the *combustion of vegetables*, and that *water* had no agency in their formation.

In order to prove the high prairies of the state of Ohio to have been once covered by the waters of Lake Erie, Mr. A. maintains, that the channel of the Niagara river has been worn down "*several hundred feet*" by the attrition of its waters. Mr. A. should have shown, that the banks of the Niagara are, at this time, several hundred feet high, or, like the Potomac, at Harper's Ferry, has broken through a mountain "several hundred feet" high; but neither the one nor the other is the fact; the face of the country, on either side of the river, is comparatively low and champaign; and were it possible for the waters of the lake to rise considerably above their present level, they would meet with no obstruction or impediment, for many miles on either side the river, but would be precipitated over the cataract, into Ontario, and down the St. Lawrence to the Atlantic. But supposing there had been a mountain running between Lakes Erie and Ontario of sufficient height to prevent the water of the former from passing into the latter, it must evidently have found other places through which to escape, and before it would rise high enough to overflow the elevated region of Madison and Fayette counties, in Ohio, it would have passed over into the heads of the Alleghany. But it is impossible to imagine this, unless we suppose the Atlantic to have been six or seven hundred feet higher than at present, which, according to Mr. A. would have made prairie of all the Atlantic states.

The fact of shells and other marine substances having been found in a few places, by digging in the prairies, proves nothing, or proves too much, for they are found in equal or greater quantities all over America, in the sides and near the summit of the Alleghany mountains; on the Andes, in South

America, and the Alps, in Europe. The resemblance which the soil, in the low prairies, and not in the high, bears to the *alluvial*, can justly be attributed, it is presumed, to the leaves and other vegetables and light materials of which they are composed, having been washed by heavy rains, for ages past, from the higher to the lower places. This will also account for the circumstance of trees growing upon the summits of the hills of steep ascent: being thin and poor, the grass neither grows sufficiently long or thick to kill the timber when fired. They *could not* have been islands in this fairy lake; because their summits are frequently much *lower* than high prairie flats a few miles distant. These are facts which will be recollected by those who have ever travelled through a prairie country of any extent.

But suppose it to have been proved, that the waters of Lake Erie once overspread the state of Ohio, from its present shore to Chillicothe, (a supposition which I trust has however been shown to be visionary) does it follow that the prairies were occasioned by such overflowing? If the water, by covering the country, prevented the timber from growing, should we not naturally look for the largest timber on the higher grounds which would be first forsaken by the waters, and for small timber on the low grounds, where the water remained longest? If this be true, (and it is unquestionable) we should then look for prairies on the low grounds bordering on Lakes Erie, Huron, and Michigan; and the thickly timbered country would be on the high land, near the sources of the rivers. But the contrary is absolutely the fact: we find heavy timbered land, and no *prairies*, in the low countries north of the lakes, and none south, either in Michigan territory or elsewhere, until we arrive near the sources of the rivers. If is true, that the water standing in ponds will prevent the timber from growing; but the difference is readily observed between prairies, properly so called, and those bogs.

But to prove farther that water had no agency in bringing the prairies into existence, we may mention those on and near the summit of the Alleghany mountains, (principally in

Alleghany county.*) Many of those prairies are ten or twelve miles in length, and three or four in width. Will it be pretended that the sides of those mountains were also lakes? Farther—the most extensive prairies known, are the very high plains immediately west of the Rocky Mountains, and east of the mountains near the sources of the Arkansaw and Missouri rivers, extending even on the spurs of those mountains; a country the highest perhaps in North America, with a great and continued descent to the Pacific on the one side, and to the Gulf of Mexico on the other.

The barrens, also, found in Kentucky, are another evidence that water had no agency in their formation—they are situate, it is believed, in the elevated parts of the country exclusively.

The writer of this, deeming it unnecessary to say more, or to produce more facts, (although much more may be said, and many more facts produced) to prove that prairies were not lakes, will now endeavour to prove that they were occasioned by the *combustion of vegetables.*

Prairies are found in those countries only that are congenial to the growth of grass, and only where the soil is sufficiently rich to produce it luxuriantly—they are found commonly on high plains, sufficiently drained to prevent water from remaining on them the whole year; for it is by no means necessary that they should be always dry; on the contrary, if they are sufficiently level to prevent the rains from running off immediately, the grass will grow thicker and higher—but they must be sufficiently dry to burn, at least once in two or three years, during the long, dry season, called *Indian summer.* It has been universally remarked, that these seasons are much longer as we proceed westerly—commencing usually in October, and continuing a month and a half or two months, during which the vegetation is killed by the frosts, and dried by the sun; the wet prairies are also dried, and before the season has expired, the grass is perfectly combustible.

* The proper name of these prairies, and of one of the places where they are found, being illegible in the MS. we were obliged to omit those names; we believe however that the sense is not injured.—*Editor.*

The Indians, it is presumed, (and the writer, from a residence in their country and with them, is well acquainted with their customs) burn the woods, not *ordinarily* for the purpose of taking or catching game, as suggested by Mr. A. but for many other advantages attending that practice. If the woods be not burned as usual, the hunter finds it impossible to kill the game, which, alarmed at the great noise made in walking through the dry grass and leaves, flee in all directions at his approach. Also the Indians travel much during the winter, from one village to another, and to and from the various hunting grounds, which becomes extremely painful and laborious, from the quantity of briers, vines, grass, &c. To remedy these and many other inconveniences, even the woods were originally burned so as to cause prairies, and for the same and like reasons they continue to be burned towards the close of the Indian summer.

Woodland is not commonly changed to prairie by one burning, but by several successive conflagrations; the first will kill the undergrowth, which causing a greater opening, and admitting the sun and air more freely, increases the quantity of grass the ensuing season: the conflagration consequently increases, and is now sufficiently powerful to destroy the smaller timber; and on the third year you behold an open prairie.

Ordinarily, all the country, of a nature to become prairie, is already in that state; yet the writer of this has seen, in the country between the Missouri and Mississippi, after unusual dry seasons, more than one hundred acres of woodland together converted into prairie. And again, where the grass has been prevented from burning by accidental causes, or the prairie has been depastured by large herds of domestic cattle, it will assume, in a few years, the appearance of a young forest. Numerous proofs of this fact can be adduced, but a few shall suffice. The vicinity of St. Louis and St. Charles affords instances. Both these beautiful places are situated on what are termed first and second bottoms, or flats—the former on the Missisippi, the latter on the Missouri; the second or upper bottoms, in both, are high plains, that commence within a few hundred yards of the rivers, and extend back many

miles ; all the old French inhabitants will tell you, that the prairies formerly came immediately up to those places. Now the surrounding country for several miles is covered with a growth of trees of four or five inches diameter, near the towns where the burning first ceased, and gradually diminishing in size as you recede, until you at length gain the open prairies. So the barrens in Kentucky ; many of the first settlers of that state distinctly recollect when many of those barrens were clear prairies, now partially covered with small trees. It is deemed unnecessary to offer more proofs, or additional arguments, in support of the opinion that the prairies were occasioned by *fire*, and not by *water*. Indeed one glance at the maps of those extensive prairie countries, surveyed by order of government, where the prairies and woodland are distinguished and correctly delineated, should carry conviction. The timber will be there observed to skirt the rivers ; in the country near their sources a few solitary trees are seen, close on the banks, secure from the fires, and increasing in numbers as the rivers increase in size, and the low grounds become more extensive.

The view given of the prairies by Mr. A. is correct ; but was certainly painted in the *winter* season—they are, at that season, bleak and uncomfortable both to the feelings and sight ; but a full return is made to both when the spring opens. The prairies (particularly to the west) are then covered with the richest verdure, interspersed with an immense variety of wild flowers, that send forth the most grateful odours. Ascend one of the small hills, and you have a prospect as delightful as it is possible for the imagination to conceive. Far as the eye can carry you, a delightful country extends, through which numerous streams wind their serpentine courses, with groves and clumps of trees at intervals upon their banks. On one hand, at an immense distance, the small hills and groves are seen rising above the blue horizon ; on the other, the view is pleasantly terminated by the wood on the low grounds skirting the river to which the smaller streams are tributary—while herds of buffalo, elk, deer, and other animals, are frequently seen slowly travelling to and from the

watering-places, or grazing on the plains. The inhabited parts of the country present a prospect still more pleasing; around the margin of those extensive rich prairies, numerous habitations are seen, withdrawn a short distance in the wood, from the winter's cold and summer's heat—their finely cultivated fields lie in the prairies, which yield at once to the plough, without the previous Herculean labour of demolishing the forest. The area between the farms is a common of pasture to the numerous herds during the spring, summer, and autumn, and a small part mowed affords hay for the winter. The farmer who takes up his habitation in the neighbourhood of the prairies, has *many* of the advantages of an *old* inhabited country, and *all* the advantages of the *new*.

ART. III. *Sketch of the Mineralogy and Geology of the Vicinity of Williams' College, Williamstown, Mass. By* PROFESSOR DEWEY, *of Williams' College, in a letter to the Editor.*

THE following sketch includes a space extending from Hoosack mountain on the east, to the State of New-York on the west, and a small distance into Vermont on the north. The accompanying map shows the relative situation of the streams, and the principal hills and mountains. The map is an enlarged copy of Carleton's map of this part of the state, with one or two corrections, which truth required. The latitude and longitude are probably not perfectly accurate.

Williams' College is situated in a valley, having on the west the hills of the *Taconick** range; on the east, *Saddle Mountain*, which separates it for the most part from Adams; and on the north, and northeast, two hills which belong to the southwestern part of the range of the *Green Mountains*. *Hoosack River*, rising several miles at the southeast, and passing through the northeastern part of Williamstown, winds its course northwest,

* Former orthography, *Toghconnuck* and *Toghconnuc*. That of the text deviates farther from the *Indian*, but is later and preferable.

to the Hudson. It is an inconsiderable stream, about six rods in width, and its current is rapid. From the south, runs *Green River*, a smaller stream, and enters the Hoosack one mile northeast of the college. The *green* colour of this stream, appears to be caused by a *magnesian* clay, which is washed from its banks at the south part of the town. At the west is *Westbrook*, rising in Williamstown, and entering the Hoosack one mile and a half northwest of the college. The *soil* in this whole tract is generally *clayey*, rather light for such a soil, and very rich. A gravelly soil appears in a few places, especially at the northern part. The *interval* on the Hoosack extends only a small distance from its banks, rarely exceeding, and often much less, than half a mile, and presents the common appearances of *alluvial* land. Rising from ten to twenty feet above this interval, the soil is in various places filled with rolled stones of quartz and limestone, as if the Hoosack had once been much above the banks which confine it at present. It is not improbable that its waters were formerly intercepted by the hills in Pownal, five miles at the northwest, forming a small lake in this valley.

The hills of the *Taconick* range, (A*) on which passes the line between Massachusetts and New-York, have generally an elevation from twelve hundred to fourteen hundred feet; *Pownal Mountain* (B) on the north, about fourteen hundred; and *Oak hill* (D) on the northeast, twelve hundred feet above the east college (C.) *Saddle Mountain* (EF) is an insulated mass, separated from the Taconick range by the valley of Williamstown, and from Hoosack Mountain, by the valley in Adams. It lies about south southwest, and is nearly eight miles in length, and two in breadth. It is composed of two ranges, the eastern and highest (FG) being in Adams. The mountain has its name from two of its peaks, which present at a distance the appearance of the two elevations of a *saddle*. The west range (E) is divided into *two parts* quite to its base, which with the slope of the east range encloses, on three sides, an irregular hollow, called the *Hopper*. (H) The northern

* See Map.

part (E) of the west range is nearly two miles in length, and rises to the height of eighteen hundred feet; the southern (I) rises abruptly into a peak of the elevation of seventeen hundred feet. The height of the valley between the two ranges is about fourteen hundred feet. You enter the *Hopper* from the west, passing along a branch of Green River, and a romantic, wild, and sublime prospect opens before you. Nearly east of the entrance into the Hopper, lies the highest point of the Saddle, familiarly called *Gray Lock*, (F) being about twenty-eight hundred feet above the college, and probably four thousand feet above the *tide-water* of the Hudson at Troy. This is the highest land in Massachusetts. About two miles north northeast, is the northern peak (G) elevated twenty-three hundred feet. The valley in Adams is bounded on the east by Hoosack mountain, (K) elevated from fourteen hundred to eighteen hundred feet, and extending several miles west of south: it forms a part of the range which commences at *West Rock* in Connecticut.

The country included in this sketch is principally *primitive;* lying on the west of the summit of the primitive range, which passes southerly into Connecticut. The rocks and minerals will be mentioned in the following order.

1. *Granite.* A few pieces have been found at the foot of Oak hill, one mile northeast of the college. It consists principally of feldspar. Four miles east, are large masses of granite on both sides of the Hoosack, and on ascending Hoosack mountain they become more numerous. The principal part of this is quartz, often of a purple colour; the mica black, and the rocks exceedingly hard. I have never noticed any minerals imbedded in it. The vortex of Pownal mountain is also granitic.

2. *Gneiss* and *Mica Slate.* I connect these two, because they are not often distinct, and appear to pass into each other. They are found in large strata on Hoosack Mountain, on a hill (L) connected with Saddle Mountain, and on the east side of Saddle Mountain. The highest and the west ridge of Saddle Mountain are mica slate. The *Hopper* shows the inclination of the strata quite to the base of the mountain. The inclination is to

the east and northeast, from ten to forty degrees. On the southwest mountain of *Saddle*, the strata are bare to the summit for a considerable distance, and are very fine grained mica slate, having somewhat the appearance of a *soapstone slate.* By this name they are called in Mr. Eaton's Index to Geology. Some of the rocks appear to be *talcose.* I have been able, however, to detect but a very minute quantity of magnesia in any specimens I have tried, though I obtained a considerable proportion of alumine. The higher hills of the Taconick range are composed principally of a similar slate, lying in the same direction, and with similar inclination; but it appears to have passed still farther from mica slate. At the northwest corner of the state, which is near the foot of the ridge in this place, the rock is very similar to some of that on the southwest mountain mentioned above. About a mile northwest of this *corner*, the rocks are cleft in several places, and in one, to such a depth, that the snow and ice remain here through the year. The *Snow Hole* (M) is about thirty feet long, and nearly as deep at the east end, ascends to the west, or towards the summit of the ridge, and is from ten to twenty feet wide. When I visited it in June, the snow was six feet deep on ice of unknown depth. The rock is here passing into *argillaceous slate;* and in many places it becomes *argillaceous* and *chlorite slate.* For the other rock, you have, I believe, proposed the name *talcose slate.*

3. *Quartz.* Though quartz is scattered through all the preceding rock in masses of different sizes, it is found in great quantity on the northeast part of Saddle Mountain, 300 or 400 feet above the college, and thence to the Hoosack along the side of the hill (L.) It is granular, often white and translucent, and often coloured with oxyd of iron. It forms *Stone Hill*, (N) a mile southwest of the college, on the vertex of which is argillaceous slate. This hill slopes to West Brook, where quartz often forms perpendicular banks from 50 to 100 feet high. Here also argillaceous slate rests on the quartz, as well as on the vertex, and on the east side of Stone Hill. Quartz appears again on the opposite side of West Brook, but further north, on a hill connected with the Taconick range.

On these two hills, it lies in large strata, inclining, like the mica slate, to the east and northeast, often divided by veins into rhomboidal masses. On the east side of Stone Hill, it is more granular, and may perhaps be called *arenaceous quartz*, containing a larger proportion of iron. Near the base of Hoosack Mountain, similar quartz is found, which extends round the north side of the Hoosack to Oak Hill, (D) which is wholly composed of it. It lies in rounded fragments, called *hardheads*, through the northern part of the valley, and on the sides of Oak Hill in huge rocks, presenting nearly perpendicular fronts from 20 to 50 feet in height, and many rods in length. The strata are in some places horizontal, and in others nearly perpendicular. In one place it forms plates, from 2 to 5 feet on a side, and from half an inch to several inches in thickness, which are nearly perfect rhomboids, the edges never being perpendicular to the sides. Most of the quartz, except the white, yields a small portion of lime, and has been called *calcareous quartz*. *Greasy quartz*, *rose quartz*, hornstone, and *rock crystal*, are occasionally found; the last in considerable quantity south of Stone Hill. On the stream which issues from the *Hopper*, is arenaceous quartz of a slaty structure, which is an excellent stone for sharpening the *chisels* used by *stonecutters*.

4. *Granular Limestone* is abundant at the *Cave* or *Falls*, in Adams, and on both sides of the Hoosack. The *Cave* or *Falls*, (O) is a singular chasm between limestone rocks. A small stream, which appears once to have run on the surface of the hollow between two small elevations, has now worn a passage many feet in depth through the limestone. The chasm is narrow, winding in its course several rods long, and its opposite sides were connected, till four years ago, by a natural bridge of limestone. From the bridge to the water is 70 feet. There is a dark cavern of several feet diameter, and some passages into the rocks. The white marble walls, the foaming of the water below, the piles and irregularity of the rocks, and the thick overhanging trees, make the scene very wild and interesting. The limestone rests on mica slate. On the west bank of the Hoosack, and east base of the hill, (L) the same

coarse-grained and white limestone is found, resting on the mica slate at the west of it.

At the north and west base of Saddle Mountain, (E) and at a less elevation than the quartz, are extensive strata of limestone, inclining the same way as the mica slate of the mountain. It is less distinctly granular, and less white than the other, but belongs to the same rock. It forms tolerably good marble. Between the strata are crystals of carbonate of lime, rhomboidal, and tending to the *lenticular* form. Some of these strata appear to be composed of blended crystals of this kind. In one place are strata of several rods in length and breadth, which are inclined to the southwest, and thus lie against the mica slate of the mountain. The inclination is about forty-five degrees. Unless this limestone be connected with that on the east of Saddle Mountain, (and no connexion has yet been traced,) it must be considered as lying on both sides of the mica slate, or alternating with it.

5. *Argillaceous Slate* rests on quartz on Stone Hill, and is also found low down in the valley connected with limestone. It constitutes the hill (P) connected with the Taconick range, and also Northwest hill, (Q) whose base is compact limestone. A few miles north, this slate is distinctly marked, and in about 12 miles, forms hills of *roof slate* in Hosack, New-York. It is annually carried in large quantities to Albany. On the first-mentioned hill, it contains some *talc*.

6. *Aluminous slate*. This is found in argillaceous slate, in Pownal, 5 miles north, at the base of a hill east of the Hoosack. It is used to *set* colours.

7. *Chlorite*. In rounded masses, generally with quartz, scattered through the valley in Williamstown, and found at an elevation of some hundred feet on the hills of the Taconick range. Chlorite slate has already been mentioned as occurring on the same range.

8. *Rubble Stone*. In rounded masses through the valley.

9. *Compact Limestone*. In several places low in the valley. Near the college it is white and deep gray. In the veins of the latter, *talc* is diffused in all directions. It contains silex, often from 3 to 15 per cent., and sometimes gives fire with

steel. In some cases it is earthy. On Green River, one and a half mile south of the college, it lies in thin strata, which are divided by seams into very regular rhomboidal plates of various sizes. On some scattered fragments on this river, are found carbonate of lime in crystals, with pieces of white feldspar. On West Brook, this gray limestone is traversed by a vein of quartz, containing sulphuret of iron. The strata of this rock are almost invariably inclined to the east. A coarse *soapstone* is found in the limestone near the college, and a vein made up of brown argillaceous slate, soapstone, quartz, and sulphuret of iron, passes through it. This limestone appears to be very different from that at the base of Saddle Mountain, and from that which yields the marble of Berkshire county. It may still be *primitive*, but *primitive compact limestone*.

10. *Granitell* of Kirwan, *Quartz*, and *Feldspar*. This aggregate forms extensive strata at the east base of Stone Hill. The feldspar is diffused in grains through the quartz, and sometimes crystalline, forming *porphyritic quartz*. This aggregate is often compact and very hard, but frequently it is porous and hard, forming good millstones. Sometimes the quartz appears in such fragments, that the stone resembles *breccia*.

11. *Black Tourmaline*. In beautiful small six-sided prisms, in scattered pieces of mica slate at the base of Stone Hill.

12. *Amianthus*. Only a small specimen, attached to *argillaceous slate*.

13. *Bitter Spar*. On compact limestone at West Brook. Some of the crystals are rhomboids, and some appear to be the half of rhomboids split through their longer diagonal.

14. *Jasper*. The common brown or red, and black, in small rounded masses, and also a piece of variegated or striped jasper.

15. *Galena*. Only a specimen in the limestone on West Brook.

16. *Iron Ore*. *Bog ore* on the Hoosack, a mile northeast of the college. *Yellow earth*, from which *yellow ochre* is obtained in great quantity, in a hill (R) on the bank of Green River, 2 miles south of the college.

At the north end of Saddle Mountain, but low down, yellow earth is connected with *reddle*, or a substance much resembling it. It is less hard than the common reddle, but is composed of the same ingredients.

Magnetic Oxyd of Iron, regular octaedrons, in mica slate at the base of Stone Hill.

Supersulphuret of Iron, massive and crystallized, in argillaceous slate, mica slate, compact limestone, and quartz.

17. *Prase.* Beautiful, and containing sulphuret of iron; lately found by Mr. Eaton, a little east of the summit of Hoosack Mountain, in Florida.

18. *Puddingstone.* Where Pownal Mountain reaches the Hoosack, (T) 3 miles north of the college, are some hills of this aggregate. It is composed of rounded masses of quartz, chlorite, and limestone, of various sizes, connected by an argillaceous cement.

19. *Potters' Clay.* Excellent for vessels of common pottery.

The minerals of this section, it is obvious, are not very important; but as connected with a transverse section of the country, they possess considerable interest. For this reason they have been particularly mentioned.

In the north part of Williamstown is a *mineral spring*, familiarly called the *Sand Spring* (S.) The water rises from several places in a reservoir of about a rod in diameter, and from one to three feet deep. It is very soft and warm, but contains very little saline or earthy matter. Gas continually rises in it. It appears much to resemble the spring at New Lebanon, New-York, and has proved useful in the cure particularly of some *cutaneous* diseases.

The *transverse section*, connected with the map, passes over Stone Hill, and the north part of Saddle Mountain. The different rocks are shown in the section, directly below their places on the map, by drawing lines from the several strata parallel to the sides of the map. This section is connected with that given by Mr. Hitchcock, in the 2d number of this Journal. It ought perhaps to be mentioned, that according to Mr. Eaton's account, the granite of this section sinks under

gneiss to the east, and rises again in Hampshire County, "supporting the same rock of gneiss;" but where it reappears, the granite contains "many imbedded minerals." This section corresponds generally to the place and character of the minerals in any section across Berkshire county. There are, however, some peculiarities which may be mentioned at a future day. The colouring corresponds to that on the geological map in Cleaveland's Mineralogy.

C. DEWEY.

Williams' College, Jan. 27, 1819.

P. S. I have a part of a *rock crystal*, which contains, in a hollow, a *liquid* and a *little air*, and some *black* or *brown particles*, which just sink in the liquid. It was found several years since at Diamond Hill in Cattskill. This hill is only a small eminence on the bank of the creek at that place, composed of limestone, (if I have been correctly informed,) between the strata of which, and on the side next the creek, this and other rock crystals were found. I believe, Sir, you have one like the above, obtained from the same place. The crystal, which was generously given me by Mr. Van Loon, who found it, is only a part of two crystals connected at their bases. Partly under one of the solid angles formed by the united pyramids, is the hollow, about $\frac{5}{8}$ inch long, about $\frac{3}{8}$ filled with the air, and about $\frac{1}{4}$ inch wide. The principal curiosity about it is the liquid. It has never been known to *freeze*. It was exposed yesterday morning an hour to an atmosphere 4 and 5 degrees below zero. It became less fluid, for the bubble of air moved with less ease and rapidity. Still the liquid was fluid. Its colour, which is naturally white, had a slight tinge of yellow. The Rev. Mr. Schaeffer of New-York supposes the black particles are bitumen. Is it possible the liquid is *naptha?* This oil is sometimes colourless, and does not congeal at zero, and that which I distilled from the Seneca oil, does congeal at some degrees below zero. It can hardly be *salt water*, unless it be *very salt*, and even then, it would have congealed at the temperature of the air yesterday. What way can be devised to ascertain what it is?

Jan. 30, 1819.

After seeing the notice of the crystals found at Hudson by Mr. Schaeffer, I wrote to a member of the Lyceum of Natural History, New-York, rather more full an account than the above, of my crystal, &c. I hope to ascertain, whether the liquid will congeal at 10 or 20° below 0, but have some fear lest the crystal should be injured.

C. D.

Art. IV. *On the Tourmalines and other Minerals found at Chesterfield and Goshen, Massachusetts, by Col.* George Gibbs.

(For the American Journal of Science.)

THE schorl of the mineralogists of the last century united a variety of substances which subsequent observations have separated into several species. The green schorl is now the epidote, or the Vesuvian, or the actynolite. The violet schorl, and the lenticular schorl, are the axinite. The black volcanic schorl is the augite. The white Vesuvian schorl is the sommite. The white grenatiform is the leucite. The white prismatic is the pycnite, a species of the topaz, and another is a variety of feldspar. Of the blue schorl, one variety is the oxyd of titanium, another the sappare, and another the phosphate of iron. The schorl cruciform is the granatite. The octahedral schorl is the octahedrite, or anatase. The red schorl of Hungary, and the purple of Madagascar, are varieties of the oxyd of titanium. The spathique schorl is the spodumen.

The black schorl, and the electric schorl, only remained, and to avoid the confusion created by the use of the term schorl, the name of tourmaline was given to this species by that celebrated mineralogist, the Abbè Haüy.*

* If this memoir should ever meet the eye of this amiable man, I trust he will excuse the notice to which his labours so justly entitle him. To him we are indebted for a complete science of crystallography, and for having determined the existence and limit of species, which mineralogists had not obtained, and chemists

The tourmaline is found of almost every colour, and this variety of colour caused at first a number to be formed into new species; which are now considered only as varieties of the tourmaline: such as the rubellite, the tourmaline apyre, and indicolite.

The different analyses of the tourmaline, however, affords a greater variety of results than is known in almost any other mineral.

The specific gravity of the black varies	from	3.08 to 3.36
Green .	from	3.15 to 3.36
Red .	from	2.87 to 3.10
Analysis gives Silex from . .	35	to 58
Alumine . . .	20	to 48
Magnesia . .	0	to 10
Iron	0	to 23
Manganese . .	0	to 13
Alkali . . .	0	to 10
Water . . .	0	to 4

These differences must be in some measure ascribed to a defect in the accuracy of some of the analyses. But it appears that iron has not been discovered in the red tourmaline. It is not unworthy of notice, that the red tourmaline is considered as infusible, but the others fusible.

The red tourmaline has been the most valued, from its scarcity, its employment in jewelry, and the beauty of its crystals. It has been discovered in Siberia, in Moravia, in the East-Indies, and in Massachusetts. In Siberia it is found in a vein of decomposed feldspar in a fine-grained granite, with black tourmaline. In Moravia with quartz and lepidolite (or rose-coloured mica) in gneiss. In the East-Indies, at Ava and Ceylon, but its geological situation is not known, though it is probably in gneiss or granite.

could not determine. He has devoted a long life to the improvement of science; and it is his praise, that he has preserved the meekness of religion amidst the most flattering success. Our scientific countrymen, who have visited Paris, have been particularly indebted to him; and this notice is, in their behalf, both the tribute of justice and gratitude.

The red or rose tourmaline of Massachusetts, is found chiefly at Chesterfield, in a subordinate bed of granite, contained in mica slate. The mica slate is the predominant rock of the country. It is fine grained, and contains an abundance of small garnets. Direction of the strata north and south, varying a little easterly; inclination perpendicular. The bed of granite is about three hundred feet long, and from five to twenty feet broad. It is contained in a narrow ridge of mica slate, which descends into, and is lost in, a valley. The sides are precipitous; the highest part is about forty feet high. On the east side a considerable part of the granite has been destroyed by natural causes, leaving the granite bare. The granite consists chiefly of granular feldspar, with grains of white quartz, and a little light coloured mica, is moderately fine grained, and of a grayish white colour. In addition to tourmaline, it contains also emerald, some of the crystals of which are from three to five inches in diameter. I succeeded in getting one out of its matrix, which is three and a half inches in diameter, and its summit (which is a plane without any additional facettes) is perfect.

The tourmalines are contained chiefly in a false vein of silicious feldspar and quartz, which begins in the centre of the upper edge of the bed of granite, and passes obliquely, descending to the northeast, about twenty feet, where it is intercepted from sight by the mica slate. The vein is about one and a half foot thick in the upper part, and not more than six or eight inches where it is lost. This vein of silicious feldspar contains also a vein of bluish white transparent quartz, which is from three to eight inches thick, and passes through the centre of the vein of feldspar.

When I first examined this rock, soon after its discovery by Dr. Hunt, of Northampton, I determined the feldspar to be a new variety, which has been since confirmed by Professor Hauffman, and now ranks as a new sub-species, under the name of silicious feldspar. (P. 41, of the Mineralogical Table.)

The analysis of Professor Stromeyer, of Gottingen, gives,

Silex	70.68
Alumine	19.80
Soda	9.05
Iron, Mag. and Lime .	38
	99.91

The chief difference between this and the adularia is, that one contains fourteen potash and the other nine soda. Between this and the saussurite, or tenacious feldspar, the one contains eleven of lime, and the other only a trace.

The silicious feldspar, which I suspect to be the basis of the granite, crystallizes in thin rhomboidal tables. They are very frangible, and have one clivage perpendicular to the faces of the tables. Sometimes the tables have one lateral edge or more truncated. In one fragment of a crystal I observed a very obtuse acumination on the table, which appeared to be diedral, the sides being placed on the obtuse lateral edges of the tables. On account of the extreme frangibility of the crystals, it is certainly extremely difficult to seize their characters. Specific gravity only 2.333, probably owing to interstices between the tables. The colour is white, translucid, passing to semi-transparent; lustre sometimes dull, at others shining. The tables are sometimes so aggregated that their edges being exposed, offer wedge-shaped and stelliform figures. The tourmalines are chiefly contained in this vein. They are red, or green, rarely blue or black.

The green tourmalines vary from one-eighth of an inch to one inch in diameter; they are sometimes four inches in length, and are entirely confined to the inner vein of quartz. They are triedral prisms, with convex faces, striated longitudinally, and generally traversed perpendicularly to the axis, with very small fissures filled by some silicious substance, probably feldspar. These green crystals are opaque. The red tourmaline is frequently enclosed in the green. In certain parts of the vein almost every green crystal encloses a red one, which always corresponds by its sides and angles with

the exterior crystal. Sometimes a thin layer of talc intervenes between the outer and inner crystal. In one specimen I found three crystals of the red aggregated together, and enclosed in one of the green. In another crystal I found pyrites in the place of the red tourmaline. The largest crystal of the red was one quarter of an inch in diameter, and four inches long. The red tourmalines vary in intensity of colour, and frequently (particularly in the interior) pass into violet. They pass from translucid to semi-transparent. I have found some that were terminated by triedral pyramids. The crystals are generally perpendicular to the sides of the vein. Small crystals of the red often run from the vein of quartz into the adjoining feldspar. The granite also contains minute crystals of dark and light blue tourmaline, and pale green emerald, with a very few garnets and pyrites. In the lower part of the vein, five to six feet from its interruption by the mica slate, the red tourmaline scarcely appears, and the vein contains chiefly bluish amorphous quartz and green tourmaline. It is therefore probable that this vein will not afford henceforward a great supply of this beautiful mineral.

About six miles from Chesterfield, in Goshen, is found the rose mica, with tourmalines and emeralds interspersed in the granite. Unfortunately the bed of granite has not been discovered, and the specimens we possess are taken from loose rocks, scattered over a small extent of ground in a valley, in the neighbourhood of mica slate. The rose mica is found in a large grained granite with amorphous quartz and silicious feldspar, crystallized and amorphous. The mica is generally of a rose red, sometimes yellowish green. It crystallizes in rhomboidal tables, rarely truncated on the acute angles, passing into the hexaedral table. The tourmalines are light and dark green and blue, of various shades of intensity, frequently acicular and stellated. The black, the red, and the violet tourmalines also occur, but more rarely. Sometimes the green prisms enclose others of blue and black. Specific gravity of these varieties from 3. to 3.1. The green and blue crystals in this locality are translucid or semi-transparent. The feldspar is generally white, rarely light blue. There are

some emeralds in the granite. Among some specimens which Mr. Weeks of New-York, who discovered this locality, was so good as to give me, I found a beautiful rose emerald in its matrix. It is a hexaedral prism, about one and a quarter inch in diameter, the summit a plane, one of the lateral edges has a truncature. About half of the diameter of the prism is free from the matrix, and half an inch of the prism. The colour is a pale rose, rather more transparent than the emerald.

The colour of the mica of the Goshen granite calls to mind the lepidolite or lilalite, which (formerly considered as a distinct species) has now been united to mica. The lepidolite of Rosena is also accompanied by the tourmaline apyre, now the red tourmaline.

ART. V. *Observations on the Minerals connected with the Gneiss range of Litchfield county, by Mr.* JOHN P. BRACE, *of Litchfield, Conn.*

THE gneiss formation is the most extensive of any in Litchfield county, and embraces a number of very interesting minerals. It extends east into Hartford county. On the north it runs into Massachusetts, though frequently interrupted by the limestone formation, which rests upon it. It forms the principal, and in many cases the only rock of the eastern and north-eastern sections of the county, and of the towns of Litchfield, Goshen, Warren, Cornwall, and Norfolk. In Washington and Canaan, it constitutes the rock of the high mountains, and is a part of the same range in the other towns, while the valleys and the more moderate elevations are covered with limestone.

The river Housatonic appears to have made its way through this range, for the same rock continues on the western side of the river parallel to it in the mountains of Kent, Sharon, and Salisbury. In Litchfield commences a range of porphyritic granite, or *porphyritic gneiss*, which alternates with the common gneiss, and in some instances rests upon it. This rock

begins at Mount Prospect, between Litchfield and Warren, and runs through South Farms, Bethlem, and Watertown. The crystals of feldspar in it, are often very perfect.

The primitive granite, as a rock, is not found, though it lies scattered on the surface in great quantities, and large masses. The graphic granite in this region is often remarkably fine. Mica slate constitutes a considerable part of those rocks that rest on the gneiss, though never found in such elevated situations. The mica slate rocks are always inclined at a great angle with the horizon, and follow the direction of the other range. Litchfield village, Chesnut hill, and great part of Harwington, are entirely composed of this rock. The Bantum and the Waterbury rivers have their bottoms of it. Some of the brooks entering the Waterbury, have cut their passage through the mica slate, leaving walls of 40 or 50 feet on each side, traversed by veins of a very coarse-grained granite, and often much mixed with sulphuret of iron. The slate near Harwington meeting-house contains a great quantity of sulphuret of iron. Mica slate likewise lies on the sides of the gneiss range in Canaan and Salisbury, where it dips under the limestone. *Sienite* is scattered on the surface in large masses, especially where the porphyritic gneiss is found. Sometimes, however, the masses are so large as to form mountains. Mount Tom, between Litchfield and Washington, is of this nature, being entirely composed of sienite, resting on gneiss. Slaty sienite is frequently found, having a very large proportion of hornblende.

The minerals that are found in this region, are much more interesting than its geology. In describing them, I shall confine myself to the district east of the limestone range, intending at some future time to investigate and describe the limestone country.

Carbonate of lime, the granular limestone, is scattered over the whole of this region. It often is found in the cavities of decayed quartz rocks, and contains tremolite and augite.

Cyanite or *Sappar*, is found in great quantities, especially in Harwington and Litchfield. A crystalline mass of this was found a few years ago, weighing probably 15 cwt.; it lay on a

mica slate ridge, and undoubtedly had been formerly imbedded in the slate. Beautiful white talc, and small crystals of sulphuret of iron, are disseminated in the mass. Specimens of this mass are in almost all the cabinets in America. Smaller masses have been found associated with feldspar. Small crystals of this mineral are very common in mica slate, with staurotide and garnet. Two of these crystals are often arranged at right angles with each other. In Cornwall it is found in small crystals in the gneiss containing graphite.

Staurotide is very common and very beautiful. It is found principally in mica slate, and exhibits often the cross. It most generally is crystallized in four-sided prisms.

Quartz, of course, is common. Cornwall particularly is distinguished for the *smoky* variety. Ferruginous quartz is found in rolled masses in the whole of this range.

Petro silex, in rolled masses with ferruginous quartz, containing veins of *chalcedony* and *hornstone*, and geodes of quartz crystals, are common in Litchfield and Goshen. Sometimes these masses in the interior assume the appearance of Burr-stone.

Common opal has been found in Litchfield, though rarely. It was part of a mass of ferruginous quartz, with indelible dendritic impression. It is very hard, and its fracture is conchoidal.

Mica is very common. It is found green, white, and perfectly black. It generally occurs in blocks of granite.

Schorl, in rounded crystals, is found in all the granite in this range; in radiating crystals on quartz; and in acicular crystals on mica slate. The large crystals are so brittle, that few of them can be obtained perfect. I once found it in Litchfield, near Plymouth, in prismatic crystals on earthy graphite.

Feldspar is very common and beautiful in all the towns. It is usually found in rhomboidal fragments, and has a fine lustre. It is blue, white, and red. Some of the granite of Torringford is very beautiful, being composed of white and smoky quartz, red feldspar, and green mica. In the porphyritic gneiss, feldspar is in six-sided prisms. One small crystal of adularia, well defined, has been found by E. Wilkins, Esq.

Beryl, both crystallized and massive, is often found in Litchfield in granite. Its colours are green, greenish yellow, pale yellow, and brown. Its crystals are often very perfect.

Garnets are common in all the towns of this range.

Epidote. Very beautiful crystals of this mineral have been found in Washington, associated with feldspar. They are so rounded as to render it very difficult to discover their form. They have a very fine lustre, and are of an olive green; in Litchfield, in crystals with hornblende, and graphic granite, and in veins in sienite.

Perhaps no region can be found containing more beautiful *tremolite.* All its varieties occur; the fibrous of Litchfield and Bethlem is very distinguished. In Canaan, it is found containing crystals of sulphuret of iron. I do not speak here of the tremolite found in the limestone range.

Common asbestus exists in Washington and New Milford.

The white *augite* is a mineral found in this range; in Litchfield, in six-sided prisms very much flattened, on quartz, and carbonate of lime with tremolite. They sometimes occur several inches long.

The *lamellar* and *slaty* varieties of common *hornblende* are very common.

Radiated *actynolite* of a beautiful bluish green in Litchfield; in Canton of a brownish green.

Steatite is common, and is quarried in Litchfield. The varieties of *talc* are very common, connected with steatite, cyanite, and chlorite.

Chlorite in Litchfield, is found on quartz, with talc.

Porcelain clay in Litchfield in small quantities, and in Washington.

Graphite is found in Cornwall in great quantities. Its gangue is gneiss and sienite. It is lamellar, and has a metallic lustre; is easily obtained, and might be made useful. Epidote and cyanite are found with it.

Ores are not common. Oxides of iron, and sulphuret of iron are scattered over the whole range. Near Mount Prospect in Litchfield, sulphuret of iron in mass is in great quantities;

and sulphate of iron on the surface of the ground near it. A stone containing a few grains of *native copper* was found in Litchfield.

The red oxyde of *titanium* occurs in Litchfield sparingly. A very handsome specimen of the *reticulated oxyde of titanium*, was picked up. It was on mica, and the mica had an evident tendency towards the same form.

BOTANY.

ART. VI. *An Account of two North American Species of Rottböllia, discovered on the Sea-coast in the State of Georgia, by Dr.* WILLIAM BALDWIN, *of Philadelphia.*

Flowers in pairs, or two from each joint of the rachis, one neutral. The neutral, or imperfect flowers, pedicillate.

Rottböllia corrugata.

CULMO erecto, compresso, sulcato, glabro, ramoso: foliis longis angustisque: spicis sub-compressis, nudis super uno latere, solitariis et terminalibus, supremis approximatis; calycis bivalvis, valva exteriori transversè *corrugata* et longitudinaliter rugosa: corolla trivalvis.

Culm erect, compressed, sulcate, smooth, ramose: leaves long and narrow: spikes slightly compressed, naked on one side, solitary and terminal, approximating towards the summit: calyx 2-valved, the exterior valve transversely *corrugate*, and longitudinally wrinkled: corolla 3-valved. *Vid. Nuttall's North American Genera*, v. I. p. 84.*

* *Mr. Nuttall* will excuse me for retaining *my own specific name.* His knowledge of this plant was derived from my Herbarium, where he found it under the name of *tripsacum cylindricum, Mich?* Although it can hardly be the plant of

Culm two to three feet high, with a very solid exterior, but *spongy* within, compressed, and deeply grooved on its inner angle the whole length between the joints. *Leaves* long, narrow, and acute, scabrous on the margin and midrib. *Sheaths* compressed, corresponding with the culm, shorter than the internodes, open, with membraneous margins. *Peduncles* short, clothed with a thin membraneous acute pointed sheath, which generally encloses also the base of the spike. *Spikes* two to three inches long. The flowers are arranged in alternate order, but occupy only one side of the rachis, as in the *R. dimidiata*. The neutral florets, or *clavate* pedicels, are joined *laterally* to the perfect flowers. Articulations of the rachis remarkably tumid, attenuated beneath, flat on the interior side, exteriorly convex, scabrous, and longitudinally striate. The exterior valve of the calyx, in the perfect flowers, is ovate, obtuse, very thick, cartilaginous, the inner margin inflected, and deeply marked on its outer surface with from three to five *corrugations*, with longitudinal ridges between them; the interior valve is smaller, of equal length, acute, ruled, coriaceous, smooth, and with the inner margin also inflected. The valves of the corolla are membraneous, ovate, acute, white, shorter than the calyx, the exterior one the longest. The neutral florets are sometimes male, but most commonly consist of nothing more than a 2-valved calyx, the valves equal, gaping, scabrous, and much smaller than those of the perfect flower. *Stamens* 3, very short. *Anthers* twin, yellow. *Styles* 2, rather longer than the stamens. *Stigmas* small, plumose, dark purple.

Discovered between St. Mary's and Jefferson, in Camden county, Georgia, on the 13th of July, 1813. *Inhabits* flat, moist pine barren. I have not seen it "on the sea-coast of Florida."

OBSERVATIONS.

It will be perceived that my description of this plant differs *materially* from that of *Mr. Nuttall*. This has unavoidably

Michaux, it was so considered by the late *Dr. Muhlenberg*, when specimens were first communicated to him. It remains under this name in his Herbarium, but is not included in his *work on the grasses*. He left it for me to describe along with other new and doubtful plants from the south.

arisen from *my* having attended to it in its living state, and from *his* not availing himself of the information which it would have afforded me pleasure to have communicated, had he done me the favour to have requested it, or informed me of his wish to publish an account of plants thus obtained. He has called the culm *solid*, leaves *rather short*, spikes *cylindric*, *axillary*, the flowers and rachis *entirely smooth*, pedicel of the neutral flower *emarginate*, outer valve of the hermaphrodite calyx *acute*, the valves of the corolla *obtuse*, and the styles *very short*. I have not been able to confirm the above *characters*, nor do I find them even in the dried specimens. Besides, he has omitted to inform us that the rachis is *naked on one side*. This is a most important and prominent *specific character*, the omission of which would necessarily lead to much doubt in identifying the species. What he means by stating that the "outer valve of the hermaphrodite flower is 3-valved," I cannot imagine, nor do I comprehend what is intended by an "exterior auxiliary valve, or neutral rudiment; nearly the length of the calyx." I have noticed in a single instance, connected *laterally* with the corolla of the perfect flower, two very delicate, narrow, acute pointed bodies, the length of the outer valve, and of the same quality and appearance; but these I have considered as accidental, and cannot perceive any thing about them like *neutral rudiments*. Nor can I consider the articulations of the rachis as "deeply excavated." They are, as already stated, flat on the inner side, and constitute from their *flexuous* form, position, and connexion with the pedicels of the neutral florets, an arch, in which the perfect flowers are situated.

*Rottböllia ciliata.**

Culmo erecto, tereti, glabro, ramoso: foliis angustissimis, brevibus: spicis cylindricis super pedunculis teretibus longis, solitariis terminalibusquæ: calycis bivalvis, margine valva exteriori *ciliata*: corolla bivalvis.

* This is the specific name found in my Herbarium by *Mr. Nuttall*, under which it had been previously transmitted to Mr. Elliott. *Vid. Nuttall's North American Genera*, v. I. p. 83.

Culm erect, terete, smooth, ramose: leaves very narrow, short: spikes cylindrical upon long terete peduncles, solitary and terminal, calyx 2-valved, the margin of the exterior valve *ciliate:* corolla 2-valved.

Root perennial. *Culm* two to four feet high, *generally* ramose, solid, and terete, except that between the joints where the branches originate, it is grooved on the inner side, and often ciliate on its angles near the joints. The branches originate towards the extremity, commonly from two to three in number, each supporting a single terminal spike. *Leaves* very narrow, acute, comparatively short, those beneath much the longest, rigid, somewhat involute, and sharply serrulate towards the apex. *Sheaths* rather shorter than the *internodes*, open to the base, but closely embracing the culm. *Spikes* 3 to 5 inches long, the peduncles clothed with a very delicate acute pointed sheath, which embraces it so closely as almost to elude observation, varying much in length, but seldom extending to the base of the spike. Peduncles scabrous near the spike. *Flowers* alternate, the *male* or neutral florets situated on one side of the rachis. *Rachis* compressed, slender, flexuous, hairy on its exterior surface. Pedicel of the neutral florets also compressed, and hairy on its exterior surface. *Valves* of the calyx *nearly* equal, lanceolate, acute, coriaceous, polished, the inner margin of each inflected. The exterior margin of the outer valve finely *ciliate* towards the apex. *Valves* of the corolla lanceolate, acute, membraneous, nearly the length of the calyx. The male or neutral, are rather smaller than the hermaphrodite flowers. *Stamens* 3, very short. *Anthers* twin, purple. Styles 2, excerted, plumose, dark brown.

Discovered in flat pine barren on the north side of Satilla river, in Georgia, on the 21st of October, 1815.

GENERAL OBSERVATIONS.

These plants are unquestionably allied to *andropogon* in their mode of flowering, but have nevertheless sufficient *essential characters* to distinguish them. In *habit*, they appear but slightly similar. They differ *principally* from their congeners

in the pedicellate character of their neutral florets. *The spikes are not axillary in either of them.* The branches are *axillary*, of which several sometimes originate from the same axil in the *R. corrugata.* Each spike, when fully evolved, is not only *pedicellate*, but the *pedicel*, or peduncle, is connected with a *culm* containing one, two, or more joints.* The culm is not compressed, nor the leaves long in the *R. ciliata*, as stated by *Mr. Nuttall*, who appears to have confounded the two species in these, and some other instances. The joints of the rachis in both are *fragile*, the joints of the culm in neither.

Another species noticed by Michaux, and included in all our books as the *R. dimidiata, L.* has long been familiar to the southern botanists. Whether this be the *dimidiata* found also on the sandy shores of India, or the *compressa* of the same country, as suggested by *Mr. Elliott*, or a species distinct from either, I am not prepared to determine. But I have collected this plant in the Bermudian Isles, at Rio de Janeiro, and Bahia, on the Brazilian coast, and lastly on the island of Flores, near one hundred miles from the mouth of the Rio de la Plata, as well as on the main in the Banda Oriental.

ART. VII. *Floral Calendar kept at Deerfield, Massachusetts, with Miscellaneous Remarks, by Dr.* STEPHEN W. WILLIAMS, *of Deerfield.*

To Professor Silliman.

SIR,

ANY thing which has a tendency to elicit facts with regard to the climate of a country must be interesting. I believe that observations upon the time of the germination, foliation, florification, and fructification of plants, afford a much more correct criterion respecting climate than thermometrical, or other meteorological journals. They should be made at the

* *Mr. Nuttall* was probably deceived from having examined the *spikes* before they were fully evolved.

same time in various parts of the country, and for several years in succession. I send you a Calendarium Floræ, with miscellaneous remarks, made in Deerfield, Massachusetts, during a part of the years 1811, 1812, and 1818, which, if you please, you may insert in your valuable Journal. Latitude of Deerfield, 42° 32′ 32″, longitude 72° 41′.

1811.

March 1. Blackbirds arrived.
15. Black ducks arrived. Bees out of the hive.
20. Early garden peas, lettuce, and peppergrass sown.
28. The woods were swarming with pigeons. Wild geese passed over.

The greater part of the month of March was warm and pleasant. The sugar-maple yielded its sap profusely for a few days, but the nights were so warm that much less than the usual quantity of sugar was made this year.

April 1. Frogs begin to sing. Peas and oats sown.
8. Buds of the lilac, (*Syringa vulgaris*) the small red rose, the elm, (*Ulmus Americana*) the apple, and the peas considerably swoln.
14. Dandelion (*Leontodon taraxicum*) in full flower.
20. Indian corn planted; a few garden seeds sown. Martins and bank swallows arrived. Leaves of the currant and gooseberry expanded. Weather for a few days past sultry and smoky.
21. Blue violet (*Viola cucullata*) in full flower. Shad-bush (*Aronia Botryapium*) in blossom. Flower-buds of the lilac swoln; likewise the flower-buds of the cherry, pear, and apple.
23. Blood-root (*Sanguinaria Canadensis*) in full flower.
25. Asparagus fit for the table.
26. Chili strawberries in flower; this plant begins to blossom early, and continues to flower late in the season. English cherry, black heart (*Prunus cerasus*) in full flower.
27. Garden violet (*V. tricolor*) in full flower.

April 29. Flower-buds of the peach expanded. Large white plum (*Prunus domestica*) in full flower. Winter pear (*Pyrus communis*) in flower.

May 1. Red and white currants in flower.
2. Leaves of the Lombardy poplar (*Populus dilatata*) expanded.
3. English and field strawberries in blossom.
4. Butternut (*Juglans cinerea*) in blossom.
6. House flies arrived.
7. Apple-trees in full flower.
8. Lilac in full flower. Red-headed woodpecker arrived.
15. Rye (*Secale cereale*) beginning to head. Pleasant days and cold nights. Hard frosts for a few nights past.
18. Honeysuckle (*Azalea nudiflora*) in full flower.
19. Small red rose in flower. Choke cherry (*Prunus Serotina*) in full flower.
25. Common red clover (*Trifolium pratense*) in full flower.
26. Garden peas in full flower. Hummingbird arrived.
27. Night-hawks arrived.
30. Sugar-maple in flower.

June 2. Locust-tree (*Robinia pseudacacia*) in flower.
3. Field strawberries beginning to ripen. Piony in flower.
4. High blackberry (*Rubus villosus*) in full flower. Broad-leafed laurel (*Kalmia latifolia*) beginning to blossom.
7. Snow-ball, guelder-rose (*Viburnum opulus*) in full flower. Radishes fit for the table.
12. Our farmers begin to mow their first crop of grass in low land. Large white rose (*Rosa alba*) in full flower.
21. Red currants beginning to ripen in plenty. Blackberried elder (*Sambucus canadensis*) beginning to blossom.
27. Indian corn tasseling. Black raspberries beginning to ripen. Nodding lily (*Lilium canadense*) in flower.
29. Potato (*Solanum tuberosum*) in full flower.

July 1. Red raspberry (*Rubus strigosus*) beginning to ripen. Poppy (*Papaver somniferum*) in flower.

July 5. Chestnut-tree (*Castanea Americana*) flowering.
6. Large red cherry (*Prun. ceras.*) fully ripe. String beans fit for the table.

Perhaps we never experienced a greater degree of heat in this part of the country than has been felt for three days past. A number of hives of honey have melted during the heat.

14. Cucumbers fit for the table.
15. Rye fit for the sickle.
16. Black whortleberries (*Vaccinium resinosum*) ripening.
19. Early potatoes fit for the table. Indian corn (green) fit for the table.
20. Jenneting apples ripe.
21. Choke cherries (*Prun. serotina*) ripe.
26. Gooseberries ripening.

August 1. Martins departed.
5. Barn and bank swallows collecting in millions, upon our islands in the river, to depart.
12. Blackberries ripe.
20. Thorn apple (*Datura stramonium*) in full flower. Elderberries fully ripe.

September 1. Common pear fully ripe. Rare-ripe peaches fully ripe.
6. Bergamot pears fully ripe.
17. Great grapes (*Vitis æstivalis*) fully ripe. Frost grapes (*Vitis cordifolia*) ripening.
21. Butternuts beginning to fall from the tree.
24. Our farmers busily engaged in harvesting their corn.
26. Butternut defoliating.
28. Elm beginning to defoliate.

October 2. Chestnut burrs opening. Tree defoliating.
8. Sugar-maple and sycamore defoliating.
26. Blackbirds arrived again. Squirrels in plenty in our woods, though chestnuts and walnuts are scarce. Butternuts plenty. Cider and apples in great abundance.

November 20. Wild geese returning to the southern regions.

1812.

March 21. Blackbirds, woodpeckers, and robins arrived. Wild geese passed over.
23. Bees out of the hive.

April 3. Black ducks arrived. Large flocks of pigeons passed over.
9. Flower-buds of the elm considerably swoln.
11. Skylarks arrived.
12. Frogs begin to sing.
13. Leaf-buds of the soft maple (*Acer rubrum*) much swoln.
13. Leaf-buds of the gooseberry much swoln.
16. Early garden peas sown.
19. Dandelion (*Leon. tarax.*) in full flower. Blue or meadow violet (*V. cucullata*) in flower. Leaves of the lilac beginning to expand. Our farmers busily engaged in ploughing for sowing.
23. Peas and oats sown, and Indian corn planted.
25. Swallows arrived, and whippoorwills begin to sing.
27. Leaves of the gooseberry, and willow (*Salix Muhlenbergii*) beginning to expand.

May 5. Martins arrived.
10. Asparagus fit for the table. Blood-root (*Sang. canadensis*) in full flower.
11. Chili garden strawberries beginning to blossom. Flower-buds of the lilac swoln.
12. Elm in full flower. Leaves of the meadow violet beginning to expand.
13. Garden violet (*V. tricolor*) in flower.
14. Field strawberries in full flower. Shad-bush (*Aronia botryapium*) in blossom.
15. English cherry beginning to flower.
19. Winter pear beginning to blossom.
22. Humingbirds arrived. Large white plum (*Prunus domestica*) in full flower. Butternut beginning to flower.
23. Flower-buds of the peach (*Amydalus persica*) beginning to expand. Gooseberry in flower.

May 27. Apple-trees beginning to blossom.
29. Early garden lettuce (*Lactuca sativa*) fit for the table.
30. Apple-trees in full flower.
31. Night-hawks arrived.

Vegetation has put forth more to appearance in three days past than in all the spring before. Nature seems to revive from a state of torpidity, from the warm and invigorating rays of the sun. The month of May has been more backward than the month of April, 1811. The observation of elderly people, that the month of April, old style, was never known to terminate without producing apple-blossoms, has by no means been verified this year, they being now (June 1st.) in full flower. The snow upon the mountains, thirty or forty miles back, is at a great depth; so deep, that on the warm day of the 29th our river rose a foot from its melting. Diseases of the chronic kind have been peculiarly severe for three months past. The gladsome return of the cheering warmth will probably renovate the enfeebled constitutions of many of our aged people.

June 1. House flies arrived.
5. Choke cherry (*Prun. serotin.*) in full flower. Honeysuckle apple (*Azalea nudiflora*) in full flower.
8. Piony in full flower. Snowball (*Viburnum opulus*) in full flower. Flower-de-luce (*Iris versicolor*) in blossom.
11. Early peas in blossom. Carraway (*Carum carui*) in flower.
15. Locust-tree (*Robin. pseudacac.*) in full flower. Field strawberries beginning to ripen.
18. Common red clover in full flower. Cranesbill (*Geranium maculatum*) in blossom. Red raspberry in full flower.
23. Chili strawberries beginning to ripen. Garden sage (*Salvia officinalis*) in full flower.
29. Our farmers busily engaged in haying.
30. Large red rose, large white rose, and damask rose (*Rosa damascena*) in flower.

July 1. White pond lily (*Nymphæa odorata*) in flower.
4. Black elder (*Sambucus canadensis*) in full flower.
7. Early peas fit for the table. Red and white currants ripening.
8. Nodding lily (*Lilium canadense*) in flower.
11. Garden beans (*Phaseolus vulgaris*) in full flower. Chestnut in flower. Black raspberries ripening.
20. Early corn tasseled (*Zea mays.* Variety *præcox.*) Red raspberries fully ripe.
22. Whortleberries ripe (*Vaccin. resinos.*)
24. Cucumbers fit for the table.
28. Early potatoes fit for the table.
29. Rye fit for the sickle. Early garden squashes (*Cucurbita Melo-pepo*) fit for the table.

August 2. Jenneting apples ripening.
5. Early corn fit for the table.
8. Wheat (*Triticum hyburnum*) fit for the sickle.
28. Summer peas ripening.

September 4. Watermelons and muskmelons ripe.
5. Swallows departed.
6. Elderberries fully ripe.
11. Choke cherries and wild cherries (*Prunus virginiana*) ripe.
12. Yellow plum (*Prunus chicasa*) fully ripe.
15. Butternut beginning to fall from the tree.
16. Our farmers making their first cider.
22. Great grapes ripe.

October 2. Butternut and elm beginning to defoliate. Chestnut-burrs beginning to open.
9. Our farmers beginning to harvest their Indian corn.

1818.

March 11. Bluebirds arrived.
13. Woodpeckers, robins, and blackbirds arrived. Bees out of the hive.

March 14. Broad-leaved panic grass (*Panicum latifolium*) beginning to sprout on a southern exposure, while there is sleighing in the street. A solitary spathe of skunk-cabbage (*Pothos fœtida*) beginning to show itself on the same exposure. Leaves of curled dock (*Rumex crispa*) appeared in the same place. Maple-trees tapped for sugar.

16. *Pothos fœtida* in full flower.

25. Black ducks arrived. Catkins of the poplar-tree (*Populus tremuloides*) expanded. Catkins of the speckled willow (*Salix Muhlenbergiana*) expanded.

30. Wild geese arrived. Phœbe arrived.

It began to rain hard on the first of March, and continued raining two days and a half, which nearly carried off an immense body of snow which enveloped the ground. Our rivers, which were more firmly locked with ice than they had been before known for many years to be, rose above their usual bounds, and swept the ice with such rapidity down their channels as to destroy most of the bridges on Connecticut river, besides doing immense damage in other respects. Our meadows were nearly all under ice and water; and at that time a great explosion was heard in the north meadows, two miles from the street, similar to the noise of a cannon. It was occasioned by the throwing up of an immense quantity of frozen ground, which is a great curiosity. The cause is not yet satisfactorily explained. The weather was very warm and pleasant from the 4th to the 22d. What snow the rain did not carry off was melted by the sun during the pleasant weather. Vegetation had begun to put forth rapidly, and many of our birds of passage had arrived. A storm, which commenced on the 22d, as rapidly retarded the progress of vegetation as it was before accelerated, and the remainder of the month was gloomy and uncomfortable. Mud mid-leg deep in the streets.

April 7. Flower-buds of the elm (*Ulmus americana*) beginning to swell.

April 8. Leaf-buds of the lilac (*Syring. vulg.*) beginning to swell.

10. Leaf-buds of the soft or meadow maple (*Acer rubrum*) beginning to swell. Black alder (*Alnus serrulata*) in flower. American hazle (*Corylus americana*) in flower, and its catkins appearing.

11. Fair and pleasant, after a long storm. It has rained sixteen days in succession. Frogs begin to sing. Leaf-buds of the English cherry (*Prunus cerasus*) black heart beginning to swell. Garden peas sown.

12. Flies in myriads arrived in our streets. Catkins of the butternut (*Juglans cinerea*) beginning to swell. Saxifrage (*Saxifraga virginiensis*) in flower.

13. Skylarks arrived.

14. Sweet fern (*Comptonia asplenifolia*) in flower. White birch (*Betula populifolia*) in flower.

16. Our farmers beginning to plough for spring wheat.

18. Bank swallows arrived.

19. Leaf-buds of the currant, the gooseberry, and the apple, considerably swoln.

20. Dandelion (*Leon. tarax.*) beginning to flower. *Viola cucullata* beginning to blossom.

22. Our farmers ploughing for peas and oats. The snow upon the hills 20 miles north and west from Deerfield is two feet and a half deep, and the winds from those quarters are so chilly as to retard the progress of vegetation. Icicles scarcely melted upon the south side of buildings in Halifax, Vermont; and it is too cold for making sugar.

25. Blood-root (*Sanguinaria canadensis*) in flower on a warm south side hill. Leaves of the English gooseberry beginning to expand. Venus's pride (*Houstonia cœrulea*) in flower. Early life-everlasting, (*Gnaphalium plantagineum*) crowfoot, (*Ranunculus fascicularis*) tooth-root, (*Dentaria laciniata*) and meadow-rue (*Thalictrum cornutum*) in full flower.

26. Trailing arbutus (*Epigaea repens*) in full flower. Leaves of the barberry (*Berberis vulgaris*) beginning to ex-

pand. Five-finger, (*Potentilla pumilla*) adder's-tongue, (*Erythronium dens-canis*) liver-leaf, (*Hepatica triloba*) and wind-flower, (*Anemone nemorosa*) in flower.

April 27. Early potatoes and early corn planted. Elm in full flower.

29. Water crowfoot (*Ranunculus sceleratus*) and American cowslip (*Caltha palustris*) in full flower.

30. Daffodil (*Narcissus pseudo-narcissus*) and rue-anemone (*Anemone thalictroides*) in flower.

May 1. Soft maple (*Acer rubrum*) in flower.

2. Martins arrived.

3. Leaves of the gooseberry beginning to expand.

4. Leaves of the currant and lilac beginning to expand. Pigeons arrived.

5. Wood bulrush (*Juncus sylvaticus*) in flower. A great freshet in our meadows, from the melting of the snow upon the mountains, and from the great rain which has continued nearly a month. Beth. nodding trillion (*Trillium rhomboideum*) in flower.

7. Flowers of the garden violet (*V. tricolor*) beginning to expand.

8. The young heads of asparagus breaking the ground.

9. Our farmers busily engaged in planting their Indian corn, though the weather is excessively cold. Sowed onions, parsnips, &c.

10. Bobylincolns (*Bob of lincolns*) arrived. Flower-buds of the lilac appearing.

11. Field strawberries (*Fragaria virginiana*) in full flower. Colts-foot (*Tussilago farfara*) in flower.

12. Whip-poor-wills begin to sing.

13. Spice-bush (*Laurus benzoin*) in full flower. A freshet in the meadows.

14. Goldthread (*Coptis trifolia*) in full flower.

15. Rattlesnake violet (*Viola primulifolia*) in full flower.

16. Chimney swallows arrived.

17. Leaves of the apple-tree expanding. Sugar maple (*Acer saccharinum*) in full flower. Garden daisy (*Bellis perennis*) in full flower.

May 18. Asparagus fit for the table.
19. Smooth gooseberry (*Ribes uva-crispa*) in flower.
20. Shad-bush (*Aron. botryap.*) in flower.
21. House wrens arrived. Moose-wood (*Dirca palustris*) in flower.
22. Garden currant (*Ribes rubrum*) beginning to flower.
24. Wake-robin (*Trillium cernuum*) and peas (*Pyrus communis*) in flower.
25. Our mountain scenery diversified. Weather very warm. Garden potatoes and garden corn, planted on the 27th April, breaking the ground. Garden beans, cucumbers, squashes, watermelons, &c. planted.
26. Damson plum (*Prunus domestica*) and yellow or wild plum (*Prunus chicasa*) in flower. Elder (*Sambucus canadensis*) in flower. Carolina chatterer arrived.
27. Garden gooseberry (*Ribes grossularia*) and avens (*Geum rivale*) in blossom. Weather intensely warm. Thermometer at 36° at 2 o'clock, P. M. yesterday.
29. Apple-trees in full flower. Night-hawk arrived.
30. Choke cherries (*Prun. Serotin.*) in flower.
31. Lilac in full flower.

The weather till the last week in May was very cold and rainy. Perhaps we have never known more gloomy weather than that of the first twenty days of the month. The last week in the month of May was unusually warm and fine. Vegetation has put forth more within this week than it has in all the season before. The blossoms on apple-trees are scanty, and there is but little prospect of fruit. Peach-trees in the vicinity of this place were all killed by the extreme cold winter.

June 1. Hummingbirds arrived.
2. Honeysuckle apple (*Azalea nudiflora*) in full flower.
3. Blue-eyed grass, (*Sisyrinchium anceps*) *Krigia virginica*, and thorn-bush (*Cratægus coccinea*) in flower. Garden seeds, planted on the 25th ult. have vegetated 3 or 4 inches high. Garden rhubarb (*Rheum tataricum*) in flower.

June 4. Garden rocket (*Hesperis pinnatifida*) in flower.

6. Yellow water lily (*Nuphar advena*) in full flower. Flower-de-luce (*Iris virginica*) in flower. Garden peas in full flower.

The weather for twelve days past has been unusully warm and sultry. The thermometer, much of the time in the middle of the day, has stood at 84°, and vegetation has put forth with astonishing rapidity.

8. House-flies arrived.
9. Horse-radish (*Cochlearea armoracea*) and peony in full flower.
10. Chives (*Allium schœnoprasum*) in full flower.
11. Smooth stem lichnidea (*Phlox maculata*) in full flower. Our farmers busily engaged in hoeing their corn.
12. Fumitory (*Fumaria officinalis*) in full flower.
13. Field strawberries beginning to ripen.
14. Locust-tree (*Robinia pseudacacia*) in full flower.
15. Locusts appearing in the south part of the town. The last time of their appearance here was in the year 1801. Their periodical returns are once in seventeen years. Their appearance in the years 1733, 1750, 1767, 1784, and 1801, is recorded on the town-book. They first attack the leaves of the black oak (*Quercus nigra.*)
16. Small red rose in flower.
17. *Rosa caroliniensis* in full flower.
18. Garden sage (*Salvia officinalis*) in flower.
19. Mock syringa (*Philadelphus coronarius*) in flower.
20. Tulip-tree, commonly called cypress or white-wood (*Liriodendron tulipifera*) in blossom.
21. Carnation pink (*Dianthus caryophyllus*) in flower.
22. Our farmers commenced haying. An immense crop of grass on the ground.
23. Side-saddle flower (*Sarracenia purpurea*) in flower.
24. Common St. John's wort (*Hypericum perforatum*) in full flower.

June 26. Garden radishes fit for the table.
27. Early garden peas fit for the table. Weather intensely warm.
28. American lime or linden-tree (*Tilia americana*) in flower.
30. Flax (*Linum usitatissimum*) in full flower. Thermometer in the shade at 2 P. M. 100°.

Vegetation has put forth and increased with a more astonishing rapidity this month than has ever been known. Notwithstanding the spring was very backward, the season now is forward. Our farmers commenced their first haying about a week earlier than they did last year.

July 1. White water lily (*Nymphœa odorata*) in flower.
3. Red and white currants ripening. Yellow day lily (*Hemerocallis flava*) and *Lilium canadense* in full flower.
4. Cucumbers and watermelons in flower. Early summer corn (*Zea mays*, variety *præcox*) beginning to tassel. Garden rue (*Ruta graveoleus*) mustard (*Sinapis nigra*) motherwort, (*Leonorus cardiaca*) and mullin (*Verbascum thapsus*) in full flower. Blue whortleberries (*Vaccinium frondosum*) beginning to ripen. Dewberry (*Rubus trivialis*) ripening.
5. Poppy (*Papaver somniferum*) in flower.
6. Garden squashes (*Cucurbita Melo-pepo*) in flower.
7. Red raspberry fully ripe.
10. Black raspberry fully ripe.
11. String-beans fit for the table.
12. Unicorn plant (*Martinia proboscidea*) in full flower.
13. Thorn apple (*Datura stramonium*) and marygold (*Tagetes erecta*) in full flower.
15. Great water plantain (*Alisma plantago*) and field clover (*Trifolium arvense*) in flower.
17. Mad dog weed (*Scutellaria lateriflora*) and purple vervain (*Verbena hastata*) in blossom.

The weather for three weeks past has been excessively warm. The thermometer, for several days, has stood above

95°, part of the time at 98°. Our lands are now parching with drought. Our grass fields are completely embrowned. Our farmers beginning to reap their rye.

July 19. Cucumbers fit for the table. Early corn (*green*) fit for the table.

21. Mother of thyme (*Thymus vulgaris*) in full flower.
22. Fig-wort (*Scrophularia marylandica*) and loosestrife (*Lysimachia stricta*) in flower.
24. Morning-glory (*Convolvulus sepium*) and *Orchis ciliaris* in full flower.
26. Whortleberries (*Vaccinium resinosum*) ripe. Single-seeded cucumber (*Sicyos angulata*) in flower.
28. Garden lettuce and hop (*Humulus lupulus*) in full flower.
30. Our farmers reaping their wheat—a tolerable crop. Buckwheat (*Polygonum fagopyrum*) in flower.

We had a great rain about the 20th, which restored the parched vegetation. The latter part of the month was, however, warm and dry.

August 1. Grasshoppers begin to sing. Crickets arrived.
2. Larkspur (*Delphinium consolida*) in flower.
3. Sunflower (*Helianthus annuus*) and pigweed (*Chenopodium album*) in flower.
6. Broom-corn (*Sorghum saccharatum*) and lavender (*Lavendula spica*) in flower.
7. Early jenneting apples ripe. *Ambrosia trifida* and American senna (*Cassia marylandica*) in flower.
11. Muskmelon ripe. Garden squashes and shelled beans fit for the table.
13. Seed-box (*Ludwigia alternifolia*) in flower. Garden gooseberries fully ripe.
14. Our farmers gathering their peas and oats—an indifferent crop. Weather warm and dry.
16. Martins departing. Bush clover (*Lespedeza capitata*) in flower.
18. Our farmers beginning to mow their second crop of hay. Jerusalem oak (*Chenopodium botrys*) in flower.

20. Houseleek (*Sempervivum tectorum*) in flower.
21. Herb clarry (*Salvia sclarea*) in blossom.
22. Swallows collecting in thousands to depart. Toothed coral (*Cymbidium odontorhizom*) in flower. Saw bats for the first time this year.
24. Lopseed (*Phryma leptostachia*) and ladies' traces (*Neottia pubescens*) in flower.
27. Gay mallows (*Lavatera thuringiaca*) and *Solanum nigra* in full flower.
30. Burnet saxifrage (*Sanguisorba canadensis*) and water horehound (*Lycopus europœus*) in full flower.

STEPHEN W. WILLIAMS.

Deerfield, (*Mass.*) *Jan.* 25, 1819.

Art. VIII. *Description and Natural Classification of the Genus Floerkea, by* C. S. Rafinesque.

THIS genus was discovered in Pennsylvania, near Lancaster, by the Rev. Dr. Muhlenberg, who communicated the same to Wildenow of Berlin. This celebrated botanist ascertained that it was a new genus, to which he gave the name of a German botanist, (Floerke) and published it in the third volume of the transactions of the society *des Curieux de la Nature* of Berlin, for 1801, under the name of *Floerkea proserpinacoides*, which long and uncouth specific name has been changed by every subsequent author. Michaux has omitted it altogether, (with many more American species) in his *Flora Boreali Americana*, published in 1803. Persoon calls it *Floerkea lacustris*, in Syn. plant. 1. p. 393. Muhlenberg *Floerkea uliginosa*, in Cat. pl. Amer. Sept. p. 36. and Pursh, in Flora Amer. Sept. 1. p. 239, unites it with the genus *Nectris*, and calls it *Nectris pinnata*, putting it therefore in the Hexandria digynia of Linnæus, while all the preceding authors had classed it in the Hexandria monogynia. I will show presently which among them appear to be

wrong; but I must notice before, that no botanist had, I believe, endeavoured to class it naturally, until Mr. Correa de Serra, who in his *reduction of American genera to the natural families of Jussieu*, attempted, without having had an opportunity to see the plant, to place it in the family of *Junci*, taking it therefore to be a monocotyle plant; being led into this error by a mistaken idea, that all hexandrous plants must be monocotyle! But in the spring of 1816, I found this plant in the neighbourhood of Philadelphia, (near the falls of the Schuylkill) where it had escaped the attention of all the botanists of that city, and in particular of Dr. William Barton, who has therefore omitted it in his Prodr. fl. Philad. and having communicated it to Mr. Correa, he acknowledged that it was dicotyle, of which fact I was aware, even before seeing the plant and dissecting its seed, by attending to its habit.

The following exact description of this genus will enable the reader to ascertain how far I am correct in my presumptions towards its natural arrangement.

Floerkea. Perigone double persistent, sixpartite; the exterior calicinal 3 partile, sepals acute; the interior shorter, coloured 3 partile, sepals petaloid, oblong, obtuse. Six stamens perigyne, filaments filiform, of the length of the interior sepals, anthers round. One free ovarium, rounded and bilobed, one central and bifod style, two capitated stigmas. Fruit a bilobed atricule, tuberculated and bilocular dispermous, sometimes round, unilocular and monospermous by abortion of one lobe and cell. Seeds attached to the centre near the bottom, nearly lenticular, smooth albuminous, easily divided in two lobes. *Habit.* Small, delicate, annual, and glabrous plant, with alternate polytome pinnated leaves, flowers axillar, solitary, pedunculated.

Floerkea uliginosa. Caule tenello flaccido erecto simplex, foliis 4 petiolatis imis ternatis, summis pinnato, quinatis, pinnulis lineari oblongis obtusis, integris floribus axillaris, solitaris pedunculis longis apice incrastatis. Stem delicate, soft, upright, and simple, leaves petiolated, the inferior ternated, the superior pinnated, quinate, pinnules linear-oblong obtuse,

flowers axillar, solitary, and on long peduncles, swelled under the flower.

Among the several specific names given to this plant, I prefer Muhlenberg's, as it expresses exactly the kind of situations where it grows, say in moist grounds, occasionally swampish or overflowed; those I found near Philadelphia, grew by thousands on the banks of a small brook in a wood below the left side of the falls of Schuylkill. Persoon's name of *lacustris*, being wrong, as it would seem to imply that it grows in lakes only; and Wildenow's name being too long and illusive, its similarity of habit with the genus *Proserpinaca* not being very striking. However, even the name of *uliginosa* is liable to some slight objection; and did I think myself permitted to coin a new name, while so many have been proposed already, I should have called it either *F. tenella*, or *F. flaccida*, or *F. olitoria*, being a very delicate and tender plant, and very good to eat in sallad, as I have tried it myself, its taste is sweet and pleasant, the whole plant may be eaten, (even the root) being all juicy and tender: it grows in such an abundance in some spots, that it might occasionally afford a most precious and delightful sallad, but if cultivated for that purpose, it might be found an agreeable addition to our culinary herbs.

In addition to my above definition, it will be proper to state that the stem of this plant rises from 4 to 8 inches, it is cylindrical, smooth, and yellowish, the middle leaves are the largest, the lower peduncles are longer than the leaves, and the upper ones shorter, the petals or interior sepals, and the stamens are yellow. It blossoms in May, and is annual, it even lasts only three months.

It will be perceived that I do not agree with Mr. Pursh, in uniting this plant with the genus *Nectris*: he owns himself that it deviates a *little* from the generic character of *Nectris*, but these deviations appear to me very material; they exist in the pistils and fruits, the most essential parts of the flowers, since they agree in the perigone and stamens. The genus *Nectris* (or *Calomba* of Aublet) has *two ovaries, two styles, and two polispermous capsules*, or *achens!* and belongs therefore to the second order *Perimesia*, (class *Eltrogynia*) eighth family *Achen-*

opsia next to the genus *Myriophyllum*: while the genus Floerkea which has a *bilobed ovary, one central style, two stigmas,* and *one bilocular dispermous achen,* must belong to the eleventh order of the same class; *Isostimia,* which is characterized by having more than one stigma, the stamens in regular number, and not central; it will form a connecting link between this order and the foregoing *Polymesia,* by its affinity with many genera of the *Euphorbia's* tribe, such as *Callitriche, Tragia, Mercurialis,* &c. from which it differs merely by having hermaphrodite flowers, and perispheric regular stamens. It will at present stand nearly isolated in this order, where it may form the small family *Galenidia,* along with the genus *Galenia,* &c. and which shall have much affinity with the family *Phytolacia;* but this differs by having a multilocular berry, while the *Galenia* merely differs by having a 4 sided perigone, 8 stamens, and 2 styles.

I admit, however, that there is a strong affinity between the genera *Floerkea* and *Nectris,* but stronger affinities often exist in plants of different classes. If, however, it should happen that Aublet* might have been mistaken in describing the ovaries and capsules of the *Nectris* as double, if they should prove to be simple but bilobed, then the *Nectris* would belong to the same family as the *Floerkea;* but yet stand as a peculiar genus distinguished by having 2 styles, and the achens not monospermous!

It was insinuated to me by Mr. Correa, that the *Floerkea* might have some affinity with the tribe of *Ranunculaceous,* but I cannot discover any, since that tribe is widely different, by having many ovaries, stamens, and fruits, each ovary with 1 style or stigma, a deciduous perigone, the anthers adnate, &c. The analogy in the structure of the seed and habit, is too slight to be taken in consideration.

* Mr. Stephen Elliott has confirmed the description of Aublet, in his Botany of the Southern States. (Received January, 1818. *Editor.*)

ART. IX. *Descriptions of Three New Genera of Plants, from the State of New-York. Cylactis, Nemopanthus, and Polanisia, by* C. S. RAFINESQUE.

1. *N. G. Cylactis.*

CALYX campanulated 6 to 10 fidus, sepals a little unequal. Petals 4 to 6 equal. Many perigynous stamens. Pistils 8 to 12, ovaries sessile ovate, styles elongated, stigmas capitated. Berries few, distinct, one seeded.

This new genus belongs in the analytical and natural method, (see Analysis of Nature) to the first natural class *Eltrogynia*, first natural order *Rhodanthia*, second natural family *Senticosia*, next to the genera *Rubus*, *Oligacis*, &c. It would range itself into the artificial class *Icosandria* of the Linnæan sexual system; but not properly into any of its orders, since the number of pistils is variable, and never above 12. Only one species belongs to it, which I have discovered in company with Mr. Knevels, on the Catskill mountains. The etymology of the name derives from two Greek words meaning *radiated calyx*. It differs essentially from *Rubus* by the unequal many cleft calyx, variable petals, and few pistils.

Cylactis montana. Mountain cylactis—Stem herbaceous upright, unarmed, pubescent; leaves quinate, nearly smooth, upper ones sessile, stipules oblong, folioles ovate acuminate, incised, serrated, ciliated, base acute, entire, the middle one petiolated: flowers few corymbose, peduncles erect elongated bracteolated; calyx pubescent, sepals lanceolate acute, nerved, reflexed; petals cuneate-obovate, longer than the calyx.

It is a small perennial plant, rising about half a foot; flowers white, blossoming in June. On the Catskill mountains near the great falls, &c.

2. *N. G. Nemopanthus.*

Dioical. M. flowers calyx 5 phylle, equal, deciduous. No corolla. Stamina 5 hypogynous, alternating with the calyx.

Fem. fl. calyx deciduous 5 phylle ? Ovary ovate, stigma sessile 4 lobed. Berry 4 celled 4 seeded.

The name means *flower with a filiform peduncle.* A shrub forms this genus, which had perhaps been united with *ilex* by Michaux, &c.; but it differs altogether from it by the want of corolla, hypogynous stamens, sessile, style, &c. it does not even belong to the same family, but to the natural family *Rhamnidia*, natural order *Plynontia*, and natural class *Eltrogynia*, next to the genus *frangula*. In the sexual system it would belong to *Dioecia pentandria*, very far apart from *Frangula*.

Nemopanthus fascicularis. Fascicled nemopanthus. Shrubby, leaves fasciculated, petiolate, oblong, mucronate, entire, rather undulated, membranaceous, smooth; flowers axillary fasciculated, peduncles filiform, shorter than the leaves.

It forms a small shrub from 5 to 8 feet high, covered with gray bark, and with slender upright branches; the flowers are greenish, very small, the female flowers have shorter and thicker peduncles; they blossom in June. It grows on the Catskill mountains near the two lakes. It is, perhaps, the *Ilex canadensis ?* of Michaux and Pursh. And it has some analogy with the *Frangula alnifolia.*

3. *N. G. Polanisia.*

Calyx 4 phylle, phylles coloured unequal, the upper one unguiculated spatulated. Corolla with 4 unequal petals, the two upper ones larger and unguiculated. A nectarium upwards glandular, broad, and truncated. Stamina 9 to 14, unequal, erect, hypogynous. Ovary oblong on a short pedicel, one style, one truncated stigma. Fruit a follicular capsule, one celled, two valved, many seeded, seeds inserted on each side of each suture, nearly snail-shaped.

The type of this genus is the *Cleome dodecandra* of Linnæus, under which denomination many species were blended, which have no similitude with the real genus *cleome*, differing in the calyx, corolla, nectarium, stamina, and fruit. I shall describe here that of North America, where 2 or 3 species exist, besides those of the West Indies, Africa, and Asia, which are totally

different. The etymology of the name which I have given to it, derives from *many irregularities*. It belongs in the analytical method of botany, to the first natural class *Eltrogynia*, ninth natural order *Monostimia*, natural family *Capparidia*. It can find no place in the sexual system since the number of stamina varies from 9 to 14, unless it be forced into *Dodecandria*.

Polanisia graveolens. Clammy polanisia—hairy and glutinous all over, stem upright, leaves alternate, petiolate, ternated, folioles sessile, the intermediate longest, oblong, obtuse, entire, hairy on the margin and nerves: flowers racemose erect, bracteas petiolate, ovate, obtuse, calyx hairy, petals emarginate, crenate, capsules divaricate glutinous.

It is the *Cleome dodecandra* of Michaux and Pursh. It grows on the banks of rivers and lakes, on the Hudson near Newburgh, on the Susquehannah near Harrisburg, on Lake Erie, on the Ohio, and Mississippi, &c. It blossoms in July and August, the stem rises about 1 foot, the petals are white, or slightly red. The whole plant has a strong graveolent smell, similar to that of *Erigeron graveolens*. (Received January, 1818. *Editor*.)

Art. X. *Notice on the Myosurus Shortii.*

I HAVE the pleasure to announce to the botanists, that the genus *Myosurus*, hitherto thought an European genus, and composed of a single species, has been detected in the United States by Dr. Short of Kentucky, who has discovered it in the neighbourhood of Hopkinsville, in Christian county, West Kentucky, and has communicated me specimens of it; by which, on comparing them with the European *Myosurus*, figured in *Flora Danica*, Lamarck's Illustrations, &c. I have been enabled to ascertain, that the American plant must form a second species of that genus, which I have accordingly dedicated it to the discoverer, by naming it *Myosurus Shortii*. This adds another genus and another new species to our Flora. I add the comparative definitions of the two species, exhibiting their different characters and diagnosis.

Myosurus minimus. Lin. &c.

Leaves linear-cuneate, broader near the top, and acute. Scapes as long as the leaves, thickened towards the upper part. Calix 5 leaved, spurs consimilar: petals 5. Stamens 5 to 8. Carpophore as long as the scapes.

Myosurus Shortii. Raf.

Leaves linear obtuse, hardly attenuated below. Scapes shorter than the leaves, and filiform. Calix 3 to 5 leaved, spurs membraneous: petals 3 to 5. Stamens 10 to 12. Carpophore shorter than the scapes.

C. S. RAFINESQUE.

Philadelphia, May 1, 1819.

Art. XI. *Description of a New Species of Gnaphalium, by Professor* E. Ives.

To B. Silliman, Esq. M.D., &c.

THE following description of a new species of Gnaphalium, accompanied with a drawing, has been in my possession for two years. If the subsequent observations will be of use to correct error, or solve doubts which may have existed concerning some species of gnaphalium, they are at your service.

E. IVES.

This plant was first observed by me, in company with Mr. C. Whitlow, in July, 1817, by the margin of a brook, a few rods north of Mr. E. Whitney's gun manufactory, near New-Haven. It is also found on the margin of the Housatonick, about thirty miles from Long Island sound, where it was observed by Dr. Alfred Monson, the last summer. Specimens of this plant were sent to Z. Collins, Esq. of Philadelphia, for the purpose of comparing it with the species of gnaphalium in Muhlenberg's herbarium, more particularly with the *luteo-album* and *Pennsylvanicum*, which I had not seen.



Gnaphalium decurrens.

A. Doolittle sc

I am indebted to the politeness of Mr. Collins, for the facts on this subject relative to Muhlenberg's herbarium. He observes, "your Gnaphalium is certainly not the *luteo-album* of Muhlenberg, which may not strictly be a native, but introduced. Yours most approaches G. polycephalum Mx. Still, from the decurrent leaves and other differential marks, it appears to me to be a new species. Muhlenberg's collection has it not."

As the *luteo-album* is said to grow in New-England, yet so far as my observation has extended it has not been found by any of the botanists, I am induced to believe that this opinion has arisen from some erroneous description of the plant which is the subject of this paper.

As the decurrent leaves of this Gnaphalium distinguish it so obviously from all the other American species of Gnaphalium, I propose to give it the specific name of *decurrens*.

Specific description of Gnaphalium Decurrens (large life everlasting.)

Leaves lanceolate, broad at base, acute, decurrent, somewhat scabrous above, tomentose beneath; stem leafy branched spreading, about three feet high.—See the plate.—The plate represents a section of the upper part of the plant.

FOSSIL ZOOLOGY, &c.

ART. XII. *Observations on some Species of Zoophytes, Shells, &c. principally Fossil, by* THOMAS SAY.

IF the following descriptions and notices of some of the animal productions of our country, chiefly fossil, and of which some are but little known, should be found of sufficient interest to occupy a place in the Journal of Science, they are very much at your service for that work.

The greater portion of them are extracted, with some modification, from an essay which I read about three years ago, to the Academy of Natural Sciences, without any intention at the time of giving publicity to them. But the rapid diffusion of a taste for geological research, seems to require corresponding exertions on the part of those who have attended to fossil remains, inasmuch as geology, in order to be eminently furnished with every advantage that may tend to the developement of many important results, must be in part founded on a knowledge of the different genera and species of reliquiæ, which the various accessible strata of the earth present. The accessory value of this species of knowledge, is now duly estimated in Europe, as affording the most obvious means of estimating, with the greatest approximation to truth, the comparative antiquity of formations, and of strata, as well as of identifying those with each other which are in their nature similar.

Certainly very little is yet known about the fossils of North America, and very little can be known accurately, until we shall have it in our power to compare them with approved detailed descriptions, plates, or specimens of those of Europe; which have been made known to the world by the indefatigable industry, and scientific research of Lamarck and other naturalists.

America is rich in fossils. In many districts of the United States, vast beds of fossil shells, zoophytes, &c. are deposited, which, for the most part, are concealed from the inquiring eye, offering superficially a mere confused mass of mutilated fragments. These rich repositories must finally be exposed to view, by the onward pace of improvement, and the more interior strata will be unveiled by some fortunate profound excavations, the result of enterprise in the pursuit of gain. The very surface of the country in many regions, is almost overspread with the abundance of casts, or redintigrate fossils, many of which are apparently specifically anomalous, and some generically so. The correct, and only useful mode in which the investigation of our fossils can be conducted, is attended with some difficulty and labour.

The task presumes the knowledge, not only of fossils in all their different states, from the apparently unchanged specimen, to the fragment or section of a cast uninsulably imbedded in its rocky matrix, but it also requires an adequate acquaintance with recent specimens, or those of which the inhabitants are not yet struck from the list of animated beings, in other words those of the present, as well as those of the former world.

Due advantage being taken of the many opportunities which are from time to time offered to us, of obtaining knowledge in this department, will probably be the means of producing a list of American animal reliquiæ, coextensive with that of Europe at the present day. In the present state of the science, however, the correct naturalist will feel it a duty which he owes to his colaborators to proceed with the utmost caution, that he may not add unnecessarily to the already numerous species.

Genus Alveolites, Lam.

Coral lapideous, covering extraneous bodies, or in a simple mass, formed of concentric strata; strata composed each of a union of numerous alveoles, which are very short, contiguous, reticulate, and generally parallel.

Species.

A. glomeratus, alveoles vertical, subequal, oval, or obsoletely hexagonal, much shorter than the diameter, parallel; paries simple; strata numerous, forming a rounded mass. (*Cabinet of the Academy of Natural Sciences.*)

Found often on the coast of North America, cast up by the waves, the animals sometimes still living. Forms masses of various sizes and figures, generally more or less rounded or lobed, and composed of a great number of concentric layers. The number of these strata seems to be regulated in some degree, by the quantity of surface they have to cover. Thus if the nucleus happens to be a small shell, such as the *Naticæ*, *Nassæ*, &c. of our coast, or even the oyster, (*O. virginica*,) clam, (*V. mercenaria*,) &c. the strata are often very numerous;

but on the thoracic plate of *Limulus polyphemus*, having a considerable space over which to extend themselves, the strata are but few, not more than 2 or 3. I have seen the thoracic plate of this animal so entirely covered by the *Alveolite*, as to have the eyes and stemmata concealed so as to be perfectly blind. When composed of a single layer only, it much resembles a *Flustra*, or a *Cellapore* of which the convex surfaces have been removed by attrition. The animal I have not yet examined. The alveoles or cells of a layer, are arranged in lines of different degrees of curvature, obscurely radiating from different centres; these lines are placed side by side, the alveoles alternating with each other throughout the layer in a quincunx manner; the thickness of the paries is somewhat equal to one half of the conjugate diameter of the alveole, the length of which, or thickness of the layer, is scarcely more considerable; but these proportions vary.

The species to which it seems allied, are *madreporacea* and *incrustans*. The former is fossil, and differs in being subramose; the latter forms but a single expansion.

Genus Favosites, Lam.

Coral lapideous, simple, of a variable form, composed of parallel prismatic and fasciculated tubes; tubes contiguous, pentagonal, or hexagonal, more or less angular, rarely articulated.

Species.

F. striata, more or less turbinate; *paries of the alveoles* longitudinally striated within, and fenestrate with minute osculi; *alveoles* with very numerous septæ. (*Cabinet Acad. Nat. Sciences; and Peale's Museum—common.*)

Found fossil in various parts of the United States, at the falls of the Ohio; Genessee, New-York; Pittsburg and Wilksbarre, Pennsylvania; Missouri, &c. &c. but not yet in the alluvial deposit of New-Jersey.

The tubes are generally, partially, or entirely filled with silicious matter, sometimes so completely so, as to resemble

in miniature, basaltic columns; when the alveoles are free on the surface, these fossils are known by the name of *petrified wasp-nests*, from the resemblance they bear to the nests of those insects. The silex is usually only infiltrated into the cavities, leaving the substance of the coral in its original calcareous state, but the specimens which are found amongst the rolled pebbles of the Delaware River, near Philadelphia, are completely silicified.

The size varies from one fourth of an ounce, to two hundred pounds or more, and the tubes occur of every intermediate diameter, from the fortieth to one fourth of an inch. It is not common to find any two specimens of like form, they are, however, ordinarily more or less turbinate, but are sometimes depressed or compressed, and the tubes rectilinear or excurved, and of various lengths. The dilated summit is not so much the effect of a gradual enlargement of the tubes, as of the frequent and adventitious interposition of young ones, which of course renders the openings of the tubes unequal. The tubes or alveoles, vary in the same coral, being 5 or 6, rarely seven sided, but the hexagonal form is most common; the interior of a tube is divided into a great number of apartments or cells, by approximate transverse septæ, each of the cells appears to be connected with the corresponding cells of the surrounding tubes, by lateral orifices in the dividing paries; these orifices are minute, inequidistant, orbicular, their margins slightly prominent, and forming from one to three longitudinal series on each side of the tube; each row is separated from the adjoining one by an impressed line. By means of these osculi it seems probable that all the animals inhabiting a common coral, were connected together, or had free communication with each other, but whether by means of a common organ as in *Pyrosoma*, *Stephanomia*, &c. or simply by contact as in the aggregating *Salpa*, &c. we have no means of determining.

The *striata* differs from *Madrepora truncata*, Esper. (*F. alveolata*, Lam.) in not being "extùs transversè sulcata." It seems to be allied to *Corallium Gothlandicum*, Amœn. Acad. v. 1. p. 106, and it is possible it may prove synonymous, or very similar to it, when that species becomes better known;

the latter has been taken for Basalt, and M. Lamarck when describing it, inquires "Est-ce un polypier?" *Madrepora fascicularis*, of Volck. and Parkin. in common with *F. striata* and *F. Gothlandicum*, is distinguished by the transverse septa, a character which induced me to refer the species here described to *Favosite;* they seem therefore to be congeneric, as analogy indicates a participation in the character of osculated paries.

Amongst the great variety exhibited by this species, we have to remark more particularly the following, viz.:

1st. Alveoles perfectly free, that is, destitute of aciculi or lamellæ, the septa wanting, and sometimes the osculi obsolete.

2d. Alveoles filled almost to the summit with the septa, and resembling those combs of the bee-hive which are filled with honey and covered over.

3d. Paries beset with very numerous, interrupted, alternating, transverse lamellæ, which are denticulated at their tips, and project towards the centre with various degrees of prominence and irregularity.

The first variety corresponds with the generic character, and the third approaches the genus *Porites;* yet so unequivocally identical are they, that I have seen them all united in the same mass, and perforated throughout by the osculi. The identity is further obvious by the perfect gradation which renders them inseparable.

With respect to the transverse septa, I think their presence may be accounted for by supposing that as the animal elongates its tube in consequence of an increase of growth, or in order to maintain an equal elevation with the adjacent tubes, (rendered necessary by the origin of young tubes in the interstices) it gradually vacates the basal portions of its tube, and sustains itself at the different elevations, by successively uniting the parietal lamellæ so as to exclude the vacuity. That this is probable, we may infer from a similar procedure on the part of several species of testaceous mollusca. Thus some Linnæan *Serpula* become camerated, and a familiar instance presents itself in the *Triton tritonis*, the animal of which adds suc-

cessive partitions to the interior of the spire, as that part becomes too strait for the increasing volume of its body. If the above supposition proves correct, the organs of communication which pass through the osculi, can hardly be in common, but must rather connect the animals by simple contact only, otherwise these parts would be broken when the animal changes its place by vacating the inferior part of the tube.

The third variety is then the state of that portion of the tube which is inhabited by the body of the animal, and not yet interrupted by the septæ.

From the above observations, it is evident that this species, and probably the entire genus *Favosite* under which I have placed it, will not arrange properly with the *Tubipores*, *Millepores*, &c. but must be transferred to the *Polypiers Lamellifères* of Lamarck. And if the *Madrepora retepora* of Solander and Ellis, is a true *Porites*, as M. Lamarck supposes it to be from the appearance of its tubes, I should conclude this genus to be very proximately allied to *Favosites*, by that species and the *F. striata* having in common the remarkable character of fenestrated paries. But to this character I should conceive a generic importance ought to be attached, as indicating a differential organization of the artificers. I have no doubt that on close inspection of a perfect specimen, the same character will be found to exist in *F. Gothlandicum*, and possibly also in *F. truncata*, if not in the latter only, it may be proper to separate the genus and to withdraw from *Porites* the forementioned species, retaining to *striata* as specifically essential, the second member of the differential description.

(*To be continued.*)

PHYSICS, CHEMISTRY, &c.

ART. XIII. *Observations on Salt Storms, and the Influence of Salt and Saline Air upon Animal and Vegetable Life. Read before the Lyceum of Natural History of New-York, March* 7, 1819, *by* JOHN B. BECK, M. D.

(Communicated for this Journal.)

METEOROLOGY is a science of so much general concern, that it seems to be incumbent upon every member of society to aid in augmenting the stock of facts, which the labours of ingenious and scientific men have already accumulated on that subject. Under this impression I propose to devote the following paper to some observations on *salt winds* or *storms*, as they have occurred in this country and in Europe—a subject, which although presenting many phenomena of a more than temporary interest, has as yet excited but little attention. Indeed, the opportunities for observation have occurred so rarely as readily to account for its having in a great measure escaped the philosophical acumen of the present age.

It must have been early observed that the atmosphere in the vicinity of the sea frequently becomes impregnated with saline materials; but the first and only account of a *salt storm* that I have met with, is to be found in the Transactions of the Linnæan Society of London. The 8th volume of that work gives an interesting narration of the effects of a storm of this description, which occurred in England, in January, 1803. It was occasioned by an east wind, which blew for some days, and which, in its passage over the ocean, had imbibed large quantities of salt water, which were afterward deposited upon the land. In most cases these depositions proved fatal to the plants and vegetables which received them. So extensive

were the effects of this singular storm, that they were felt in the vicinity of London, at a distance of about seventy miles from the ocean, and in all the intermediate country. In most instances, the leaves of the plants, which suffered from it, appeared as if they had been scorched, and in some places even the tops of the branches mortified. A storm of the same kind took place in England, in February, 1804; and the memoir states, that Sir Joseph Banks had noticed another some years before in Lincolnshire.*

A storm attended with similar effects occurred in this country in 1815, and vented its fury upon the eastern states. It commenced on the 23d of September, between eight and nine o'clock, A. M. with the wind from the east. In about two hours the wind shifted to southeast, and blew a perfect hurricane. The extended devastation which ensued, is still in the recollection of every person. The tides rose from nine to twelve feet higher than ordinary, and in many of the principal cities and towns along the coast of New England, churches, houses, bridges, wharves, and in some instances valuable citizens, were buried in one common ruin. In less than three hours the gale abated, and before sunset there was a perfect calm. Such were the more striking features of this tremendous gale—but other effects were observed more peculiarly interesting to the philosopher. At New-London, Salem, and other places, both on the coast, and several miles in the interior, the air was found to be loaded with salt; and the leaves of many trees appeared, a few hours after the storm, as if they had been scorched. Besides this effect upon vegetables, there were additional evidences of the saline quality of the wind. At Salem and some other places an incrustation of salt was perceived on the windows, and the fruit in several gardens had a perceptible taste of salt on their surface. At New-London it was remarked that the air in the eddies was extremely hot and suffocating.

* I refer the scientific reader for further particulars to "*An account of a storm of Salt*, which fell in January, 1803. By Richard Salisbury, F.R.S. L.S." in the Transactions of the Linnæan Society of London. Vol. VIII. p. 207—10.

Other facts of a similar nature might be collected, but these it is presumed are sufficient to characterize the state of the atmosphere during that storm.

Several interesting inquiries arise from the consideration of the foregoing facts.

1. In what way does the salt exist in the atmosphere in these storms? On this point there are two different opinions. The most prevalent is, that it is merely the spray of the sea driven onward by the force of the wind. This opinion has received the sanction of Sir Joseph Banks,* and also of Sir Humphry Davy, if we may judge from an incidental expression in his Agricultural Chemistry.† Another opinion‡ is, that muriate of soda is continually rising into the atmosphere from the surface of the ocean, and that the air, in all maritime situations, is thus constantly more or less impregnated with salt. The most striking fact in support of this doctrine, (so opposite to the commonly received views on the subject of the evaporation of sea water) is the actual existence of muriate of soda in the rain and snow which fall in the vicinity of the ocean.§ The experiments of Vogel and Bouillon Lagrange, on the distillation of sea water, are also in favour of the position, that salt may be carried into the air in the ordinary process of evaporation. On distilling salt water they found a considerable quantity of muriate of soda in the receiver.‖

* Linnæan Transactions. Vol. VIII. p. 289. † P. 339. Lond. ed.

‡ Maintained by Dr. Mitchill.

§ My friend, Dr. John Torrey, has favoured me with the following results of some experiments, which he made at my request upon the last snow which fell. "A pint and a half of snow water was reduced by evaporation to a few drops. On testing this with vegetable blue infusions no alteration of colour took place. It was afterward evaporated to dryness, and about a quarter of a grain of a solid residuum was obtained. This was redissolved in a small quantity of pure rain water, and prussiate of potash added to it, without occasioning any precipitate. Nitrate of silver produced a white precipitate so copious, that the solution was thick with it. Carbonate of soda produced no effect. The transparency of a solution of muriate of barytes was not disturbed by it. These experiments prove, that a *free acid* does not exist in snow water, but that the muriate exists in it combined with an alkali, which is most probably soda."

‖ Mr. J. Murray, of London, considers this to be a mistake. *Free muriatic acid*, and not muriate of soda, he says, will be found in the recipient.—*Elements of Chemistry.* Part I. p. 212. Lond. ed. 1818.

Admitting the correctness of these experiments, still it is not easy to conceive, how they will account satisfactorily for the *large* quantities of salt found in the air during the storms under consideration.

Whichever of these solutions may be adopted, it is unquestionably a fact that salt does, in some way or other, exist in the atmosphere in the neighbourhood of the sea.

2. The next object of inquiry is, the influence which this saline air has upon vegetable life. Independently of the facts already stated, there are many others which prove its deleterious agency upon the vegetable creation. Dr. Mitchill informs me, that in some parts of the south side of Long-Island fruit trees do not thrive well, except at a distance of thirty miles from the sea, and even the sturdy oak does not extend its branches towards the ocean.* If I am correctly informed, it was with great difficulty, that the trees on our Battery were made to accommodate themselves to a situation so near the salt water. It is also well known, that when plants are taken to sea, they speedily perish, if exposed but a short time to a wind, which is sufficiently strong to turn over the tops of the waves into *white caps*, as they are called by the sailors.

In order to ascertain positively, whether these effects were to be attributed to the operation of salt, I made a solution of muriate of soda in common rain water; with this I watered for a couple of days the leaves of different plants. In a short time they began to dry up, and in a few days were completely dead.

It appears from Volney, that the Egyptian air is strongly charged with salts. The evidences of it are to be found even at Cairo.† It is this property of the air, which this philosophical traveller considers, as one of the causes of the rapid vegetation in that country. He mentions, however, that *exotic* plants will not thrive there. It is found necessary to renew the seeds of them every year. May not this be occasioned

* That is, in those oaks which grow near the salt water, the branches that directly face the sea do not attain so great size and strength as those on the opposite side; this has also been observed on the south side of Long-Island.

† Volney's Travels in Syria and Egypt. Vol. I. p. 48. Perth ed.

by the saline quality of the air? The *native* plants are doubtless accustomed to its action, and do not so sensibly feel its injurious effects. And if the Egyptian air is so very penetrating from this very cause, as to produce ophthalmia, may we not rationally conclude, that its influence must be equally injurious to plants not accustomed to it.

Another illustration of the influence of salt on vegetation is to be found in the *Dead Sea*, or *Lake Asphaltites*. "In Lake Asphaltites," says Volney, "there is neither animal nor vegetable life. No verdure is to be seen on its banks, nor fish to be found within its waters; but it is not true, that its exhalations are pestiferous, so as to destroy birds flying over it. It is not uncommon to see swallows skimming its surface, and dipping for the water necessary to build their nests. The *true* cause which deprives it of vegetables and animals is the extreme saltness of the water, which is vastly stronger than that of the sea. The soil around it, equally impregnated with this salt, produces no plants, and the *air* itself, which becomes loaded with it from evaporation, and which receives also the sulphureous and bituminous vapours, cannot be favourable to vegetation; hence the deadly aspect which reigns around this lake."*

3. In what way does the salt operate in producing its deleterious effects on the leaves of vegetables? It is by no means easy to answer this question. It cannot be by shutting up the pores of the leaf, and thus obstructing its perspiration. It is well known that when the surfaces of leaves are covered with oil, they will soon die.† But salt water is certainly not sufficiently viscid to act in a similar way.

Nor can it be satisfactorily attributed to the difference of structure between maritime and land plants. There is some difference indeed between many of these, maritime plants being generally covered by a pubescence, of which most land plants are destitute. It is idle however to suppose that the object of this covering is to protect maritime plants from the action of the salt air, as there are many of them

* Volney's Travels in Syria and Egypt. Vol. I. p. 217.

† Darwin's Botanic Garden. P. 256.

which do not possess it. Besides, is it not rational to conclude, from the large quantities of soda which are always found in sea plants, that this saline atmosphere is rather propitious than otherwise to their growth, and that it only proves injurious to plants accustomed to the unadulterated air of the land.

Again, I do not think that it can be explained by supposing, that the salt is absorbed into the plant, and thus acts as a poisonous substance. We know, that in land plants which are cultivated in the neighbourhood of the sea, salt is absorbed through their roots.* It must of course circulate with the juices through the whole plant; and yet in these cases the leaves are not destroyed by it.

The most plausible method of explaining it appears to be this: that the salt, by its irritating or corrosive power, destroys the small vessels in the leaf which are necessary for the circulation going on in it during health.

Dr. Darwin has ingeniously shown the analogy between the functions of the leaves of plants, and the lungs of animals. If this be admitted, it will not be difficult to account for the action of salt upon leaves. This substance, when taken into the stomach, proves not merely innocuous, but wholesome; but when accidentally introduced into the lungs, irritation, inflammation, and death are the consequences. So with plants—when admitted into them in combination with their juices, it may be harmless; but when applied to the lungs or leaves, death ensues.

* To prove that salt is absorbed into land plants growing near the sea, the following facts, for which I am indebted to my friend, Dr. D. V. Knevels, are conclusive. The fruit of those cocoa-nut trees which grow near the seashore in the West-Indies is generally found to have a saltish taste; and even the milk in the nut is perceptibly impregnated with it. Those trees on the contrary which grow in the interior, beyond the influence of salt water, have their fruit perfectly fresh and sweet.

The same gentleman informs me, that in a plantation of his father's, in the West-Indies, situated on the seashore, a whole crop of the cane was rendered unfit for the purpose of making sugar, in consequence of the great quantity of salt which it had imbibed.

4. I shall devote the remainder of this paper to a few concise observations on the effects of salt, and a saline atmosphere, upon *animal* life.

Upon the more imperfect animals, such as slugs, worms, toads, &c. it is well known that salt proves speedily destructive of life. It is not my intention to attempt an explanation of this singular fact. But it is remarkable that it should not have been turned to better account in the treatment of those worms, which infest the human body. Although used for that purpose by the common people in Ireland as well as in this country, I believe it has not, until very lately, claimed the attention of the profession, as an anthelmintick. A late English journal * contains a notice of some cases which satisfactorily prove its efficacy, when administered with this intention. This fact, in addition to numerous others, strikingly illustrates the advantages which the healing art might derive from a careful observation of the phenomena daily developed by the collateral sciences.

In cases of *hæmoptysis* and *hæmatemesis*, common salt has been used with decided success. The public is indebted to Dr. Rush, for the introduction of this remedy into general practice.

Dr. Hosack informs me, that he has found sea air extremely salutary in *remittent fever*, *cholera infantum*, and *dyspepsia*.

Among the deleterious effects caused by a *saline atmosphere*, may be mentioned the *ophthalmia* of Egypt. This disease is so common there, "that out of a hundred persons," says Volney, "I have met while walking the streets of Cairo, twenty have been quite blind, ten wanting an eye, and twenty others have had their eyes red, purulent, or blemished."† Throughout the Delta, and at Cairo, this complaint is more prevalent than in any other part of Egypt. In Syria it is also common, although less so than in Egypt, but it is only met with on the *sea-coast*. The reasoning of Volney on this subject, is decisive of the position, that the prevalence of this complaint, in these

* Journal of Science and the Arts. No. X.

† Volney's Travels in Syria and Egypt, Vol. I. p. 167.

regions, is owing to their proximity to the ocean. In confirmation, he states that he has himself experienced the irritating effects of the air of the Delta upon the organ of vision.*

In those cases of *scurvy* which occur in long voyages, the saline nature of the atmosphere co-operates very powerfully with salt provisions and bad water, in producing that general vitiation of the system which characterizes this disorder.

Of all diseases, however, those of the lungs appear to be most affected by a saline air. I have known a lady of this city who had been afflicted for many years with *asthma*, to be essentially benefited by a voyage across the Atlantic. Another case has fallen under my observation, of a lady troubled with asthma, being much relieved by removing from the interior to this city. What proves beyond a doubt that her relief is owing to the air she breathes, is, that whenever she takes a jaunt into the country, she is sure to suffer a paroxysm of her old complaint.

Pulmonary consumption certainly prevails more on the sea-coast, than in the interior. In all our sea-port towns, it is this disorder which so frightfully augments the catalogue of our bills of mortality. According to Dr. Rush, "in Salem, in the state of Massachusetts, which is situated near the sea, and exposed, during many months of the year, to a moist east wind, there died in the year 1799, 160 persons; fifty-three of whom died of the consumption."† In Philadelphia, which is more remote from the sea, the deaths from consumption are much less numerous than in New-York, or the other cities immediately on the coast. In Great Britain, which is exposed to the sea on all sides, it is calculated that about 55,000 die annually from this disease.

* On the subject of the Egyptian ophthalmia, it may be asked "why it does not appear in innumerable other situations, equally exposed to salt air, as Cape Cod, and the West-India Islands?" To this it may be replied, that in the production of any disease whatever, a *predisposing* state of the system is as necessary as an *exciting* cause. This predisposition appears to exist in a great degree among the Egyptians, and depends upon the nature of their climate, their habits, and mode of living, all of which have a tendency to produce *debility* of the eyes, and thus render them more susceptible of the impression of those causes which excite inflammation.

† Rush's Medical Observations and Inquiries, Vol. II. p. 132.

Such are some of the facts on this subject; but the conclusion does not appear to be warranted, that these pulmonary affections arise from the irritating quality of the air. In Holland, the West Indies, as well as in other countries and islands, exposed to the sea air, consumption is of rare occurrence. In Syria, Volney even states that the air of the coast is particularly favourable to those labouring under this malady. Accordingly they are in the habit of sending such patients from Aleppo to Latakia, or Saide, where they may enjoy the benefit of sea air.*

Again, we know that many persons suffering from this affection, have been completely cured by a voyage, after all the resources of medicine had been exhausted upon them in vain.

It is evident then, that a *pure* sea air is not detrimental in cases of consumption. Dr. Rush, with his usual ingenuity, explains the prevalence of this complaint in our sea-ports, by attributing it to the mixture of land and sea air; and in confirmation observes, that "those situations which are in the neighbourhood of bays and rivers, where the fresh and salt waters mix their streams together, are more unfavourable to consumptive patients than the seashore, and therefore should be more carefully avoided by them in exchanging city for country air."†

Independently, however, of these causes, I think the frequent and sudden vicissitudes of temperature, which we suffer on the coast, are alone sufficient to account for the prevalence of catarrhal and pneumonic affections, which most commonly are the precursors of consumption.

I trust the foregoing observations have not been considered too *medical* to comport with the objects of this Society. Natural history is only useful in its practical applications; and if it can be shown to throw any light upon an art, which contributes so much to the comfort and happiness of man, we have established one of the strongest considerations, which can recommend it to general patronage and investigation. Physicians ought in an especial manner to set a high value upon the researches of naturalists. The aid they have already given

* Volney's Travels, Vol. I. p. 226.

† Rush's Observations and Inquiries, Vol. II. p. 133.

is sufficient to entitle them to the lasting gratitude of our profession. It was one of the merits of that illustrious physician of our own time and country, Dr. Rush, that he seized with avidity every fact, from whatever quarter it might be drawn, to elucidate his favourite science. If ever medicine shall attain to the elevation of a truly *philosophical science*, it must be accomplished, in part at least, by imitating his example, and by developing the infinite and diversified associations which exist between it and the other sciences.

Art. XIV. *Thoughts on Atmospheric Dust. By* C. S. Rafinesque, *Esq.*

1. "WHEN we find the ruins of ancient cities buried under ground; when the plough uncovers the front of palaces and the summit of old temples, we are astonished: but we seldom reflect why they are hidden in the earth. A sort of imperceptible dust falls at all times from the atmosphere, and it has covered them during ages."

2. These are the words of the worthy and eloquent philosopher Virey, in his article Nature, Vol. XV. p. 373, of the French Dictionary of Natural History. Even before reading them I had observed the same phenomenon, and I have since studied their effects in various places. I could quote one thousand instances of the extensive and multifarious operations of this meteoric dust: but I mean to give the results merely of those that fall daily under notice, and are yet totally neglected; wishing to draw on them the attention of chemists, philosophers, and geologists.

3. Whenever the sun shines in a dark room, its beams display a crowd of lucid dusty molecules of various shapes, which were before invisible as the air in which they swim, but did exist nevertheless. These form the atmospheric dust; existing every where in the lower strata of our atmosphere. I have observed it on the top of the highest moun-

tains, on Mount Etna, in Sicily, on the Alps, on the Alleghany and Catskill mountains in America, &c. and on the ocean.

4. It deserves to be considered under many views: which are its invisibility, its shape and size, its formation and origin, its motion, its deposition and accumulation, its composition, its uses, and its properties.

5. This dust is invisible, owing to the tenuity of its particles, but they become visible in the following instances; when the sun shines on them, since they reflect the light, when their size is increased, and when they are accumulated any where.

6. The size of the particles is very unequal, and their shape dissimilar; the greatest portion are exceedingly small, similar to a whitish or grayish spark, without any determinable or perceptible shape; the larger particles are commonly lamellar or flattened, but with an irregular margin, and the largest appear to be lengthened or filiform; the gray colour prevails. Other shapes are now and then perceptible with the microscope.

7. Among the properties of atmospheric dust are those of being soft, as light as atmospheric air, of reflecting the rays received directly from the sun, of possessing a kind of peculiar electricity, which gives it a tendency to accumulate on some bodies more readily than on some others, and of forming an earthy sediment, which does not become effervescent with acids.

8. This dust is either constantly or periodically formed, but chemically in the atmosphere like snow, hail, meteoric stones, honey-dew, earthy rains, &c. by the combination of gaseous and elementary particles dissolved in the air. Its analysis has never been attempted by chemists; but the earthy sediment which is the result of its accumulated deposition, proves that it is a compound of earthy particles in a peculiar state of aggregation, and in which alumine appears to preponderate, rather than calcareous or silicious earths or oxides.

9. Its motion in calm weather, or in a quiet room, is very slow; the particles appear to float in the air in all directions, some rising, some falling, and many swimming horizontally, or

forming a variety of curved lines ; what is most singular, is that no two particles appear to have exactly the same direction ; yet after awhile the greatest proportion fall down obliquely, somewhat in the same manner as a light snow in a calm day. When a current of air is created naturally or artificially in the open air or in a room, you perceive at once an increased velocity in their motion ; they move with rapidity in all directions ; but when a strong current or wind prevails, they are carried with it in a stream, preserving however, as yet, their irregular up and down motion.

10. Its formation is sometimes very rapid, and its accumulation very thick in the lower strata of our atmosphere, but the intensity is variable. Whenever rain or snow falls, this dust is precipitated on the ground by it, whence arises the purity of the air after rain and snow ; but a small share is still left, or soon after formed. In common weather it deposits itself on the ground by slow degrees, and the same in closed rooms. It forms then the dust of our floors, the mould of our roofs, and ultimately the surface of our soil, unless driven by winds from one place to another.

11. I have measured its accumulation in a quiet room, and have found it variable from one-fourth of an inch to one inch in the course of one year ; but it was then in a pulverulent fleecy state, and might be reduced by compression to one-third of its height, making the average of yearly deposit about one-sixth of an inch. In the open air this quantity must be still more variable, owing to the quantities carried by the winds and waters to the plains, valleys, rivers, the sea, &c. or accumulated in closed places or against walls, houses, &c. I calculate, however, that upon an average, from six to twelve inches are accumulated over the ground in one hundred years, where it mixes with the soil and organic exuviæ, to form the common mould.

12. The uses of this chronic meteor are many and obvious. It serves to create mould over rocks, to increase their decomposition, to add to our cultivable soil, to amalgamate the alluvial and organic deposits, to fertilize sandy and unfruitful tracts in the course of time, to administer to vegetable life, &c.

It does not appear that it has any bad influence on men and animals breathing it along with air, unless it should be accumulated in a very intense degree.

13. At Segesta, in Sicily, are to be seen the ruins of an ancient temple; the steps, which surround it on all sides below the pillars, are built on a rock, on the top of a hill detached from any other higher ground. Yet now all the steps and the base of the pillars are under the ground, which has accumulated from this dust and the decay of plants (not trees) to which it has afforded food. There are from five to eight feet from the rock to the surface of this new soil, which has chemically combined in a variety of hardness. This soil has arisen there in about 2000 years, notwithstanding the washings of rain. I quote this as a remarkable instance of the increase of soil by aerial deposits, among many which have fallen under my personal examination.

14. It is commonly believed that the dust of our rooms is produced by the fragments of decomposed vestments, beddings, furnitures, &c.; this cause increases it, and produces a different dust, which mixes with the atmospheric dust; but it is very far from producing it.

15. The dust of the open air is ascribed to that raised from roads and fields, by the pulverization of their surface; but this secondary and visible dust is only a consequence of the first. From whence could arise the dust observed by the means of the sunbeams in a dark corner, in winter, when the ground is frozen, or when it is wet and muddy, or at sea, or on the top of rocky mountains?

16. It is therefore a matter of fact, worth taking into consideration by geologists, that the air still deposits a quantity of dust, which must have been much greater in former periods. Just the same as the sea deposits still a quantity of earthy and saline particles dissolved in it, and which were superabundant at the period when the rocky strata were formed on its bottom. Water being more compact, deposits rocks. Air, which is less dense, deposits a pulverulent matter!

ART. XV. *On the Effect of Vapour on Flame. By* J. F. DANA, *Chemical Assistant in Harvard University, and Lecturer on Chemistry and Pharmacy in Dartmouth College.*

Cambridge, Mass. February 5, 1819.

To Professor Silliman.

DEAR SIR,

ABOUT a year since I made some experiments on the effect of steam on ignited bodies, with a view to learn the theory of the action of the "American water-burner." These experiments were published in an anonymous paper in the North American Review, and have been published in London, without an acknowledgment of their source.

The effect of them concerning bodies is peculiar, and it probably admits of more extensive application to the arts than in the above named instrument alone.

When a jet of steam, issuing from a small aperture, is thrown on burning charcoal, the brightness is increased, if the coal be held at the distance of four or five inches from the pipe through which the steam passes; but if the coal be held nearer it is extinguished, a circular black spot first appears where the steam is thrown on it. The steam in this case does not appear to be decomposed, and the increased brightness of the coal depends probably on a current of atmospheric air, occasioned by the steam. But when a jet of steam, instead of being thrown on a single coal, is made to pass into a charcoal fire, the vividness of the combustion is increased, and the low attenuated flame of coal is enlarged.

When the wick of a common oil lamp is raised, so as to give off large columns of smoke, and a jet of steam is thrown into it, the brightness of the flame is increased, and no smoke is thrown off.

When spirits of turpentine is made to burn on a wick, the light produced is dull and reddish, and a large quantity of thick smoke is given off; but when a jet of steam is thrown into

this flame, its brightness is much increased; and when the experiment is carefully performed, the smoke entirely disappears.

When the vapour of spirits of turpentine is made to issue from a small orifice, and inflamed, it burns, and throws off large quantities of smoke; but when a jet of steam is made to unite with the vapour, the smoke entirely disappears. When vapour of spirits of turpentine and of water are made to issue together from the same orifice, and inflamed, no smoke appears. Hence its disappearing, in the above experiment, cannot be supposed to depend on a current of atmospheric air.

When a jet of steam is thrown into the flame of a spirit of wine lamp, or into flames which evolve no smoke or carbonaceous matter, the same effect is produced as by a current of air.

It appears, from these experiments, that in all flames which evolve smoke, steam produces an increased brightness, and a more perfect combustion.

Now, with a very simple apparatus, steam might be introduced into the flames of street lamps, and that kind of lamp which is used in butchers' shops in London, and in all flames which evolve much smoke. The advantage of such an arrangement would be a more perfect combustion, and a greater quantity of light from the same materials. The flame of the lamps, to which steam is applied, might be made to keep the water boiling which supplies the steam.

I hope the above may not be altogether uninteresting and useless to the readers of your Journal.

Very respectfully, your obedient servant,

J. F. DANA.

ART. XVI. *Analysis of the Harrodsburg Salts, by* EDWARD D. SMITH, M. D. *Professor of Chemistry and Mineralogy in the South-Carolina College.*

MORE than a year since I received a quantity of a white earthy substance, which was said to be obtained by the evaporation of certain mineral waters at Harrodsburg, Kentucky, and there vended at a considerable price, under the name of Epsom salts. The respectable character who presented this powder to me, requested that I would make an analysis of it; but I had not sufficient leisure until lately, to pay the requisite attention to this subject. The results of my examinations are now submitted to the public eye.

The external qualities of this substance are as follow: small white lumps, hard to the touch, but dry and easily yielding to pressure, somewhat gritty to the teeth, and imparting an earthy and saline taste to the tongue.

1. 120 grains of the powder were put into about a half ounce of alcohol, digested for six hours, then washed with more alcohol, filtered and carefully dried.

2. On weighing the dry powder, the loss appeared to be but one grain, so that it contains very little of any substance which is soluble in alcohol.

3. 115 grains (four grains having been lost in the transfer from the filter) were collected and put into rather more than eight times their weight of cold distilled water, and digested for two hours.

4. This watery solution was then filtered, and on weighing, the residue appeared to be 48 grains, so that 67 grains must have been dissolved.

5. 10 grains of the insoluble residue (4) were put into a flask, with 10 ounces of distilled water, and boiled for 1 hour.

6. A small portion of this solution, on being tested with nitrate of barytes, gave a copious white precipitate, with oxalic acid, a white cloud; with ammonia, a slight white cloud; with muriatic acid, a slight bluish tinge. From these tests it was

inferred that sulphate of lime was present, with perhaps a slight trace of muriate of lime.

7. The remainder of this solution was filtered, and on weighing the dried residuum, the loss appeared to be 2 grains, so that sulphate of lime probably constitutes nearly $\frac{1}{5}$ of the insoluble residence (48 grains. 4.)

8. The watery solution, (4) which was supposed to contain 67 grains, was evaporated, and left a residue that weighed but 34 grains, so that 33 grains must have disappeared in the process.

9. Some of this residue dissolved in distilled water, was tested with carbonate of soda, forming an immediate white cloud; with nitrate of barytes, the same; with ammonia, the same; but with oxalate of ammonia, it did not form any cloud until it had stood some time, and then it was slight. From these tests it was inferred that sulphate of magnesia was present.

10. A portion of the dried residuum (7) was treated with diluted muriatic acid, which dissolved nearly the whole of it, with considerable effervescence. The new compound, on examination, proved to be muriate of lime; so that it may be concluded the residuum (7) was principally carbonate of lime.

On considering the results of the preceding experiments, it will appear that more than one half of the substances submitted to analysis, was easily soluble in water, and from the chemical tests used, that it was composed principally of sulphate of magnesia, (Epsom salt) with perhaps a small portion of muriate of lime or magnesia, that of the remainder, about $\frac{1}{5}$ was sulphate of lime, and difficultly soluble in water; and that the rest was perfectly insoluble in water, and consisted principally of carbonate of lime.

There can be no doubt then, that the Harrodsburg salt, in its present state, is very improperly prepared, containing in its composition a large proportion of matter, that is not only inert, but which may produce considerable inconvenience and injury in the stomach and bowels, from its ponderous nature and tendency to form mechanical obstructions. Perhaps the occurrence of such injury may not be frequent, from the circumstance of a large portion of the salt being so insoluble; but,

admitting this to be the fact, there is a manifest impropriety in offering to the public, as medicine, an article which cannot be used as such. Probably the proprietors of this manufactory are not aware of the real nature of the case, and of the facility with which, by a little additional trouble, they could separate the useful and valuable material, from that which is at least useless, and which might also be pernicious.

South-Carolina College, March, 1819.

ART. XVII. *Additional Notice of the Tungsten and Tellurium, mentioned in our last Number.*

PART I. *Description of the Ore.*

COLOUR, dark brown, almost black; brittle, powder a lighter shade of brown than the mineral; hard, scratches glass, scintillates with steel, with a red spark; a degree of polish produced, where the steel strikes, and when the steel is impressed upon it.

Structure compact, in some places slightly porous; lustre, generally dull, sometimes glimmering, and almost resinous.

Crystals octahedral. Specific gravity of three massive pieces, 5.7, 6. and 6.44 mean, 6.05 nearly; probably that of the crystals would be higher.

Infusible by the blow-pipe even with borax, and does not by strong ignition impart any colour to it or to potash; not magnetic, even in fine powder, nor after being heated red hot on charcoal, and in contact with burning grease.

Many specimens decrepitate violently under the blow-pipe. When heated on coals in pieces of considerable size, they often explode with a smart report, and are thrown in fragments sometimes several yards from the fire.

Gangue quartz; accompanying minerals in the same vein, native bismuth, native silver, galena, iron and copper pyrites, much magnetic pyrites, blende, &c.

Geological relations. The country is primitive, and the immediate rock which forms the walls of the vein is said to be gneiss ; (we have not seen it.)

Locality, town of Huntington, parish of New Stratford, county of Fairfield, 20 miles west from New-Haven, Connecticut.

Remark. Native bismuth in small quantities, has been for several years obtained from this mine, but the shaft has been sunk only about ten feet.

Part II. *A variety of the Ore.*

General characters as above, but on some parts, there is seen a whitish, or yellowish, or sometimes darkish metallic substance ; it is in thin plates, like the leaf metals, and sometimes reticulated, and graphic in its disposition ; it is soft and easily cut with the knife. In the specimens examined, it was so much blended with the other ore, and so trifling in quantity, that it was not possible to separate it mechanically, so as to examine it separately.

Part III.—A. *Chemical Trials.*

1. Muriatic acid, hot or cold, produces no effect ; hot nitro-muriatic dissolves the ore with energy, red fumes are evolved, and generally a red solution obtained, from which ammonia precipitates red oxyd of iron abundantly.

2. A heavy lemon-yellow powder remains, insoluble of course in acids, but easily and completely soluble in warm ammonia.

3. A dark powder, in diminished quantity, again appears, more acid dissolves it in part, and again reveals the yellow powder, which ammonia again dissolves, and so on, till nothing remains but some portion of the gangue.

4. The ammoniacal solution, which contains the oxyd of tungsten, is decomposed by acids, and by heat, and instantly deposits a white heavy powder, becoming yellowish by standing, and full yellow by heat.

5. This powder is infusible by the blow-pipe, but ignited with borax in a platinum crucible, it became of a superb blue, like smalt, or between that and Prussian blue.

6. The quantity obtained was too small to make it convenient to attempt its reduction to the metallic state; no doubt remained, however, that it was oxyd of tungsten, or as it is sometimes called, tungstic acid.

7. There were traces of manganese, and all the facts perhaps justify the conclusion, that the ore is very similar to the ferruginous tungsten or wolfram.

8. The calcareous tungsten occurs in octahedral crystals, but we have not before heard of this form in the ferruginous species, which generally affects the prismatic forms.

B. REMARK.

We had been for some time inclined to believe, that the above ore was ferruginous tungsten, but although fortified by the opinion of Col. Gibbs, we were withheld from announcing it, because the form of the crystals, the specific gravity, the colour, and perhaps some other characters, were not perfectly accordant with European descriptions, and with the specimens in our possession, which are from Saxony and Cornwall.

During the necessary chemical trials (which have, we trust, established the correctness of the above opinion,) we very unexpectedly discovered in some of the ores of tungsten, proofs of the existence of tellurium. The conclusion was induced by the phenomena, for nothing was farther from our expectations.

Two fragments were pulverized *by an assistant*, and we therefore cannot say whether they had any external characters different from those of the other pieces; they came, however, from the same part of the vein, and their powder resembled that of the other pieces.

1. Digested in nitro-muriatic acid, a straw-yellow solution, slightly inclining to green, was obtained, and a black powder was left behind.

2. More acid digested on this powder, gave a deep red solution of iron, and left the yellow oxyd of tungsten, which being

dissolved in ammonia, the black powder again appeared, and so on, as under 3. Part III.

3. The solution 1, diluted largely with water, deposited an abundant white precipitate, which was very heavy and rapidly subsided.

4. Alcohol and ammonia, respectively produced the same effect, only more decidedly.

5. This precipitate, evidently an oxyd of a metal, being collected on a filter and dried, exhibited the following properties.

6. Heated by the blow-pipe on charcoal, it was instantly volatilized in part, and in part decomposed, with an almost explosive effervescence; numerous ignited globules of metal appeared on the charcoal, and burned with an abundant flame of a delicate blue colour, edged occasionally with green.

7. In many trials, these results always occurred, and sometimes a peculiar odour was perceived, at first thought to be owing to arsenic, but it was incomparably feebler, and somewhat resembled that of radishes.*

8. Zinc, iron, and tin, plunged into separate portions of the nitro-muriatic solution, precipitated abundantly a black flocculent substance.

9. On charcoal before the blow-pipe, this substance was very combustible, with a blue flame, and was completely dissipated in the form of white oxyd, with the above smell.

10. Some of it was obtained on the charcoal in metallic globules; it was a brittle metal, white, with a tinge of red, and foliated, but not so distinctly as bismuth and antimony.

11. The filters on which the white oxyd had been deposited, burned almost with explosion, nearly as rapidly as if they had been soaked with nitrate of potash, or of ammonia, and the characteristic blue flame appeared while the burning lasted.

* This was most remarkably perceived on one occasion, where, under the idea that possibly chrome might exist in the ores, they had been intensely heated in a forge along with pearl ashes. The mass, when lixiviated, gave only a greenish solution, becoming colourless by nitric acid, and again greenish by an alkali; this was supposed to be owing to iron and manganese. No metal was obtained, except a few minute globules of attractable iron, but the laboratory was filled with white fumes, having the peculiar odour alluded to.

12. Other experiments were made upon the metal, (not the oxyd.) It gave to strong sulphuric acid, (simply by standing in it in the cold) an amethystine colour, which disappeared as the acid grew weaker, by attracting water from the air.

13. With nitric acid it formed a colourless solution, not decomposed by water.

14. It did not dissolve in muriatic acid, till a few drops of nitric acid were added.

15. The white oxyd heated with charcoal in a small coated recurved glass tube, afforded brilliant metallic globules, which rose by distillation, collected in the bend of the tube, and resembled drops of quicksilver, except that they were solid.

C. REMARK.

The above facts having induced the conclusion that the metal, thus unexpectedly discovered in the ores of tungsten, was tellurium,* we were led to search for external characters by which to judge what specimens contained it. The ores from Transylvania, (the only telluric ores with which we are acquainted,) bearing no analogy in appearance or composition to those before us, we were led to inquire whether the tellurium in these latter ores was *in combination* with tungsten, or merely *in mixture*. The external characters detailed in part II, tend perhaps to fortify the latter opinion. If we mistake not, we there found a proper ore of tellurium mixed with a proper ore of tungsten, but we have also by chemical means, found tellurium where similar external characters were not apparent. Before the appearance of our next Number, we hope to obtain purer and better specimens. In the mean time we add the following facts.

1. A crystal, and a massive piece of the kind described under part I, and specimens of two varieties of those described under part II, were digested in nitro-muriatic acid.

* Several of the facts, we are aware, accord with the properties of bismuth, between which and tellurium there are several strong points of resemblance, but a number of other facts appear irreconcileable with the properties of that metal, and of every other except tellurium.

2. Both oxyd of tungsten, and oxyd of tellurium were obtained from all of them.

3. Many specimens have been examined which have afforded tungsten only, and no tellurium.

At a convenient time, it is hoped that a more complete examination of this subject may be presented to the public.

In the mean time, we may submit to mineralogists and chemists, whether if this is not a new mineral, it is not at least a new association of two minerals before known. It has not been forgotten that gold and silver are frequently combined with tellurium: neither of them has, however, been discovered, (although sought after by proper tests) during the above trials.

Yale College, March, 1819.

ART. XVIII. *A Substitute for Woulfe's or Nooth's Apparatus, by* ROBERT HARE, M. D. *Professor of Chemistry in the Medical Department of the University of Pennsylvania, and Member of various Learned and Scientific Societies.* With a Plate.

FEW subjects have more occupied the attention of chemists, than the means of impregnating fluids with gaseous substances. The contrivances of Woulfe and Nooth, especially the former, have been almost universally used; and have gained for the inventors merited celebrity. Various improvements in Woulfe's bottles have been devised. Still I believe an apparatus replete with similar advantages, but less unwieldy, less liable to fracture; and having fewer junctures to make at each operation, has been a great desideratum with every practical chemist. It has, however, ceased to be so with me, since I contrived the apparatus which I am about to describe.

Fig. 1. represents 3 jars placed concentrically within each other, and so proportioned and situated, as to admit 2 open-necked concentric bell glasses alternately between them. The

Fig . 1.

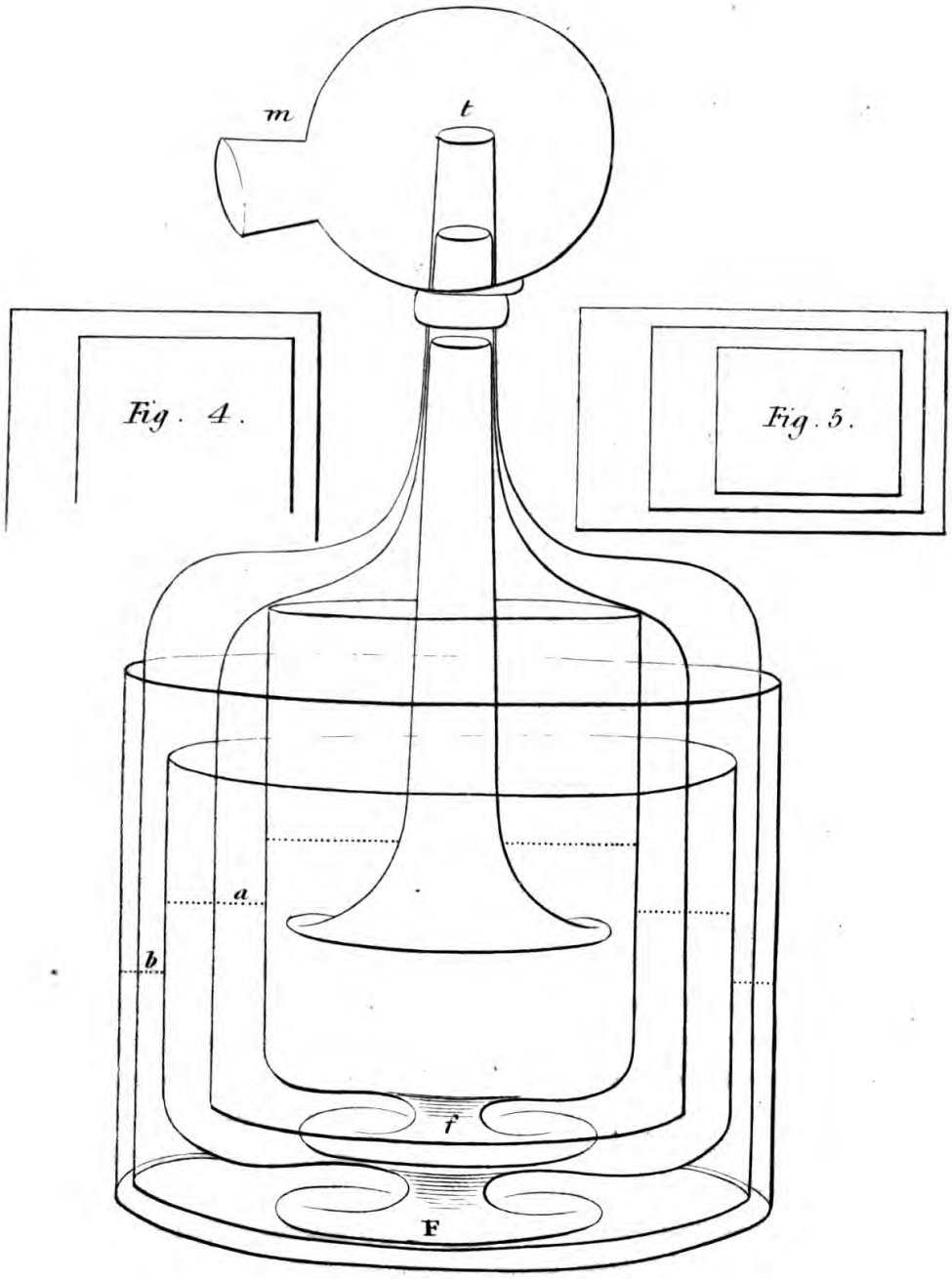


Fig . 4.

Fig . 5.

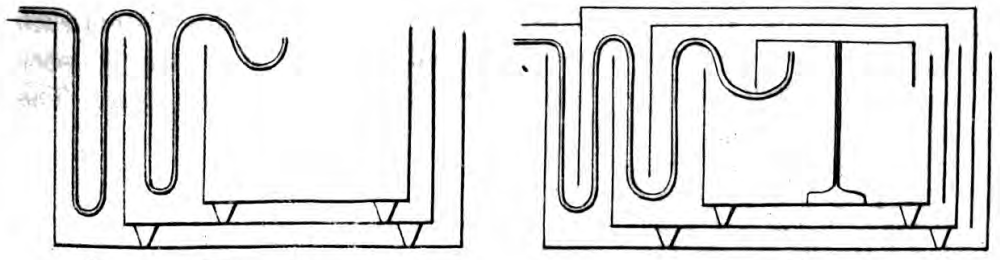


Fig . 3.

Fig . 2.

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neck of the exterior bell glass is introduced into the tubulure of the receiver above, and receives the neck of the interior bell glass. Into this is inserted a trumpet-shaped tube. The two interior jars are furnished with feet F, *f*. In order to put this apparatus into operation, remove (without taking them apart) the bell glasses, receiver, and tube from the jars. Pour into the latter the fluid, to be impregnated, till it reaches the height marked by the dots. The funnel mouth, *m*, of the receiver being provided with a suitable cork soaked in wax, fasten into it firmly the beak of the retort, containing the generating materials. The bell glasses are then to be replaced in the jars, and arranged as in the figure. It must be self-evident that the gas proceeding from the retort, (if the juncture at *m* be air tight) must press on the fluid in the innermost jar, through the trumpet-shaped tube. If not imbibed with adequate speed, it must soon press on the fluid at *a*, causing it to subside to the narrow part of the foot *f*, and thus to expose a much larger surface. If the absorption be still inadequate, a further subsidence must ensue, and the gas escaping round the brim of the interior bell glass will act on the fluid at *b*, and enlarge its surface by depressing it to the narrow part of the foot F. Should the increased pressure and more extended contact thus obtained, be still incompetent to effect a complete absorption, the excess of the gas may escape round the brim of the external bell glass into the atmosphere.

But so effectual is this process in promoting impregnation, that I have obtained strong muriatic acid in the central jar, without producing any sensible acidity in the outside one. Absorption into the retort or receiver, is prevented by not allowing as much fluid to be above the mouth of the trumpet-shaped tube, as would be competent to fill the cavity between it, and the termination of the open neck of the exterior bell glass at *t*. As this neck rises about 2 or 3 inches into the receiver, it prevents any foul matter which may condense or boil over, from getting into the jars. If practicable, it would be better that the bell glasses, and tube, and receiver, should be united together while hot, at the glass-house. If all could not be joined in this way, it would still be advantageous to unite

thus the receiver, and the exterior bell glass. The interior bell and tube might then be fastened together, by grinding or luting. As yet I have only used lutings of waxed cloth, or cork. It may be proper to point out, that 3 or more concentric bell glasses, and 4 or more jars, might be used. The union of the bells, receiver, and tube once effected, it is hardly more troublesome to use 3 than 2. When the fluid in the central jar is saturated, this may be emptied and replenished from the middle jar, the latter from the external one. Then supplying the external jar anew, the process may be continued.

The other figures are to explain an apparatus on the same principle, constructed of hollow, oblong paralellopipeds, differing in length more than in breadth; so as to allow a serpentine tube to wind into the interior, and deliver gas under a vessel shaped like a T.

Fig. 2. represents a vertical section of the whole as when situated for use.*

Fig. 3. a vertical section of the lower vessels only.

Fig. 4. a vertical section of the covers alone.

Fig. 5. a horizontal section, or ground plan of the lower vessels. The upper vessels are so proportioned as to divide the distances between the lower ones equally.

It may be well to mention, that this apparatus, from the facility with which it may be cleaned and inspected internally, admits of being made of porcelain or stone ware.† I have had a cylindrical one constructed of the latter material, in which the covers are in one piece, with a tube in the centre for introducing gas. The apparatus may be made more efficacious, by drilling a series of small holes round the brims of the bell glasses or covers, so as to cause the gas, instead of passing round the brims in large bubbles, to divide itself into very small ones. By this means it will be more thoroughly intermingled with fluid.

* Excepting, that the covers ought to be so depressed, as that their brims may be lower than the bottoms of the interior vessels over which they are placed respectively. This is necessary to prevent the gas from escaping, ere it have access to the surface of the fluid beneath those bottoms.

† The apparatus may also be made of glass bottles, duly proportioned, and cut (truncated) alternately near the shoulder and near the bottom.

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Fig. 1.

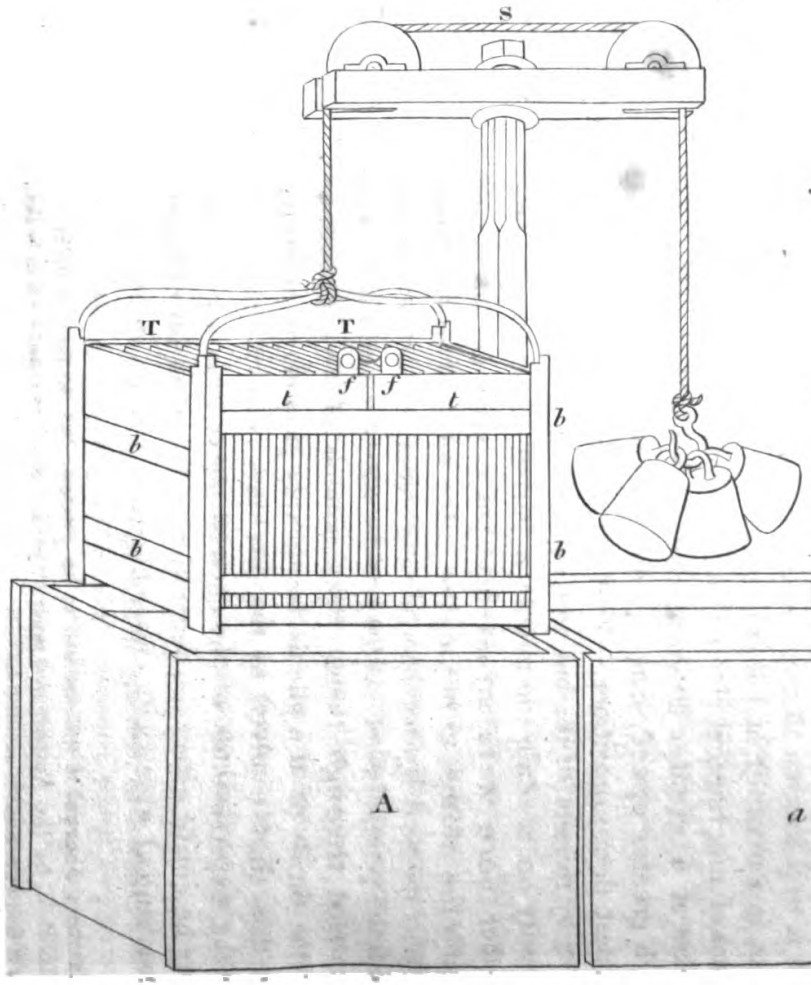


Fig. 2.

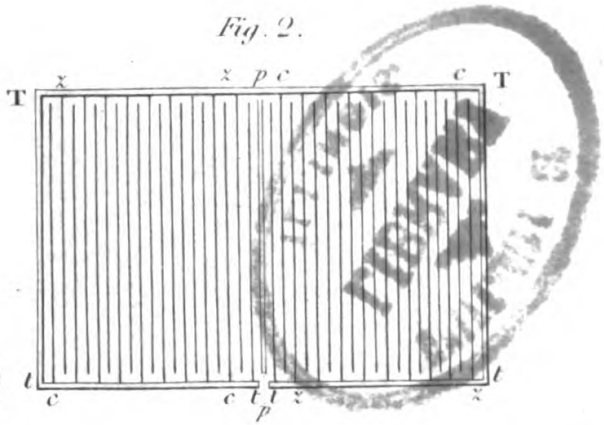


Fig. 3.

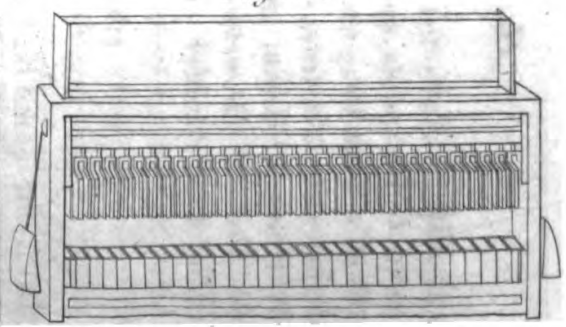
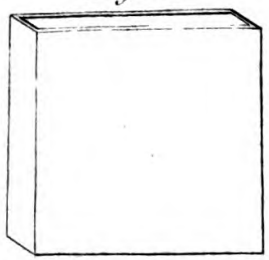


Fig. 4.



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ART. XIX. *A New Theory of Galvanism, supported by some Experiments and Observations made by means of the Calorimotor, a new Galvanic Instrument. Read before the Academy of Natural Sciences, Philadelphia,* by* ROBERT HARE, M. D. *Professor of Chemistry in the Medical Department of the University of Pennsylvania, and Member of several Learned Societies.*

(With an Engraving.)

I HAVE for some time been of opinion that the principle extricated by the Voltaic pile is a compound of caloric and electricity, both being original and collateral products of Galvanic action.

The grounds of this conviction and some recent experiments confirming it, are stated in the following paper.

It is well known that heat is liberated by the Voltaic apparatus, in a manner and degree which has not been imitated by means of mechanical electricity; and that the latter, while it strikes at a greater distance, and pervades conductors with much greater speed, can with difficulty be made to effect the slightest decompositions. Wollaston, it is true, decomposed water by means of it; but the experiment was performed of necessity on a scale too minute to permit of his ascertaining, whether there were any divellent polar attractions exercised towards the atoms, as in the case of the pile. The result was probably caused by mechanical concussion, or that process by which the particles of matter are dispersed when a battery is discharged through them. The opinion of Dr. Thomson, that the fluid of the pile is in quantity greater, in intensity less, than that evolved by the machine, is very inconsistent with the experiments of the chemist above mentioned, who, before he could effect the separation of the elements of water by mechanical electricity, was obliged to confine its emission

* In whose Journal it was ordered to be printed, but, to prevent delay, it was published, by the Author, in a separate paper, and forwarded by him to the Editor of this Journal.

to a point imperceptible to the naked eye. If already so highly intense, wherefore the necessity of a further concentration? Besides, were the distinction made by Dr. Thomson correct, the more concentrated fluid generated by a galvanic apparatus of a great many small pairs, ought most to resemble that of the ordinary electricity; but the opposite is the case. The ignition produced by a few large Galvanic plates, where the intensity is of course low, is a result most analogous to the chemical effects of a common electrical battery. According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air, with immeasurable celerity, and distributing itself in the interior of bodies, communicates a reciprocally repellent power to atoms, but not to masses. Electricity does not radiate in or through any matter; and while it pervades some bodies, as metals, with almost infinite velocity; by others, it is so far from being conducted, that it can only pass through them by a fracture or perforation. Distributing itself over surfaces only, it causes repulsion between masses, but not between the particles of the same mass. The disposition of the last-mentioned principle to get off by neighbouring conductors, and of the other to combine with the adjoining matter, or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties, consequent to some such union, they could not, in piles of thousands of pairs, be carried forward through the open air and moisture; the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric.

Pure electricity does not expand the slips of gold-leaf, between which it causes repulsion, nor does caloric cause any repulsion in the ignited masses which it expands. But as the compound fluid extricated by Galvanic action, which I shall call electro-caloric, distributes itself through the interior of bodies, and is evidently productive of corpuscular repulsion, it is in this respect more allied to caloric than to electricity.

It is true, that when common electricity causes the deflagration of metals, as by the discharge of a Leyden jar, it must be supposed to insinuate itself within them, and cause a reaction between their particles. But in this case, agreeably to my hypothesis, the electric fluid combines with the latent caloric previously existing there, and, adding to its repulsive agency, causes it to overpower cohesion.*

Sir Humphry Davy was so much at a loss to account for the continued ignition of wire at the poles of a Voltaic apparatus, that he considers it an objection to the materiality of heat; since the wire could not be imagined to contain sufficient caloric to keep up the emission of this principle for an unlimited time. But if we conceive an accumulation of heat to accompany that of electricity throughout the series, and to be propagated from one end to the other, the explanation of the phenomenon in question is attended by no difficulty.

The effect of the Galvanic fluid on charcoal is very consistent with my views, since, next to metals, it is one of the best conductors of electricity, and the worst of heat, and would therefore arrest the last, and allow the other to pass on. Though peculiarly liable to intense ignition, when exposed between the poles of the Voltaic apparatus, it seems to me it does not display this characteristic with common electricity. According to Sir Humphry Davy, when in connexion with the positive pole, and communicating by a platina wire with the negative pole, the latter is less heated than when, with respect to the poles, the situation of the wire and charcoal is reversed. The rationale is obvious: charcoal, being a bad conductor, and a good radiator, prevents the greater part of the heat from reaching the platina, when placed between it and the source whence the heat flows.

I had observed that as the number of pairs in Volta's pile had been extended, and their size and the energy of the iner-

* Possibly the electric fluid causes decompositions when emitted from an impalpable point (as in the experiments of Wollaston) because its repulsive agency is concentred between integral atoms, in a mode analogous to that here referred to; a filament of water in the one case, and of wire in the other, being the medium of discharge.

posed agents lessened, the ratio of the electrical effects to those of heat had increased; till in De Luc's column they had become completely predominant; and, on the other hand, when the pairs were made larger and fewer (as in Children's apparatus) the calorific influence had gained the ascendancy. I was led to go farther in this way, and to examine whether one pair of plates of enormous size, or what might be equivalent thereto, would not exhibit heat more purely, and demonstrate it, equally with the electric fluid, a primary product of Galvanic combinations. The elementary battery of Wollaston, though productive of an evanescent ignition, was too minute to allow him to make the observations which I had in view.

Twenty copper and twenty zinc plates, about nineteen inches square, were supported vertically in a frame, the different metals alternating at one half inch distance from each other. All the plates of the same kind of metal were soldered to a common slip, so that each set of homogeneous plates formed one continuous metallic superficies. When the copper and zinc surfaces, thus formed, are united by an intervening wire, and the whole immerged in an acid, or acetosaline solution, in a vessel devoid of partitions, the wire becomes intensely ignited; and when hydrogen is liberated it usually takes fire, producing a very beautiful undulating, or corruscating flame.

I am confident, that if Volta and the other investigators of Galvanism, instead of multiplying the pairs of Galvanic plates, had sought to increase the effect by enlarging one pair as I have done, (for I consider the copper and zinc surfaces as reduced to two by the connexion) the apparatus would have been considered as presenting a new mode of evolving heat, as a primary effect independently of electrical influence. There is no other indication of electricity when wires from the two surfaces touch the tongue, than a slight taste, such as is excited by small pieces of zinc and silver laid on it and under it, and brought into contact with each other.

It was with a view of examining the effects of the proximity and alternation in the heterogeneous plates that I had them

cut into separate squares. By having them thus divided, I have been enabled to ascertain that when all of one kind of metal are ranged on one side of the frame, and all of the other kind on the other side of it, the effect is no greater than might be expected from one pair of plates.

Volta, considering the changes consequent to his contrivance as the effect of a movement in the electric fluid, called the process electro-motion, and the plates producing it electromotors. But the phenomena show that the plates, as I have arranged them, are calori-motors, or heat movers, and the effect calori-motion. That this is a new view of the subject, may be inferred from the following passage in Davy's Elements. That great chemist observes, "When very small conducting surfaces are used for conveying very large quantities of electricity, they become ignited; and of the different conductors that have been compared, charcoal is most easily heated by electrical discharges,* next iron, platina, gold, then copper, and lastly, zinc. The phenomena of electrical ignition, whether taking place in gaseous, fluid, or solid bodies, always seem to be the result of a violent exertion of the electrical attractive and repellent powers, which may be connected with motions of the particles of the substances affected. That no subtile fluid, such as the matter of heat has been imagined to be, can be discharged from these substances, in consequence of the effect of the electricity, seems probable, from the circumstance, that a wire of platina may be preserved in a state of intense ignition in vacuo, by means of the Voltaic apparatus, for an unlimited time; and such a wire cannot be supposed to contain an inexhaustible quantity of subtile matter."

But I demand where are the repellent and attractive powers to which the ignition produced by the Calorimotor can be attributed? Besides, I would beg leave respectfully to inquire of this illustrious author, whence the necessity of considering the heat evolved under the circumstances alluded to as the effect of the electrical fluid; or why we may not as well suppose the latter to be excited by the heat? It is evident, as he

* The conclusions are drawn from experiments made by the electricity of the Voltaic apparatus.

observes, that a wire cannot be supposed to contain an inexhaustible supply of matter however subtile; but wherefore may not one kind of subtile matter be supplied to it from the apparatus as well as another; especially, when to suppose such a supply is quite as inconsistent with the characteristics of pure electricity, as with those of pure caloric?

It is evident from Mr. Children's paper in the Annals of Philosophy, on the subject of his large apparatus, that the ignition produced by it was ascribed to electrical excitement.

For the purpose of ascertaining the necessity of the alternation and proximity of the copper and zinc plates, it has been mentioned that distinct square sheets were employed. The experiments have since been repeated and found to succeed by Dr. Patterson and Mr. Lukens, by means of two continuous sheets, one of zinc, the other of copper, wound into two concentric coils or spirals. This, though the circumstance was not known to them, was the form I had myself proposed to adopt, and had suggested as convenient for a Galvanic apparatus to several friends at the beginning of the winter;* though the consideration above stated induced me to prefer for a first experiment a more manageable arrangement.

Since writing the above, I find that when, in the apparatus of twenty copper and twenty zinc plates, ten copper plates on one side are connected with ten zinc on the other, and a communication made between the remaining twenty by a piece of iron wire, about the eighth of an inch in diameter, the wire enters into a vivid state of combustion on the immersion of the plates. Platina wire equal to No. 18 (the largest I had at hand) is rapidly fused if substituted for the iron.

This arrangement is equivalent to a battery of two large Galvanic pairs; excepting that there is no insulation, all the plates being plunged in one vessel. I have usually separated the pairs by a board, extending across the frame merely.

Indeed, when the forty plates were successively associated in pairs, of copper and zinc, though suspended in a fluid held

* Especially to Dr. T. P. Jones, and Mr. Rubens Peale, who remember the suggestion.

in a common recipient without partitions; there was considerable intensity of Galvanic action. This shows that, independently of any power of conducting electricity, there is some movement in the solvent fluid which tends to carry forward the Galvanic principle from the copper to the zinc end of the series. I infer that electro-caloric is communicated in this case by circulation, and that in non-elastic fluids the same difficulty exists as to its retrocession from the positive to the negative end of the series, as is found in the downward passage of caloric through them.

It ought to be mentioned, that the connecting wire should be placed between the heterogeneous surfaces before their immersion, as the most intense ignition takes place immediately afterward. If the connexion be made after the plates are immersed, the effect is much less powerful; and sometimes after two or three immersions the apparatus loses its power, though the action of the solvent should become in the interim much more violent. Without any change in the latter, after the plates have been for some time suspended in the air, they regain their efficacy. I had observed in a Galvanic pile of three hundred pairs of two inches square, a like consequence resulting from a simultaneous immersion of the whole.* The bars holding the plates were balanced by weights, as window-sashes are, so that all the plates could be very quickly dipped. A platina wire, No. 18, was fused into a globule, while the evolution of potassium was demonstrated by a rose-coloured flame arising from some potash which had been placed between the poles. The heat however diminished in a few seconds, though the greater extrication of hydrogen from the plates indicated a more intense chemical action.

Agreeably to an observation of Dr. Patterson, electrical excitement may be detected in the apparatus by the condensing electroscope; but this is no more than what Volta observed to be the consequence of the contact of heterogeneous metals.

The thinnest piece of charcoal intercepts the calorific agent, whatever it may be. In order to ascertain this, the inside of

* See Plate. Fig. 3.

a hollow brass cylinder, having the internal diameter two inches, and the outside of another smaller cylinder of the same substance, were made conical and correspondent, so that the greater would contain the less, and leave an interstice of about one-sixteenth of an inch between them. This interstice was filled with wood, by plugging the larger cylinder with this material, and excavating the plug till it would permit the smaller brass cylinder to be driven in. The excavation and the fitting of the cylinders was performed accurately by means of a turning lathe. The wood in the interstice was then charred by exposing the whole covered by sand in a crucible to a red heat. The charcoal, notwithstanding the shrinkage consequent to the fire, was brought into complete contact with the inclosing metallic surfaces by pressing the interior cylinder further into the exterior one.

Thus prepared, the interior cylinder being made to touch one of the Galvanic surfaces, a wire brought from the other Galvanic surface into contact with the outside cylinder, was not affected in the least, though the slightest touch of the interior one caused ignition. The contact of the charcoal with the containing metals probably took place throughout a superficies of four square inches, and the wire was not much more than the hundredth part of an inch thick, so that unless it were to conduct electricity about forty thousand times better than the charcoal, it ought to have been heated; if the calorific influence of this apparatus result from electrical excitement.

I am led finally to suppose, that the contact of dissimilar metals, when subjected to the action of solvents, causes a movement in caloric as well as in the electric fluid, and that the phenomena of Galvanism, the unlimited evolution of heat by friction, the extrication of gaseous matter without the production of cold, might all be explained by supposing a combination between the fluids of heat and electricity. We find scarcely any two kinds of ponderable matter which do not exercise more or less affinity towards each other. Moreover, imponderable particles are supposed highly attractive of ponderable ones. Why then should we not infer the existence of similar affinities between imponderable particles reciprocally?

That a peculiar combination between heat and light exists in the solar beams, is evident from their not imparting warmth to a lens through which they may pass, as do those of our culinary fires.

Under this view of the case, the action of the poles in Galvanic decomposition is one of complex affinity. The particles of compounds are attracted to the different wires agreeably to their susceptibilities to the positive and negative attraction, and the caloric leaving the electric fluid with which it had been combined, unites with them at the moment that their electric state is neutralized.

As an exciting fluid, I have usually employed a solution of one part sulphuric acid, and two parts muriate of soda with seventy of water; but, to my surprise, I have produced nearly a white heat by an alkaline solution barely sensible to the taste.

For the display of the heat effects, the addition of manganese, red lead, or the nitrates, is advantageous.

The rationale is obvious. The oxygen of these substances prevents the liberation of the gaseous hydrogen, which would carry off the caloric. Adding to diluted muriatic acid, while acting on zinc, enough red lead to prevent effervescence, the temperature rose from 70 to 110 Fahrenheit.

The power of the calorimotor is much increased by having the communication between the different sheets formed by very large strips or masses of metal. Observing this, I rendered the sheets of copper shorter by half an inch, for a distance of four inches of their edges, where the communication was to be made between the zinc sheets; and, vice versa, the zinc was made in the same way shorter than the copper sheets where these were to communicate with each other. The edges of the shortened sheets being defended by strips of wood, tin was cast on the intermediate protruding edges of the longer ones, so as to embrace a portion of each equal to about one quarter of an inch by four inches. On one side, the tin was made to run completely across, connecting at the same time ten copper and ten zinc sheets. On the other side there was an interstice of above a quarter of an inch left between the stratum of tin embracing the copper, and that embracing

the zinc plates. On each of the approaching terminations of the connecting tin strata was soldered a kind of forceps, formed of a bent piece of sheet brass, furnished with a screw for pressing the jaws together. The distance between the different forceps was about two inches. The advantage of a very close contact was made very evident by the action of the screws; the relaxation or increase of pressure on the connecting wire by turning them being productive of a correspondent change in the intensity of ignition.

It now remains to state, that by means of iron ignited in this apparatus, a fixed alkali may be decomposed extemporaneously.* If a connecting iron wire, while in combustion, be touched by the hydrate of potash, the evolution of potassium is demonstrated by a rose-coloured flame. The alkali may be applied to the wire in small pieces in a flat *hook* of sheet iron. But the best mode of application is by means of a tray made by doubling a slip of sheet iron at the ends, and leaving a receptacle in the centre, in which the potash may be placed covered with filings. This tray being substituted for the connecting wire, as soon as the immersion of the apparatus causes the metal to burn, the rose-coloured flame appears, and if the residuum left in the sheet iron be afterward thrown into water, an effervescence sometimes ensues.

I have ascertained that an iron heated to combustion, by a blacksmith's forge fire, will cause the decomposition of the hydrate of potash.

The dimensions of the Calorimotor may be much reduced without proportionably diminishing the effect. I have one of sixty plates within a cubic foot, which burns off No. 16, iron wire. A good workman could get 120 plates of a foot square within a hollow cube of a size no larger. But the inflammation of the hydrogen which gives so much splendour to the experiment, can only be exhibited advantageously on a large scale.

* This evidently differs from the common mode of decomposing the fixed alkalies by galvanism: there the effect depends on electrical attractions and repulsions—here on the chemical agency of ignited iron produced *extemporaneously* in the galvanic circuit: this mode of operating appears to be new. *Editor.*

EXPLANATION OF THE PLATE.

A *a*, Fig. 1st, two cubical vessels, 20 inches square, inside. *b b b b* a frame of wood containing 20 sheets of copper, and 20 sheets of zinc, alternating with each other, and about half an inch apart. T T *t t* masses of tin cast over the protruding edges of the sheets which are to communicate with each other. Fig. 2, represents the mode in which the junction between the various sheets and tin masses is effected. Between the letters *z z*, the zinc only is in contact with the tin masses. Between *c c* the copper alone touches. It may be observed, that, at the back of the frame, ten sheets of copper between *c c*, and ten sheets of zinc between *z z*, are made to communicate, by a common mass of tin extending the whole length of the frame, between T T : but in front, as in fig. 1, there is an interstice between the mass of tin connecting the ten copper sheets, and that connecting the ten zinc sheets. The screw forceps, appertaining to each of the tin masses, may be seen on either side of the interstice : and likewise a wire for ignition held between them. The application of the rope, pulley, and weights, is obvious. The swivel at S permits the frame to be swung round and lowered into water in the vessel *a*, to wash off the acid, which, after immersion in the other vessel, might continue to act on the sheets, encrusting them with oxide. Between *p p* there is a wooden partition which is not necessary, though it may be beneficial.

Fig. 3, represents an apparatus alluded to, page 419. It consists of a couronne des tasses, reduced to a form no less compact than that of the trough. Hollow parallelopipeds of glass are substituted for tumblers or cells. The plates are suspended to bars counterpoised like window-sashes.

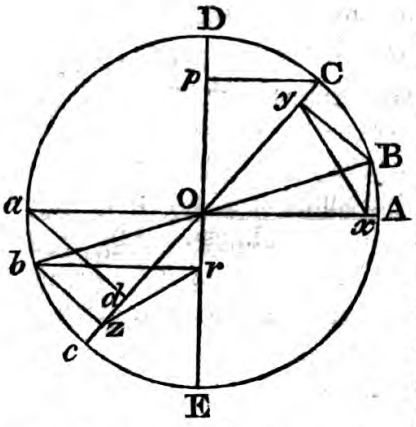
The advantages are as follows. The material is one of the best non-conductors, is easily cleansed, and is the most impervious to solvents. The fracture of one of the cups is easily remedied by a supernumerary. They may be procured (as in the United States) where porcelain cannot be had. The shock from 300 pairs is such as few will take a second time. Some of the effects have already been stated.*

At Fig. 4, one of the hollow glass parallelopipeds on an enlarged scale is represented.

* The glasses may be had by applying to Edw. A. Pearson, No. 71 Cornhill, Boston.

MATHEMATICS.

ART. XX. *An improved Method of obtaining the Formulæ for the Sines and Cosines of the Sum and Difference of two Arcs, by* PROFESSOR STRONG, *of Hamilton College.*



IN the circle ABCD let AB and BC denote any two arcs contiguous to each other. Draw their limiting diameters Aa, Cc; their sines Bx, By; and join x, y. Then will xy = sine of (AB + BC): for if upon OB as a diameter we describe a circle, it will manifestly pass through the points x and y, (since the angles OxB, OyB are right, see Euc. 31. 3.) therefore OxBy is a quadrilateral inscribed in a circle described on OB as a diameter, and the angle yOx at the circumference stands upon an arc whose chord is xy. Again, if from a we draw ad perpendicular to Cc, it will be the sine of the arc ac (=AB+BC). If now we describe a circle on aO as diameter, it will pass through d, (see Euc. 31. 3.) therefore ad is the chord of an arc on which the angle aOc stands in the circle described on aO. But in equal circles the chords of arcs on which equal angles at the centres or circumferences stand are equal; (see Euc. 26. and 29. 3.) hence $xy = ad = \sin(AB + BC)$. Now sine OxBy is a quadrilateral inscribed in the circle described on OB as diameter, we shall have (Euc. D. 6.) $OB \cdot xy = Bx \cdot Oy + By \cdot Ox =$ sinAB · cosCB + sinCB · cosAB. If OB be denoted by r, we shall have xy, or sin(AB + BC)=

$$\frac{\text{sinAB} \cdot \text{cosCB} + \text{sinCB} \cdot \text{cosAB}}{r}.$$

If AB = A, BC = B, and the radius $r = 1$, sin(A + B) = sin

A· cosB + sinB· cosA; which is the known formula for the sine of the sum of two arcs, to the radius 1.

Again, if through O we draw the diameter DE perpendicular to A*a*, then will DC be the complement of (AB + BC). Draw C*p*, the sine of DC = cos(AB + BC). Through B draw the diameter B*b*; from *b*, draw the sines *bz*, *br*, of the arcs *bc*, *b*E respectively, and join *z*, *r*. Then by describing two circles, one on *b*O as diameter, the other on OC, it may be proved as before that the circle described on *b*O passes through the points *z* and *r*, and that the circle described on CO passes through *p*: and hence, by the same reasoning as before, *zr* = C*p* = cos(AB + BC). Now O*bzr* being a quadrilateral inscribed in the circle described on *b*O, we have (by the prop. before cited) *b*O· *zr* + O*r*· *bz* = *br*. O*z*; and hence *b*O· *zr* = *br*· O*z* − O*r*· *bz*. But *br* = sine arc *b*E = sine arc BD; and since BD is the complement of AB, *br* = cosAB. In like manner O*z* = cosBC, O*r* = sinAB, and *bz* = sinBC; hence by substitution, *b*O· *zr* = cosAB· cosBC − sinAB· sinBC. By using the same notation as before, we have cos(A + B) $= \frac{\text{cosA}\cdot\text{ cosB} - \text{sinA.\ sinB}}{r} =$ (if $r = 1$) cosA· cosB − sinA· sin B, which is the known formula for the cosine of the sum of two arcs.

The same construction will answer for the two remaining cases: for if we suppose that *b*E and *bc* are two arcs, then will *c*E be their difference, and *zr* the sine of *c*E, as proved above; hence *zr* $(= \sin(b\text{E} - bc)) = \frac{br\cdot\text{ O}z - \text{O}r\cdot bz}{b\text{O}}$. But *br* = sin *b*E, and O*r* = its cosine; and *bz* = sine *bc*, and O*z* = its cos. hence if *b*E be denoted by *a*, *bc* by *b*, and O*b* as before, then will $\sin(a-b) = \frac{\text{sin}a\cdot\text{ cos}b - \text{sin}b\cdot\text{ cos}a}{r} =$ (if $r = 1$) sin*a*· cos*b* − sin*b*· cos*a*·−. Again, AB+BC is the complement of DC or *c*E; hence by the first part of the above investigation, *xy* = sin(AB + BC) = cos*c*E: but *xy* or sin(A+B) = cos $(a-b) = \frac{\text{sinA}\cdot\text{ cosB} + \text{sinB}\cdot\text{ cosA}}{r}$; and as sinA or AB = cosBD = cos*b*E, O*x* = cosA or AB = sinBD = sin*b*E, B*y*

$= bz = \sin bc$, and $Oy = Oz = \cos bc$, we shall have, by substitution, $\cos(a-b) = \frac{\cos a \cdot \cos b + \sin a \cdot \sin b}{r}$, $= (\text{if } r = 1) \cos a \cdot \cos b + \sin a \cdot \sin b$.

From what has been said it appears, that if A and B be any two arcs, of which A is the greatest, then

$$\text{Sin}(A \pm B) = \frac{\text{sinA} \cdot \text{cosB} \pm \text{sinB} \cdot \text{cosA}}{r};$$

$$\text{Cos}(A \pm B) = \frac{\text{cosA} \cdot \text{cosB} \mp \text{sinA} \cdot \text{sinB}}{r}.$$

When the radius r is supposed $= 1$, the denominators in these formulæ disappear. In the latter, A and B are used for a and b, for the sake of homogeneity. The propriety of this is manifest; for as a and b denote two indefinite arcs, the same reasoning will apply to A and B, as to a and b, the first being supposed in each case the greatest.

The following Diophantine Problem was proposed for solution some months ago in a Periodical Journal, which has since been discontinued. To those who are interested in speculations of this nature, we presume that the following solution, forwarded by Professor STRONG, of Hamilton College, will not be unacceptable.

PROBLEM.

To find three positive rational Numbers, x, y, and z, such that x^2-y, x^2-z, y^2-x, and y^2-z may all be squares.

Assume $x-ay$ for the root of the square x^2-y: then $x^2-y = (x-ay)^2$, whence $x = \frac{a^2y+1}{2a}$. In like manner, by assuming $x-bz$ for the root of the square x^2-z, we find $z = \frac{2bx-1}{b^2}$. But $y^2-x = y^2 - \frac{a^2y+1}{2a}$, $\left(\text{since } x = \frac{a^2y+1}{2a}\right)$; and as this is

to be made a square, assume $y-e\left(\frac{a^2y+1}{2a}\right)$ for its root; whence, by proceeding as before, we find $y=\frac{2a+c^2}{4ca-a^2c^2}$. But $x=\frac{a^2y+1}{2a}$ = (by substituting for y its value) $\frac{a^2+2c}{4ca-c^2a^2}$. Again $z=\frac{2bx-1}{b^2}$ = (by substituting for x its value)

$$\frac{2b\left(\frac{a^2+2c}{4ca-c^2a^2}\right)-1}{b^2};$$ hence

$$y^2-z=\frac{\left(\frac{2a+c^2}{4ca-c^2a^2}\right)^2\times b^2-2b\left(\frac{a^2+2c}{4ca-c^2a^2}\right)+1}{b^2}$$

(by substituting for y and z their values;) and as this also is to be made a square, assume for its root $\frac{be-1}{b}$. Then we shall have $\left(\frac{2a+c^2}{4ca-c^2a^2}\right)^2\times b^2-2b\left(\frac{a^2+2c}{4ca-c^2a^2}\right)+1=(be-1)^2$; from which, by reduction,

$$b=2\times\frac{e(4ca-c^2a^2)^2-(a^2+2c)(4ca-c^2a^2)}{e^2(4ca-c^2a^2)^2-(2a+c^2)^2}.$$

Hence the values of the required numbers are as follows: $z=\frac{2bx-1}{b^2}$, (in which the value of b is to be found from the last equation,) $x=\frac{a^2+2c}{4ca-c^2a^2}$, and $y=\frac{2a+c^2}{4ca-c^2a^2}$.

The numbers a, c, and e, are to be so assumed that x, y, and z, may come out positive. If $a=1$, $c=2$, and $e=2$, then will $x=\frac{5}{4}$, $y=\frac{3}{2}$, and $z=\frac{14}{9}$, which numbers will be found upon trial to satisfy the question. It may also be observed that c and a being positive, ca must not exceed 4; but the form of the above expressions for x, y, z, will be sufficient to direct us how a, c, and e, are to be assumed.

MISCELLANEOUS.

ART. XXI. *An Account of several Ancient Mounds, and of two Caves, in East Tennessee, by* MR. JOHN HENRY KAIN, *of Knoxville.*

(Communicated for the American Journal of Science, &c.)

Mounds.

ON the plantation of Mr. John Kain of Knox county, near the north bank of the Holston River, 5 miles above its junction with the French Broad, is a curious collection of mounds of earth, evidently the work of art, but of an almost antediluvian antiquity, if we may form any conjecture of their age, from that of the forest which grows around and upon them. They are about half a dozen in number, and arise on about half an acre of level ground without any seeming regularity. They are pyramidal in their shape, or rather sections of pyramids, whose bases are from 10 to 30 paces in diameter. The largest one in this group rises about 10 feet above the level ground, and is remarkably regular in its figure. A perpendicular section of this mound was made about a year since, but no important discovery was made. It was found to consist of the surface thrown up, and contained a good deal of ashes and charcoal.

This group of mounds is surrounded by a ditch, which can be distinctly traced on three sides, and enclosing besides the mounds, several acres of ground. It is like the mounds covered with trees, which grow in it and about it. At every angle of this ditch, it sweeps out into a semicircle, and it appears in many respects well calculated for defence.

There are many other mounds of the same form in Tennessee. At the junction of the French Broad with the Holston, there is one in which human bones are said to have been found.

Farther up French Broad, near Newport, is a very large mound. It reposes on a very level and extensive plain, and is itself the largest I ever saw. It is thirty feet high, and its base covers half an acre of ground. As it ascends from its base, there is a slight inclination from a perpendicular on all sides, and the upper surface is as level as the rest is regular. From the great size of this mound, its commanding situation, and the mystery which veils its history, it is a most interesting spot of ground. There are many other mounds of this description in the State of Tennessee, but I have not visited them.

Though not immediately connected with this subject, I take the liberty to subjoin an account of a remarkable cave or grotto, in a bluff of limestone, on the south bank of the Holston River, opposite the mounds first described. The bluff is perhaps 100 feet high and 50 wide. The grotto is a large natural excavation of the rock, 60 feet high and 30 feet wide. It is very irregular, and to the very top bears marks of the attrition of waves. The river to have been so high, must have covered the valley through which it now winds its quiet way. The excavation gradually diminishes in size as you proceed backward, till at 100 feet from the entrance, it terminates. A remarkable projection of the rock divides the back part into two stories. This grotto, whose walls are hung with ivy, and the bluff crowned with cedars, and surrounded by an aged forest, on which the vine clambers most luxuriantly, viewed from the river which winds slowly around it, and reflects its image, is more than beautiful: it is even venerable. But what renders it most interesting to many visiters, is a number of rude paintings, which were, as tradition reports, left on it by the Cherokee Indians. These Indians are known to have made this cave a resting-place, as they passed up and down the River Holston. These paintings are still distinct, though they have faded somewhat within my remembrance. They consist of representations of the sun and moon, of a man, of birds, fishes, &c. They are all of red paint, and resemble in this respect, the paintings on Paint Rock near the warm springs.

Much has been said of the objects of curiosity in the country north of us; and I took the liberty to describe some of them

in my preceding communication. Indeed we may say, without danger of exaggeration, that the range of Alleghany Mountains presents a variety of the most curious features, and many objects of beauty and sublimity. I have noticed a few of the most prominent, but "the half is not told."

Extract of a Letter, &c.

Knoxville, Nov. 24, 1818.

I WAS on a visit to a friend a few days since, about 30 miles to the north of this, and was invited by him to visit an interesting curiosity in the neighbourhood. We crossed the Clynch River where it is much confined by mountains, and banks as high as mountains. Our guide conducted us to the foot of a steep declivity, where we left our horses, and with some difficulty ascended about 70 yards. Here we came to the mouth of a cave which had been stopped up by a stone wall. The wall was made of limestone and mortar, which is now harder than the stone itself. It is, without a doubt, artificial, for besides the evidence afforded by its structure, it contains bones and animal remains.

What was this wall built for? There was a tradition among the inhabitants that it contained money, and they were much disappointed on opening it, not to find any. Like other caves, it contains a variety of calcareous concretions, and I obtained some fine specimens of brown spar, which I will take the first opportunity to send you.

I remain your Friend,

JOHN H. KAIN.

N. B. This wall is 10 feet thick.

For the American Journal of Science, &c.

BENJAMIN SILLIMAN, ESQ.

Dear Sir,

SHOULD you think the facts detailed in the following statement worthy of publication, you are at liberty to publish them. The knowledge of the first, I derived in the year 1802, from a gentleman and a lady, both inhabitants of the town where the person whose case is detailed, lived: of the third in 1802, from the same lady: and of the second in 1802, from a lady, a near relative of Mrs. S. When the facts were communicated to me, I immediately committed them to writing, and to avoid mistakes, read what I had written to the persons communicating them.

I am very respectfully,
Your Friend, and obedient Servant,
BENJAMIN W. DWIGHT.

ART. XXII. *Facts illustrative of the Powers and Operations of the Human Mind in a Diseased State.*

1. SOME years ago a farmer of fair character, who resided in an interior town in New England, sold his farm, with an intention of purchasing another in a different town. His mind was naturally of a melancholy cast. Shortly after the sale of his farm, he was induced to believe that he had sold it for less than its value. This persuasion brought on dissatisfaction, and eventually a considerable degree of melancholy. In this situation, one of his neighbours engaged him to enclose a lot of land, with a post and rail fence, which he was to commence making the next day. At the time appointed he went into the field, and began with a beetle and wedges to split the timber, out of which the posts and rails were to be prepared. On finishing his day's work, he put his beetle and wedges into a hollow tree, and went home. Two of his sons had been at work through the day in a distant part of the same field. On

his return, he directed them to get up early the next morning, to assist him in making the fence. In the course of the evening he became delirious, and continued in this situation several years; when his mental powers were suddenly restored. The first question which he asked after the return of his reason, was, whether his sons had brought in the beetle and wedges. He appeared to be wholly unconscious of the time that had elapsed from the commencement of his delirium. His sons, apprehensive that any explanations might induce a return of his disease, simply replied that they had been unable to find them. He immediately arose from his bed, went into the field where he had been at work a number of years before, and found the wedges, and the rings of the beetle, where he had left them, the beetle itself having mouldered away. During his delirium, his mind had not been occupied with those subjects with which it was conversant in health.

2. Mrs. S., an intelligent lady, belonging to a respectable family in the State of New-York, some years ago undertook a piece of fine needlework. She devoted her time to it almost constantly for a number of days. Before she had completed it, she became suddenly delirious. In this state, without experiencing any material abatement of her disease, she continued for about seven years; when her reason was suddenly restored. One of the first questions which she asked after her reason returned, related to her needlework. It is a remarkable fact, that during the long continuance of her delirium she said nothing, so far as was recollected, about her needlework, nor concerning any such subjects as usually occupied her attention when in health.

3. A lady in New England, of a respectable family, was for a considerable period subject to paroxysms of delirium. These paroxysms came on instantaneously, and after continuing an indefinite time, went off as suddenly; leaving her mind perfectly rational. It often happened that when she was engaged in rational and interesting conversation, she would stop short in the midst of it, become in a moment entirely delirious, and commence a conversation on some other subject, not having the remotest connexion with the previous one, nor would she ad-

vert to that during her delirium. When she became rational again, she would pursue the same conversation in which she had been engaged during the lucid interval, beginning where she had left off. To such a degree was this carried, that she would complete an unfinished story or sentence, or even an unfinished word. When her next delirious paroxysm came on, she would continue the conversation which she had been pursuing in her preceding paroxysm; so that she appeared as a person might be supposed to do, who had two souls, each occasionally dormant, and occasionally active, and utterly ignorant of what the other was doing.

INTELLIGENCE.

ART. XXIII. 1. *Discovery of American Cinnabar and Native Lead.*

Extract of a letter from Dr. Comstock of Hartford, to the Editor.

SIR,

IN answer to your inquiry concerning the discovery of sulphuret of mercury and native lead in this country, I send you the following summary of a letter I received from B. F. Stickney, Esq. Indian agent, dated Fort Wayne, Dec. 1, 1818.

Mr. Stickney states, that the situation of Fort Wayne, and the country surrounding, is a high level, probably about 800 feet above the sea. From this place the water-courses divide and take different directions, on the one hand falling into the Gulf of Mexico, and on the other into the Bay of St. Lawrence. The whole country is of secondary formation, chiefly calcareous and aluminous.

Bitumen and sulphur are every where to be found, and as usual, accompanied by the metals.

In speaking of the cinnabar, his words are, "I have found a black and garnet-coloured sand, in great abundance on the

shores of the Lakes Erie and Michigan, this is a sulphuret of mercury, and yields about sixty per cent. It is so easy to be obtained, and in so convenient a form for distillation, that it must become an important article of commerce."

The native lead was found on the Anglaize River, at a considerable distance from the fort.

Of this he says, "metallic lead is so interspersed with galena, as to prove incontestably the existence of native lead."

Respectfully,

Your obedient Servant,

J. L. COMSTOCK.

Hartford, Conn. Feb. 17, 1819.

Benjamin Silliman, M. D., &c.

2. *Theoretical views of Professor Hare of Philadelphia.*

We are authorized to mention, that Dr. Robert Hare has taught in his lectures during the last eighteen months, that acid properties never appearing in the absence of water, this fluid or its elements are most entitled to be considered as the acidifying principle: but that probably it does not exist in acids as water, but is decomposed when added to them, the particles of hydrogen and oxygen by their different polarities taking opposite sides of those composing the base. The extrication of hydrogen by the action of diluted sulphuric acid on iron or zinc, being the consequence of a previous, not simultaneous decomposition of water. Hence when sulphuric or nitric acids are so concentrated as to char or ignite, they are not acids really.

3. *New Work on Chemistry.*

Dr. John Gorham of Boston, Professor of Chemistry in Harvard University, &c. has published the first volume of his Elements of Chemical Science. The work will be comprised in two volumes, and its completion will be anticipated with interest by the scientific public.

4. *Botanical.*

Dr. Romer of Zurich, has begun, since 1815, to publish a new edition of the Systema Vegetabilium of Linnæus ; he proceeds in its publication ; it will form several volumes.

Robert Brown of London, is endeavouring to group the natural orders of plants into natural classes, or rather into larger natural orders, with determinate characters : he has communicated some parts of his labour to the botanists of Paris. He has been the first to employ as a new character in the distinction of natural orders, the estivation of flowers, or the manner in which they are folded in the buds.

C. S. Rafinesque, in his Analysis of Nature, has adopted a new practice, that of giving single substantive Latin names to the natural orders and families of plants.

Mirbel has proposed a new nomenclature of fruits in his Elements of Botany.

Decandolle, after publishing the principles of the science in his Theory of Botany, has begun to undertake a general species plantarum, according to the natural classification.

Three splendid Floras of the south of Europe have been undertaken. 1. Flora Græca, by Sibthorp and Smith in England. 2. Flora Lusitanica, by Link and Hoffmansegg in Germany. 3. Flora Nepolitana, by Tenore in Naples. They are very expensive works, and are not yet terminated. *Received in January*, 1819.

5. *Staurotide.*

Extract of a letter to the Editor, from John Torrey, M.D., of New-York.

" Mr. Pierce and myself lately found staurotide on the island of New-York. It occurs in considerable quantity in a rock of *mica slate*, on the banks of the Hudson, about three and a half miles from the city. The crystals very seldom form the perfect cross, though many were found, intersecting each other imperfectly at angles of 60°. Several single crystals were obtained exceedingly perfect. They were short 4-sided prisms, with the acute lateral edges truncated at each ex-

tremity on the two solid angles of the most obtuse lateral edges, forming diedral terminations at each extremity of the prism. The faces of these terminations were inclined to each other at an angle of 67° and a few minutes. The annexed figure shows the form of the crystal.



6. *Supplement to the "Remarks on the Geology and Mineralogy of a Section of Massachusetts, on Connecticut River, &c." contained in No. 2, Art. I, of this Journal, by* E. Hitchcock, A. M.

The following minerals, found in the region above named, were either omitted in the former list, or have been noticed since that was made out.

Bog-iron Ore. In Greenfield and Warwick.

Hornstone. Rare; in Deerfield and Conway.

Silicious Slate. In rolled pieces, on the banks of Deerfield river; not abundant.

Basanite, or *Lydian Stone.* Same locality.

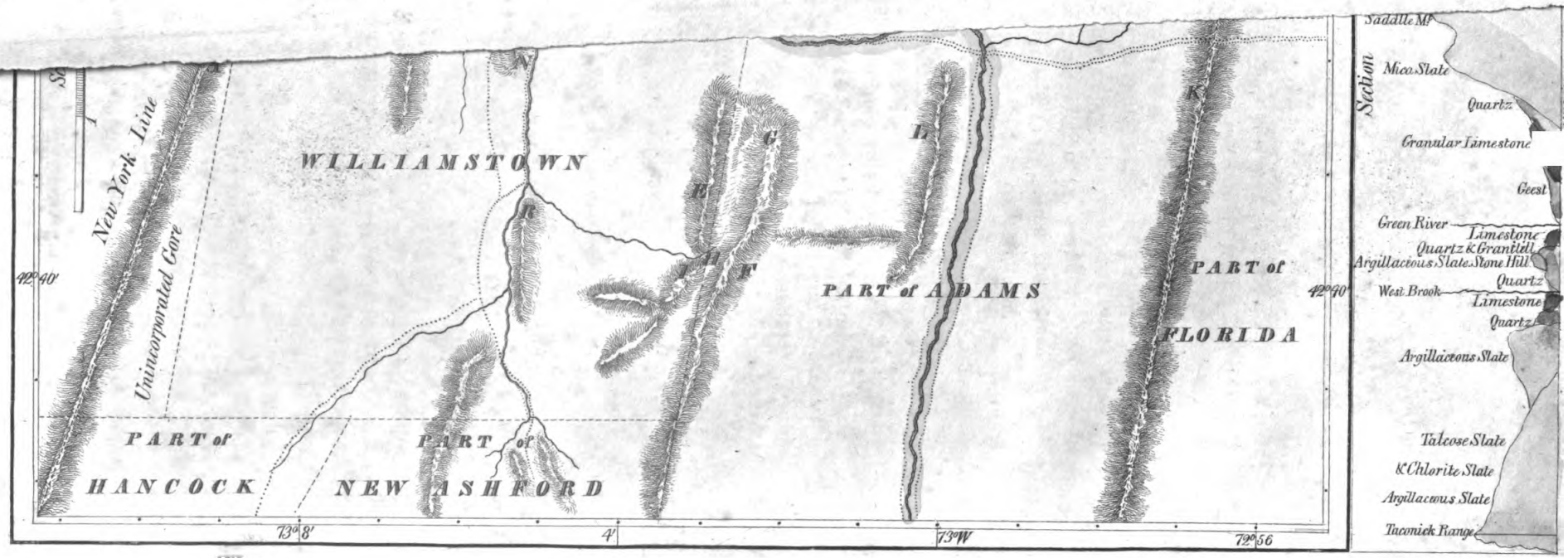
Augite. In an aggregate of greenstone, quartz, and calcareous spar, in the greenstone range, Deerfield. Colour black, and the crystals usually imperfect, or broken.

Staurotide. In mica slate, Northfield, one mile east of the village, on the turnpike to Boston. The crystals observed were six-sided prisms. The same rock contains reddish garnets.

THE LEVERETT RANGE OF GRANITE.

This name is given to a granite range that emerges from the puddingstone near the centre of Amherst, and extends northerly, with some interruption, nearly thirty miles, through Leverett and Montague to Northfield. And, indeed, there is some reason to suppose that it again appears to the north of Northfield. The range is widest in Leverett, where its breadth is more than a mile. It is noticed in the "Remarks," No. 2, Art. I, of this Journal, and may be seen on the section

A GEOLOGICAL MAP OF THE NOR



accompanying that communication. But on further examination it has been found to be more extensive than was supposed. The texture of the rock is coarse. Plates of mica, 3 or 4 inches across, are common in it; and one specimen of a beautiful blue feldspar, the fragment only of a crystal, measured in one direction 8 inches.

Two circumstances in this range give it an interest in the eye of a geologist. The one is its proximity to sandstone and puddingstone; and the other, its small elevation in comparison with the surrounding rocks of later formations. In some places no other rock could be found lying between the granite and puddingstone; though the soil prevented my observing whether there is an actual contact. But in general there is a stratum of mica slate a few rods wide between these rocks, and not unfrequently gneiss lies between the mica slate and granite.

Standing on this range in Leverett, you have on the west, at about 100 rods distant, a precipitous mountain of sandstone and puddingstone, five or six hundred feet higher than the granite. On the east, a mile or two distant, a mountain of sienite gradually rises to a still greater height than the puddingstone; and on the southwest, at nearly the same distance, you can see an alluvial formation. In general this granite does not rise so high as the adjacent rocks, whether secondary or primitive.

VEINS OF ORE IN THIS GRANITE.

1. *Of Galena in Leverett.*

This ore forms a narrow vein in the southwest part of the town, on land of Moses Smith, two miles from the Congregational meeting-house. The direction of the vein is nearly north and south, and where I saw it, only a foot wide. The gangue is sulphate of barytes.

2. *Of Galena, Copper Pyrites, and Blende.*

This vein is a little more than a mile north of the one above described, and it may be a continuation of the same

vein. The gangue is nearly an equal admixture of sulphate of barytes and quartz; and galena and sulphuret of copper are disseminated through it in about the same, that is, equal proportions. The blende, which is of a yellowish aspect when the fractured crystal is held in a certain position, appears only occasionally. This vein is several feet wide, has been wrought to a small extent in two places, and its direction is nearly north and south. It is on land of Mr. Field.

Radiated quartz. In the above vein. A considerable tendency to crystallization appears at this place, not only in the quartz, but in the foliated structure of the barytes.

Brown spar. In the same place. But little of this mineral was noticed. It exfoliated before the blowpipe, turned black, and became magnetic.

3. *Of Specular Oxide of Iron in Montague.*

This is found in a partially detached eminence, 100 feet high, near the north line of Montague, on land of Mr. Taft, a little southwest from the confluence of Miller's river with the Connecticut. The whole hill, not less than 100 rods in circumference at its base, is traversed by numerous veins of this ore; and scarcely a foot of the rock is to be seen that does not contain these, varying in width from a mere line to several inches. The principal vein appears on the top of the hill; and is, as nearly as I could determine, not less than ten feet wide, lying in a north and south direction. The ore seems to be abundant, and generally pure. Masses, that have been separated by blasting, and weighing from 100 to 200 pounds, lie on the surface. A small proportion of sulphuret of iron was observed in some specimens. The gangue is quartz, and the walls and hill granite.

No opinion is here intended to be offered concerning the probable value of these ores, if worked. If they be useless to the present generation, they may not be so to some future one, when labour shall be cheaper; and therefore it was thought to be of some consequence to point out their localities.

In the remarks, to which this paper is a supplement, *blue quartz* was inadvertently put down among the minerals found

in Deerfield. I presume it does not exist there. It is also probable that the variety of garnets found in Conway, is not, as formerly stated, the melanite.

7. *New Process for Tanning.*

A process for effecting the tanning of leather in a neat, expeditious, and thorough manner, has been discovered by a Mr. Steel, of Connecticut: some account of it may be given hereafter.

8. *Connexion between Chemistry and Medicine.*

This subject has been discussed in an able and interesting manner by Professor Cooper, of Philadelphia, in a public discourse, which has now been some months before the public.

9. *Brucite.*

A new Species in Mineralogy, discovered by the late Dr. Bruce. We hope to publish in the next Number a description and analysis of it.

10. *Lithography.*

We are promised for our next Number, a full account of this art, of which we have received a beautiful specimen, *A Minerva*, executed by Mr. Bates Otis, an ingenious and enterprising artist of Philadelphia, who, under the patronage of Dr. Samuel Brown, is preparing to disseminate the productions of his skill, and to make this important art (executed with American materials,) extensively useful in this country.

N. B. As this number has already much exceeded its proper size, we are obliged to suppress many articles of domestic, and all those of foreign intelligence.

CONCLUSION.

IN the prospectus of this work, the expectation was expressed that each Number would contain from 64 to 80 pages ; that as many as four Numbers might be issued within the year, and engravings were promised for such subjects as might require them.

The Numbers published, have actually contained from 104 to 132 pages, the four have been issued within a period of ten months, and twelve copper-plate engravings and several wood-cuts, illustrate the present volume.

Of the subjects proposed in the plan of the work, our pages contain notices, more or less extensive, of Geology, Mineralogy, Botany, Zoology, Chemistry, Natural Philosophy, Mathematics, Useful Arts, Fine Arts, Inventions, Reviews, Biography, and Intelligence. How far then we have redeemed our pledge, we leave it for our readers to decide.

In the commencement of an enterprise, for the first time attempted in this country, an enterprise arduous in its nature and uncertain in its issue, it will not be doubted that considerable solicitude was experienced.

To concentrate American efforts in science and the arts, by furnishing a Journal to record their proceedings, will, in our view, not only have a direct influence in promoting the honour and prosperity of the nation as connected with its physical interests, but will also tend in no small degree to nourish an enlarged patriotism, by winning the public mind from the odious asperities of party. That entire success will attend our efforts, it would perhaps be presumptuous to expect, but we trust that the interesting previous question, whether such a work can be adequately sustained, by appropriate materials, may be considered as now decided. The support which we have received, and for which we are deeply grateful, has been far beyond our most sanguine hopes, and has caused us to dispense with no small portion of those less important efforts of our own, with which we were prepared to succour our infant undertaking.

If we may be allowed to express a wish relative to the nature of future communications, it would be, that those of a scientific nature should not be diminished, while those relating to the arts, to agriculture, and to domestic economy, should be increased; we particularly solicit the communications of practical men, versed in the useful and ornamental arts, and they will be acceptable should they not even be clothed in a scientific dress.

Arrangements have been made for the reception of an increased number of the best European Journals, both from the continent and from Britain; they have already begun to arrive, and we hope to give in future numbers, more full details of foreign scientific intelligence, although it is true that this species of information has hitherto been stinted, not from poverty of materials, but from the pressure of original American communications.

In justice to the publishers of this work, we add, that *this publication is an expensive one;* very heavy advances have been already made by them, while only a trivial amount has been received in return. It is hoped, therefore, that subscribers will promptly remit, *free from postage*, the small stipulated sum, and also make the required advance for the succeeding volume. This last is not due till the first number of that volume has been issued, but it would save postage to remit both sums at once, and thus also it will be known what subscriptions are continued. In a subscription so widely dispersed over a large portion of the United States, an inattention to *punctual payment*, must soon put in hazard the existence of a work, having otherwise the fairest prospects of continuance, and we hope of usefulness.

Should this appeal be promptly answered, the first number of the next volume (already in considerable forwardness,) will be published in the course of the summer; and should men of ability continue to furnish communications, and *the public be*

willing to pay for the work, it is our wish to publish future numbers with greater frequency, and to complete our volumes whenever we are prepared, without confining ourselves to particular periods of time.

New-Haven, Conn. May 17, 1819.

POSTSCRIPT.

American geological society.

We have the pleasure to announce, that an American Geological Society has been recently organized by an association of gentlemen, residing in various parts of the United States. An Act of Incorporation, conferring the necessary powers, has been granted by the Legislature of Connecticut, and farther accounts of the plan and progress of the Society may be expected in future numbers of this work.

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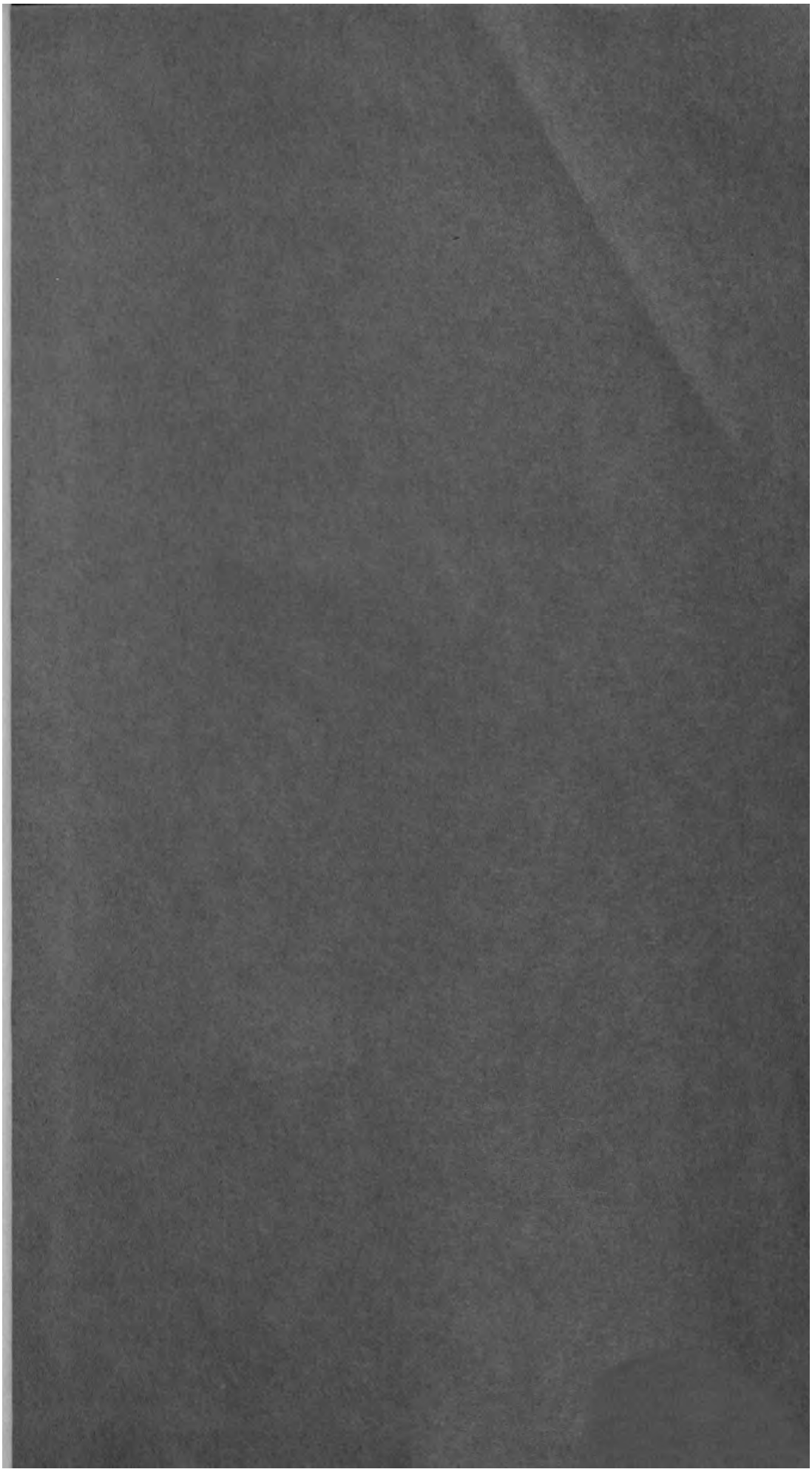
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ERRATA.

P. 101, l. 4, for "ethyl," read "silver."
" 102, l. 13, for "43·5," read "44·8" (as the half sum of equivalents U and Ni.)
" 102, l. 25, for "Mg," read "Mn."
" 107, l. 8, for "C," read "B."
" 110, l. 2 from bottom, for "Co," read "Cr."
" 111, l. 1, for "No," read "Nb."
" 272, l. 8 from bottom, for "HENTZ," read "HEINTZ."

Vol. XXVIII, p. 135, l. 33 from top, for Vol. VI, read Vol. IV.—P. 356, line 20 from bottom, after the word "cultivation," insert a period, putting a capital W for the word "where." Same page, line 19 from bottom, omit "then."—P. 357. line 10 from top, for "deposition of sand," read "deposition of mud."

THE

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[SECOND SERIES.]

ART. I.—*On the Origination and Distribution of Species:—Introductory Essay to the Flora of Tasmania;* by Dr. JOSEPH D. HOOKER.*

§ 1. *Preliminary Remarks.*

THE Island of Tasmania does not contain a vegetation peculiar to itself, nor constitute an independent botanical region. Its plants are, with comparativelv few exceptions, natives of extra-tropical Australia; and I have consequently found it necessary to study the vegetation of a great part of that vast continent, in order to determine satisfactorily the nature, distribution, and

* *To the Editors of the American Journal of Science, &c.:*—The sheets of this *Introductory Essay*, having been obligingly communicated to me in advance of the publication of the concluding part of the *Flora of Tasmania*, to which it belongs, I asked and have received the distinguished author's permission to reprint them, or a considerable portion of them, in your Journal, and now offer them for that purpose. This is in order that we may have before us, at the earliest date, an essay which cannot fail to attract the immediate and profound attention of scientific men; but which, if confined to the pages of the *Flora of Tasmania*, would be seen by very few American readers. To those who have intelligently observed the course of scientific investigation, and the tendency of speculation, it has for some time been manifest that a re-statement of the Lamarkian hypothesis is at hand. We have this, in an improved and truly scientific form, in the theories which, recently propounded by Mr. Darwin, followed by Mr. Wallace, are here so ably and altogether independently maintained. When these views are fully laid before them, the naturalists of this country will be able to take part in the interesting discussion which they will not fail to call forth.

To save room, a few paragraphs are omitted which do not directly bear upon the subject in hand. A. G.

affinities of the Tasmanian Flora. From the study of certain extratropical genera and species in their relation to those of Tasmania, I have been led to the far more comprehensive undertaking of arranging and classifying all the Australian plants accessible to me. This I commenced in the hope of being able thereby to extend our knowledge of the affinities of its Flora, and, if possible, to throw light on a very abstruse subject, viz. the origin of its vegetation, and the sources or causes of its peculiarity. This again has induced me to proceed with the inquiry into the oxigin and distribution of existing species; and, as I have already treated of these subjects in the Introduction to the New Zealand Flora, I now embrace the opportunity afforded me by a similar Introduction to the Tasmanian Flora, of revising the opinions I then entertained, and of again investigating the whole subject of the creation of species by variation, with the aid of the experience derived from my subsequent studies of the Floras of India and Australia in relation to one another and to those of neighboring countries, and of the recently published hypotheses of Mr. Darwin and Mr. Wallace. * * *

In the Introductory Essay to the New Zealand Flora, I advanced certain general propositions as to the origin of species, which I refrained from endorsing as articles of my own creed: amongst others was the still prevalent doctrine that these are, in the ordinary acceptation of the term, created as such, and are immutable. In the present Essay I shall advance the opposite hypothesis, that species are derivative and mutable; and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision since the recent publication by the Linnean Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.

Further, there must be many who, like myself, having hitherto refrained from expressing any positive opinion, now, after a careful consideration of these naturalists' theories, find the aspect of the question materially changed, and themselves freer to adopt such a theory as may best harmonize with the facts adduced by their own experience.

The Natural History of Australia seemed to me to be especially suited to test such a theory, on account of the comparative uniformity of its physical features being accompanied with a great variety in its Flora; of the differences in the vegetation of its several parts; and of the peculiarity both of its Fauna and Flora, as compared with those of other countries. I accordingly prepared a classified catalogue of all the Australian species in the Herbarium, with their ranges in longitude, latitude, and elevation, as far as I could ascertain them, and added what fur-

ther information I could obtain from books. At the same time I made a careful study of the affinities and distribution of all the Tasmanian species, and of all those Australian ones which I believed to be found in other countries. I also determined as accurately as I could the genera of the remainder, and especially of those belonging to genera which are found in other countries, and I distinguished the species from one another in those genera which had not been previously arranged. In this manner I have brought together evidence of nearly 8000 flowering plants having been collected or observed in Australia, of which I have seen and catalogued upwards of 7000. About two-thirds of these are ascertained specifically with tolerable accuracy, and the remainder are distinguished from one another, and referred to genera with less certainty, being either undescribed, or described under several names, whilst some are members of such variable groups that I was left in doubt how to dispose of them.

To many who occupy themselves with smaller and better worked botanical districts, such results as may be deduced from the skeleton Flora I have compiled for Australia may seem too crude and imperfect to form data from which to determine its relations. But it is not from a consideration of specific details that such problems as those of the relations of Floras and the origin and distribution of organic forms will ever be solved, though we must eventually look to these details for proofs of the solutions we propose. The limits of the majority of species are so undefinable that few naturalists are agreed upon them;* to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinions; the progress of systematic science being, in short, obviously unfavorable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation. On the other hand, when dealing with genera, or other combinations of species, all that is required is that these be classified in natural groups; and that such groups are true exponents of affinities settled by Nature is abundantly capable of demonstration. It is to an investigation of the extent, relations, and proportions of these natural combinations of species, then, that we must look for the means of obtaining and expressing the features of a Flora; and if in this instance the exotic species are well ascertained, it matters little whether or not the endemic are in all cases accurately distinguished from one another. Further, in a Flora so large as that of Australia, if the species are limited and

* The most conspicuous evidence of this lies in the fact, that the number of known species of flowering plants is by some assumed to be under 80,000, and by others over 150,000.

estimated by one mind and eye, the errors made under each genus will so far counteract one another, that the mean results for the genera and orders will scarcely be affected. As it is, the method adopted has absorbed many weeks of labor during the last five years, and a much greater degree of accuracy could only have been obtained by a disproportionately greater outlay of time, whilst it would not have materially affected the general results.

With regard to my own views on the subjects of the variability of existing species and the fallacy of supposing we can ascertain anything through these alone of their ancestry or of originally created types, they are, in so far as they are liable to influence my estimate of the value of the facts collected for the analysis of the Australian Flora, unaltered from those which I maintained in the 'Flora of New Zealand.' On such theoretical questions, however, as the origin and ultimate permanence of species, they have been greatly influenced by the views and arguments of Mr. Darwin and Mr. Wallace above alluded to, which incline me to regard more favorably the hypothesis that it is to variation that we must look as the means which Nature has adopted for peopling the globe with those diverse existing forms which, when they tend to transmit their characters unchanged through many generations, are called species. Nevertheless I must repeat, what I have fully stated elsewhere, that these hypotheses should not influence our treatment of species, either as subjects of descriptive science, or as the means of investigating the phenomena of the succession of organic forms in time, or their dispersion and replacement in area, though they should lead us to more philosophical conceptions on these subjects, and stimulate us to seek for such combinations of their characters as may enable us to classify them better, and to trace their origin back to an epoch anterior to that of their present appearance and condition. In doing this, however, the believer in species being lineally related forms must employ the same methods of investigation and follow the same principles that guide the believer in their being actual creations, for the latter assumes that Nature has created species with mutual relations analogous to those which exist between the lineally-descended members of a family, and this is indeed the leading idea in all natural systems. On the other hand, there are so many checks to indiscriminate variation, so many inviolable laws that regulate the production of varieties, the time required to produce wide variations from any given specific type is so great, and the number of species and varieties known to propagate for indefinite periods a succession of absolutely identical members is so large, that all naturalists are agreed that for descriptive purposes species must be treated as if they were at their origin distinct,

and are destined so to remain. Hence the descriptive naturalist who believes all species to be derivative and mutable, only differs in practice from him who asserts the contrary, in expecting that the posterity of the organism he describes as species may, at some indefinitely distant period of time, require redescription.

I need hardly remark that the classificatory branch of Botany is the only one from which this subject can be approached; for a good system must be founded on a due appreciation of all the attributes of individual plants,—upon a balance of their morphological, physiological, and anatomical relations at all periods of their growth. Species are conventionally assumed to represent, with a great amount of uniformity, the lowest degree of such relationship; and the facts that individuals are more easily grouped into species limited by characters, than into varieties, or than species are into limitable genera or groups of higher value, and that the relationships of species are transmitted hereditarily in a very eminent degree, are the strongest appearances in favor of species being original creations, and genera, etc., arbitrarily limited groups of these.

The difference between varieties and species and genera in respect to definable limitation is however one of degree only, and if increased materials and observation confirm the doctrine which I have for many years labored to establish, that far more species are variable, and far fewer limitable, than has been supposed, that hypothesis will be proportionally strengthened which assumes species to be arbitrarily limited groups of varieties. With the view of ascertaining how far my own experience in classification will bear out such a conclusion, I shall now endeavor to review, without reference to my previous conclusions, the impressions which I have derived from the retrospect of twenty years' study of plants. During that time I have classified many large and small Floras, arctic, temperate and tropical, insular and continental: embracing areas so extensive and varied as to justify, to my apprehension, the assumption that the results derived from these would also be applicable to the whole vegetable kingdom. I shall arrange these results successively under three heads; viz., facts derived from a study of classification; secondly, from distribution; thirdly, from fossils; after which I shall examine the theories with which these facts should harmonize.

§ 2. *On the General Phenomena of Variation in the Vegetable Kingdom.*

1. All vegetable forms are more or less prone to vary as to their sensible properties, or (as it has been happily expressed in regard to all organisms), "they are in a state of unstable equilibrium."* No organ is exactly symmetrical, no two are exact

* Essays: Scientific, Political, and Speculative; by Herbert Spencer: p. 280.

counterparts, no two individuals are exactly alike, no two parts of the same individual exactly correspond, no two species have equal differences, and no two countries present all the varieties of a species common to both, nor are the species of any two countries alike in number and kind.

2. The rate at which plants vary is always slow, and the extent or degree of variation is graduated. Sports even in color are comparatively rare phenomena, and, as a general rule, the best-marked varieties occur on the confines of the geographical area which a species inhabits. Thus the scarlet Rhododendron (*R. arboreum*) of India inhabits all the Himalaya, the Khasia Mountains, the Peninsular Mountains, and Ceylon; and it is in the centre of its range (Sikkim and the Khasia) that these mean forms occur which by a graduated series unite into one variable species, the rough, rusty-leaved form of Ceylon, and the smooth, silvery-leaved form of the northwestern Himalaya. A white and a rose-colored sport of each variety is found growing with the scarlet in all these localities, but everywhere these sports are few in individuals. Also certain individuals flower earlier than others, and some occasionally twice a year, I believe in all localities.

3. I find that in every Flora all groups of species may be roughly classified into three large divisions: one in which most species are apparently unvarying; another in which most are conspicuously varying; and a third which consists of a mixture of both in more equal proportions. Of these the unvarying species appear so distinct from one another that most botanists agree as to their limits, and their offspring are at once referable by inspection to their parents; each presents several special characters, and it would require many intermediate forms to effect a graduated change from any one to another. The most varying species, on the contrary, so run into one another, that botanists are not agreed as to their limits, and often fail to refer the offspring with certainty to their parents, each being distinguished from one or more others by one or a few such trifling characters, that each group may be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus*, *Rosa*, *Salix*, and *Saxifraga*, afford conspicuous examples of these unstable species; *Veronica*, *Campanula*, and *Lobelia*, of comparatively stable ones.

4. Of these natural groups of varying and unvarying species, some are large and some small; they are also variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which

are the simplest in structure.* Complexity of structure is generally accompanied with a greater tendency to permanence in form: thus Acotyledons, Monocotyledons, and Dicotyledons are an ascending series in complexity and in constancy of form. In Dicotyledons, *Salices*, *Urticeæ*, *Chenopodiaceæ*, and other orders with incomplete or absent floral envelops, vary on the whole more than *Leguminosæ*, *Lythraceæ*, *Myrtaceæ*, or *Rosaceæ*, yet members of these present, in all countries, groups of notoriously varying species, as *Eucalyptus* in Australia, *Rosa* in Europe, and *Lotus*, *Epilobium*, and *Rubus* in both Europe and Australia. Again, even genera are divided: of the last named, most or all of the species are variable; of others, as *Epacris*, *Acacia*, and the majority of such as contain upwards of six or eight species, a larger or smaller proportion only are variable. But the prominent fact is, that this element of mutability pervades the whole vegetable kingdom; no class nor order nor genus of more than a few species claims absolute exemption, whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.

5. The above remarks are equally applicable to all the higher divisions of plants. Some genera and orders are as natural, and as limitable by characters, as are some species; others again, though they contain many very well-marked subordinate plans of construction, yet are so connected by intermediate forms with otherwise very different genera or orders, that it is impossible to limit them naturally. And as some of the best marked and limited species consist of a series of badly marked and illimitable varieties, so some of the most natural† and limitable orders

* Mr. Darwin, after a very laborious analysis of many Floras, finds that the species of large genera are relatively more variable than those of small; a result which I was long disposed to doubt, because of the number of variable small genera and the fact that monotypic genera seldom have their variations recorded in systematic works, but an examination of his data and methods compels me to acquiesce in his statement. It has also been remarked (Bory de Saint-Vincent, Voy. aux Quatre Iles de l'Afrique) that the species of islands are more variable than those of continents, an opinion I can scarcely subscribe to, and which is opposed to Mr. Darwin's facts, inasmuch as insular Floras are characterized by peculiar genera, and by having few species in proportion to genera. Bisexual trees and shrubs are generally more variable than unisexual, which however is only a corollary from what is stated above regarding plants of simple structure of flower. On the whole, I think herbs are more variable than shrubby plants, and annuals than perennials. It would be curious to ascertain the relative variableness of social and scattered plants. The individuals of a social plant, in each area it is social upon, are generally very constant, but individuals from different areas often differ much. The *Pinus sylvestris*, *Mughus*, and *uncinata* are cases in point, if considered as varieties of one; as are the Cedars of Atlas, Algeria, and the Himalaya.

† It should be borne in mind that the term *natural*, as applied to orders or other groups, has often a double significance; every natural order is so in the sense of each of its members being more closely related to one or more of its own group than to any of another; but the term is often used to designate an easily limited natural order, that is, one whose members are so very closely related to each other by conspicuous peculiarities that its differential characters can be expressed, and itself

and genera may respectively consist of only undefinable groups of genera or of species. For instance, both *Gramineæ* and *Compositæ* are, in the present state of our knowledge, absolutely limited orders, and extremely natural ones also; but their genera are to a very eminent degree arbitrarily limited, and their species extremely variable. *Orchideæ* and *Leguminosæ* are also well-limited orders (though not so absolutely as the former), but they, on the contrary, consist of comparatively exceedingly well-marked genera and species. *Melanthaceæ* and *Scrophularineæ*, on the other hand, are not limitable as orders, and contain very many differently constructed groups; but their genera, and to a great extent their species also, are well-marked and limitable. The circumstance of a group being either isolated or having complex relations, is hence no indication of its members having the same characters.

Again, as with species, so with genera and orders, we find that upon the whole those are the best limited which consist of plants of complex floral structure: the orders of Dicotyledons are better limited than those of Monocotyledons, and the genera of Dichlamydeæ than those of Achlamydeæ.*

always recognized; these may be called *objective* orders; *Orchideæ* and *Gramineæ* are examples. Any naturalist, endowed with fair powers of observation and generalization, recognizes the close affinity between a pseudobulbous epiphytical, and a terrestrial tuberous-rooted Orchid, or between the Bamboo and Wheat, though the differences are exceedingly great in habit and in organs of vegetation and reproduction. Other orders are as natural and may be as well limited, but having no conspicuous characters in common, and presenting many subordinate distinct plans of structure, may be regarded as *subjective*. Such are *Ranunculaceæ* and *Leguminosæ*, of which a botanist must have a special and extensive knowledge before he can readily recognize very many of their members. No degree of natural sagacity will enable an uninstructed person to recognize the close affinity of *Clematis* and *Ranunculus*, or of *Acacia* and *Cytisus*, though these are really as closely related as the Orchids and Grasses mentioned above. We do not know why some orders are subjective and some objective; but if the theory of creation by variation is a true one, we ought through it to reach a solution.

* There are too many exceptions to this to admit of our concluding at once that it is attributable to any simple and uniform law of variation; but it may be explained by assuming that the degree or amount of variation is differently manifested at different epochs in the history of the group. Thus, if a genus is numerically increasing, and consequently running into varieties, it will present a group of species with complex relations *inter se;* if, on the contrary, it is numerically decreasing, such decrease must lead to the extinction of some varieties, and hence result in the better limitation of the remainder. The application of this assumption to the fact of the best limited groups being most prevalent among the higher classes (*i. e.*, among those most complicated in their organization), would at first sight appear an argument against progression, were it not for the consideration that the higher tribes of plants have in another respect proved themselves superior, in that they have not only far surpassed the lower in number of genera and species, but in individuals, and also in bulk and stature. And lastly, as all the highest orders of plants contain numerous species and often genera of as simple organization as any of the lower orders are, it follows that that physical superiority which is manifested in greater extent of variation, in better securing a succession of race, in more rapid multiplication of individuals, and even in increase of bulk, is in some senses of a higher order than that represented by mere complexity or specialization of organ.

Now my object in dwelling on this parallelism between the characteristics of individuals in relation to species, of species in relation to genera, and of genera in relation to orders, is because I consider (Introd. Essay to Fl. N. Z.) that it is to the extinction of species and genera that we are indebted for our means of resolving plants into limitable genera and orders. This view is now, I believe, generally admitted, even by those who still regard species as the immutable units of the vegetable creation; and it therefore now remains to be seen how far we are warranted in extending it to the limitation of species by the elimination of their varieties through natural causes.*

6. The evidence of variability thus deduced from a rapid general survey of the prominent facts elicited from a study of the principles of classification, are to a certain extent tested by the behavior of plants under cultivation, which operates either by hastening the processes of nature (in rapidly inducing variation), or by effecting a prolepsis or anticipation of those processes (in producing sports, *i. e.* better marked varieties, without graduated stages), or by placing the plant in conditions to which it would never have been exposed in the ordinary course of natural events, and which eventually either kill it or give origin to a series of varieties which might otherwise have never existed.†

* It follows as a corollary to the proposition (that species, etc., are naturally rendered limitable by the destruction of varieties), that there must be some intimate relation between the rate of increase and the duration of genera (or other groups of species) on the one hand, and the limitability of their species on the other. Thus, when a genus consists of a multitude of illimitable forms, we may argue with much plausibility that it is on the increase, because no intermediates have as yet been destroyed, and that the birth of individuals and the production of new forms is proceeding at a greater proportional rate than in an equally large genus of which the species are limitable.

† My friend Mr. Wallace treats of animals under domestication, not only as if they were in very different physical conditions from those in a state of nature, inasmuch as every sense and faculty is continually fully exercised and strengthened by wild animals, whilst certain of these lie dormant in the domesticated, but also as if they were subject to the influence of fundamentally different laws. He says, "No inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals. The two are so much opposed that what applies to the one is almost sure not to apply to the other." But, in the first place, of the same species of wild animals some families must be placed where certain faculties and senses are far more exercised than others, and the difference in this respect between the conditions of many families of wild animals is as great as those between many wild and tame families; and secondly, other senses and faculties, latent and unknown in the wild animal, but which are as proper to the species as any it exercised in its wild state, are manifested or developed by it under domestication. An animal in a state of nature is not then, as Mr. Wallace assumes, "in the full exercise of every part of its organization;" were it so, it could not vary or alter with altered conditions, nor could other faculties remain to be called into play under domestication. The tendency of species when varying cannot be to depart from the original type in a wild condition and to revert to it under domestication, for man cannot invert the order of nature, though he may hasten or retard some of its processes.

7. Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which we have derived from a survey of the affinities of plants in a state of nature: a large number remain apparently permanent and unalterable, and a large number vary indefinitely. Of the permanent there is little to remark, except that they belong to very many orders of plants, nor are they always those which are permanent in a state of nature. Many plants, acknowledged by all to be varieties, may be propagated by seed or otherwise, when their offspring retains for many successive generations the characters of the variety. On the other hand, species which have remained immutable for many generations under cultivation, do at length commence to vary, and having once begun, are thereafter peculiarly prone to vary further.

8. The variable cultivated species present us with the most important phenomena for investigating the laws of mutability and permanence; but these phenomena are so infinitely varied, complex, and apparently contradictory, as to defeat all attempts to elucidate the history of any individual case of variation by a study of its phases alone. It would often appear doubtful whether the natural operations of a plant tend most to induce or to oppose variation; and we hence find the advocates of original permanent creations, and those of mutable variable species, taking exactly opposite views in this respect, the truth, I believe, being that both are right. Nature has provided for the possibility of indefinite variation, but she regulates as to extent and duration; she will neither allow her offspring to be weakened or exhausted by promiscuous hybridization and incessant variation, nor will she suffer a new combination of external conditions to destroy one of these varieties without providing a substitute when necessary; hence some species remain so long hereditarily immutable as to give rise to the doctrine that all are so normally, while others are so mutable as to induce a belief in the very opposite doctrine, which demands incessant lawless change.

9. It would take far too long a time were I to attempt any analysis of the phenomena of cultivation, as illustrative of those of variability in a state of nature. There are however some broad facts which should be borne in mind in treating of variation by cross impregnation and hybridity.

10. Variation is effected by graduated changes; and the tendency of varieties, both in nature and under cultivation, when further varying, is rather to depart more and more widely from the original type, than to revert to it: the best marked varieties of a wild species occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener. I am aware

that the prevalent opinion is that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed; and I have myself quoted this opinion, without questioning its accuracy,* as tending to support the views of those who regard species as permanent. A further acquaintance with the results of gardening operations leads me now to doubt the existence of this centripetal force in varieties, or at least to believe that in the phrase "reversion to the wild type," many very different phenomena are included. In the first place, the majority of cultivated vegetables and cerealia, such as the Cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants;† they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabi, to be all as unlike their common parent, the wild *Brassica oleracea*, as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become crabs, but in so doing they become crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has also been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the seaside are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form. This is however a subject which would fill a volume with most instructive matter for reflection, and which receives a hundredfold more illustration from the Animal than from the Vegetable Kingdom. I can here only indicate its bearing on the doctrine of variation, as evidence that Nature operates upon mutable forms by allowing great variation, and displaying

* Fl. N. Zeal., Introd. Essay, p. x, and Flora Indica, Introduction, p. 14.

† Hence the great and acknowledged difficulty of determining the wild parent species of most of our cultivated fruits, cerealia, etc., and in fact of almost every member of our Flora Cibaria. This would not be so were their any disposition in the neglected cultivated races to revert to the wild form.

little tendency to reversion.* With this law the suggestive observation of M. Vilmorin well accords, that when once the constitution of a plant is so broken that variation is induced, it is easy to multiply the varieties in succeeding generations.

It may be objected to this line of argument that our cultivated plants are, as regards their constitution, in an artificial condition, and are, if unaided, incapable of self-perpetuation; but an artificially induced condition of constitution is not necessarily a diseased or unnatural one, and, so far as our cultivated plants are concerned, all we do is to place them under conditions which Nature does not provide *at the same particular place and time.* That Nature might supply the conditions at other places and times may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state. We have no reason to suppose that we have violated Nature's laws in producing a new variety of wheat,—we may have only anticipated them; nor is its constitution impaired because it cannot, unaided, perpetuate its race; it is in as souud and unbroken health and vigor during its life as any wild variety is, but its offspring has so many enemies that they do not perpetuate its race. In the case of annual plants, those only can secure the succession of their species which produce more seeds annually than can be eaten by animals or destroyed by the elements. Cultivated wheat will grow and ripen its seed in almost all soils and climates, and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that it is not to the artificial or peculiar condition of the plant itself, and still less to any change effected by man upon it, that its annual extinction is due, but to causes that have no effect whatever upon its own constitution, and over which its constitutional peculiarities can exercise no control.

11. Again, the phenomena of cross impregnation amongst individuals of all species appear, according to Mr. Darwin's accurate observations, to have been hitherto much underrated, both as to extent and importance. The prominent fact that the stamens and pistil are so often placed in the same flower, and come to maturity at the same epoch, has led to the doctrine that flowers are usually self-impregnated, and that the effect is a conservative one as regards the permanence of specific forms. The ob-

* It is not meant by this that any character of a species which may be lost in its variety never reappears in the descendants of the latter, for some occasionally do so in great force; what is meant is, that the newly acquired characters of the variety are never so entirely obliterated that it has no longer a claim to be considered a variety.

servations of Carl Sprengel and others have, however, proved that this is not always the case, and that while Nature has apparently provided for self-fertilization, she has often insiduously counteracted its operation, not only by placing in flowers lures for insects which cross-fertilize them, but often by interposing insuperable obstacles to self-fertilization, in the shape of structural impediments to the access of the pollen to the stigma of its own flower.* In all these instances the double object of Nature may be traced; for self-impregnation (or "breeding in"), while securing identity of form in the offspring, and hence hereditary permanence, at the same time tends to weakness of constitution, and hence to degeneracy and extinction: on the other hand, cross-impregnation, while tending to produce diversity of form in the offspring, and hence variation and apparent mutability, yet by strengthening the offspring favors longevity and apparent permanence of specific type. The ultimate effect of all these operations is of course favorable to the hypothesis that variability is the rule, and permanence the exception, or at any rate only a transitory phenomenon.

12. Hybridization, or cross-impregnation between species or very well marked varieties, again, is a phenomenon of a very different kind, however similar it may appear in operation and analogous in design. Hybridizable genera are rarer that is generally supposed, even in gardens, where they are so often operated upon, under circumstances the most favorable to the production of a hybrid, and unfavorable to self-impregnation. Hybrids are almost invariably barren, and their characters are not those of new varieties. The obvious tendency of hybridization between varieties or other very closely allied forms (in which case the offspring may be fertile) is not to enlarge the bounds of variation, but to contract them; and if between very different forms, it will only tend to confound these. That some supposed species may have their origin in hybridization cannot be denied, but we are now dealing with phenomena on a large scale, and balancing the tendencies of causes uniformly acting, whose effects are unmistakable, and which can be traced throughout the Vegetable Kingdom. In gardening operations the number of hybridized genera is small, their offspring doomed, and since they are more readily impregnated by the pollen of either parent than by their own, or by that of any other plant,† they eventu-

* Thus, in *Lobelia fulgens*, the pollen is entirely prevented by natural causes from reaching the stigma of its own flower. In kidney beans impregnation takes place imperfectly except the carina is worked up and down artificially, which is effected by bees, who may thus either impregnate the flower with its own pollen or with that brought from another plant. I am indebted to Mr. Darwin for both these facts: see 'Gardener's Chronicle,' 1858, p. 828.

† A very able and careful experimenter, M. Naudin, performed a series of experiments at the Jardin des Plantes at Paris, in order to discover the duration of the

ally revert to one of their parents: on the other hand, the number of varieties is incalculable, the power to vary further is unimpaired in their progeny, and these tend to depart further and further in sensible properties from the original parent.

In conformity with my plan of starting from the variable and not the fixed aspect of Nature, I have now set down the prominent features of the Vegetable Kingdom, as surveyed from this point of view. From the preceding paragraphs the evidence appears to be certainly in favor of proneness to change in individuals, and of the power to change ceasing only with the life of the individual; and we have still to account for the fact that there are limits to these mutations, and laws that control the changes both as to degree and kind; that species are neither visionary nor even arbitrary creations of the naturalist; that they are, in short, realities, whether only temporarily so or not.

13. Granting then that the tendency of nature is first to multiply forms of existing plants by graduated changes, and next by destroying some to isolate the rest in area and in character, we are now in a condition to seek some theory of the *modus operandi* of Nature that will give temporary permanence of character to these changelings. And here we must appeal to theory or speculation; for our knowledge of the history of species in relation to one another, and to the incessant mutations of their environing physical conditions, is far too limited and incomplete to afford data for demonstrating the effects of these in the production of any one species in a native state.

Of these speculations by far the most important and philosophical is that of the delimitation of species by natural selection, for which we are indebted to two wholly independent and original thinkers, Mr. Darwin and Mr. Wallace.* These authors assume that all animal and vegetable forms are variable, that the average amount of space and annual supply of food for each species (or other group of individuals) is limited and constant, but that the increase of all organisms tends to proceed annually in a geometrical ratio; and that, as the sum of organic life on the surface of the globe does not increase, the individuals annually destroyed must be incalculably great; also that each species is ever warring against many enemies, and only holding its own by a slender tenure. In the ordinary course of nature this

progeny of fertile hybrids. He concludes that the fertile posterity of hybrids disappears, to give place to the pure typical form of one or other parent. "Il se peut sans doute qu'il y ait des exceptions à cette loi de retour, et que certains hydrides, à la fois très-fertiles et très-établis, tendent à faire souche d'espèce; mais le fait est loin d'être prouvé. Plus nous observons les phénomènes d'hybridité, plus nous inclinous à croire que les espèces sont indissolublement liées à une fonctions dans l'ensemble des choses, et que c'est le rôle même assigné à chacune d'elles qui en détermine la forme, la dimension et la durée." (Annales des Sc. Nat., sér. 4, v. 9.)

* Journal of the Linnean Society of London, Zoology, vol. iii, p. 45.

annual destruction falls upon the eggs or seeds and young of the organisms, and as it is effected by a multitude of antagonistic, ever-changing natural causes, each more destructive of one organism than of any other, it operates with different effect on each group of individuals, in every locality, and at every returning season. Here then we have an infinite number of varying conditions, and a superabundant supply of variable organisms, to accommodate themselves to these conditions. Now the organisms can have no power of surviving any change in these conditions, except they are endowed with the means of accommodating themselves to it. The exercise of this power may be accompanied by a visible (morphological) change in the form or structure of the individual, or it may not, in which case there is still a change, but a physiological one, not outwardly manifested; but there is always a morphological change if the change of conditions be sudden, or when, through lapse of time, it becomes extreme. The new form is necessarily that best suited to the changed condition, and as its progeny are henceforth additional enemies to the old, they will eventually tend to replace their parent form in the same locality. Further, a greater proportion of the seeds and young of the old will annually be destroyed than of the new, and the survivors of the old, being less well adapted to the locality, will yield less seed, and hence have fewer descendants.

In the above operations Nature acts slowly on all organisms, but man does so rapidly on the few he cultivates or domesticates; he selects an organism suited to his own locality, and by so modifying its surrounding conditions that the food and space that were the share of others falls to it, he ensures a perpetuation of his variety, and a multiplication of its individuals, by means of the destruction of the previous inhabitants of the same locality; and in every instance, where he has worked long enough, he finds that changes of form have resulted far greater than would suffice to constitute conventional species amongst organisms in a state of nature, and he keeps them distinct by maintaining these conditions.

Mr. Darwin adduces another principle in action amongst living organisms as playing an important part in the origin of species, viz., that the same spot will support most life when peopled with very diverse forms, as is exemplified by the fact that in all isolated areas the number of classes, orders, and genera is very large in proportion to that of species.

§ 3. *On the General Phenomena of Distribution in Area.*

Turning now to another class of facts, those that refer to the distribution of plants on the surface of the globe, the following are the most obvious:—

14. The most prominent feature in distribution is that circumscription of the area of species, which so forcibly suggests the hypothesis that all the individuals of each species have sprung from a common parent, and have spread in various directions from it. It is true that the area of some (especially cryptogamic and aquatic plants) is so great that we cannot indicate any apparent centre of diffusion, and that others are so sporadic that they appear to have had many such centres; but these species, though more numerous than is usually supposed, are few in comparison with those that have a definite or circumscribed area.

With respect to this limitation in area,* species do not essentially differ from varieties on the one hand, or from genera and higher groups on the other; and indeed, in respect of distribution, they hold an exactly intermediate position between them, varieties being more restricted in locality than species, and these again more than genera.

The universality of this feature (of groups having defined areas) affords to my mind all but conclusive evidence in favor of the hypothesis of similar forms having had but one parent, or pair of parents. And further, this circumscription of species and other groups in area, harmonizes well with that principle of divergence of form, which is opposed to the view that the same variety or species may have originated at different spots. It also follows that, as a general rule, the same species will not give rise to a series of similar varieties (and hence species) at different epochs; whence the geological evidence of contemporaneity derived from identity of fossil forms may be relied upon.

The most obvious cause of this limitation in area no doubt exists in the well-known fact that plants do not necessarily inhabit those areas in which they are constitutionally best fitted to thrive and to propagate; that they do not grow where they would most like to, but where they can find space and fewest enemies. We have seen (13) that most plants are at warfare with one or more competitors for the area they occupy, and that both the number of individuals of any one species and the area it covers are contingent on the conditions which determine these remaining so nicely balanced that each shall be able at least to

* It is a remarkable fact that there are some striking anomalies in the distribution of plants into provinces, as compared with animals. Thus there is no peculiarity in the vegetation of Australia to be compared with the rarity of placental mammals, nor with the fact of so many of the mammals, birds, and fish of Tasmania differing from those of the continent of Australia. Nearer home, we find the basin of the Mediterranean with a tolerably uniform flora on the European and North African sides, but these ranking as different zoological provinces. The much narrower delimitation in area of animals than plants, and greater restriction of faunas than floras, should lead us to anticipate that plant types are, geologically speaking, more ancient and permanent than the higher animal types are, and so I believe them to be, and I would extend the doctrine even to plants of highly complex structure.

hold its own, and not succumb to the enervating or etiolating or smothering influences of its neighbors. The effects of this warfare are to extinguish some species, to spare only the hardier races of others, and especially to limit the remainder both as to area and characters. Exceptions occur in plants suited to very limited or abnormal conditions, such as desert plants, the chief obstacles to whose multiplication are such inorganic and principally atmospheric causes as other plants cannot overcome at all; such plants have no competitors, are generally widely distributed, and not very variable.*

15. The three great classes of plants, Acotyledons, Monocotyledons, and Dicotyledons (Gymnospermous and Angiospermous), are distributed with tolerable equality over the surface of the globe, inasmuch as we cannot indicate any of the six continents (Europe, Asia, Africa, North and South America, and Australia) as being peculiarly rich in one to the exclusion of another. Further, the distribution of some of the larger orders is remarkably equable, as *Compositæ*, *Leguminosæ*, *Gramineæ*, and others; facts which (supposing existing species to have originated in variation) would seem to indicate that the means of distribution have overcome, or been independent of the existing apparent impediments, and that the power of variation is equally distributed amongst these classes, and continuously exerted under very different conditions. I do not mean that all the classes are equally variable, but that each displays as much variety in one continent as in another.

16. Those classes and orders which are the least complex in organization are the most widely distributed, that is to say, they contain a larger proportion of widely diffused species. Thus the species of Acotyledons are more widely dispersed than those of Monocotyledons, and these again more so than those of Dicotyledons; so also the species of *Thallophytes* are among the most widely dispersed of Acotyledons, the *Gramineæ* of Monocotyledons, and the *Chenopodiaceæ* of Dicotyledons. This tendency of the least complex species to be most widely diffused is most marked in Acotyledons, and least so in Dicotyledons,† a fact which is analogous to that already stated (4), that the least complex are also the most variable.

* Though invariable forms, they may be, and often are, themselves varieties or races of a species that inhabits more fertile spots, as *Poa bulbosa*, which is a very well-marked and constant form of *P. pratensis*, occurring in dry sandy soil, from England to Northwestern India, its "meadow" relative being a very variable species in the same countries, and always struggling for existence amongst other grasses, etc.

† Very much, no doubt, because of the difficulty in classifying Dicotyledons by complexity of organization; in other words, of our inability to estimate in a classificatory point of view the relative value of the presence or absence of organs in plants, where many are present, and where those of low morphological importance may have a comparatively high physiological significance.

17. Though we rarely find the same species running into the same varieties at widely sundered localities (unless starved or luxuriant forms be called varieties), yet we do often find a group of species represented in many distant places by other groups of allied forms; and if we suppose that individuals of the parent type have found their way to them all, the theory that existing species have originated in variation, and that varieties depart further from the parent form, will account for such groups of allied species being found at distant spots; as also for these groups being composed of representative species and genera.

18. No general relations have yet been established between the physical conditions of a country and the number of species or varieties which it contains, further than that the tropical and temperate regions are more fertile than the polar, and that perennial drought is eminently unfavorable to vegetation. It is not even ascertained whether the tropical climates produce more species than the temperate.

19. Though we cannot explain the general relations between the vegetation and physical condition of any two countries that contrast in these respects, we may conclude as a general rule that those tracts of land present the greatest variety in their vegetation that have the most varied combinations of conditions of heat, light, moisture, and mineral characters. It is, in the present state of our knowledge, impossible to measure the amount of the fluctuations of these conflicting conditions in a given country, nor if we could can we express them symbolically or otherwise so as to make them intelligible exponents of the amount of variety in the vegetation they affect; but the following facts in general distribution appear to me to be favorable to the idea that there is such a connection.

There are certain portions of the surface of the globe characterized by a remarkable uniformity in their phænogamic vegetation. These may be luxuriantly clothed, and abound in individuals, but are always poor in species. Such are the cooler temperate and subarctic lake regions of North America, Fuegia and the Falkland Islands, the Pampas of Buenos Ayres, Siberia and North Russia, Ireland and Western Scotland, the great Gangetic plain, and many other tracts of land. Now all these regions are characterized by a great uniformity in most of their physical characters, and an absence of those varying conditions which we assume to be stimulants to variation in a locality. On the other hand, it is in those tracts that have the most broken surface, varied composition of rocks, excessive climate (within the limits of vegetable endurance), and abundance of light, that the most species are found, as in South Africa, many parts of Brazil and the Andes, Southern France, Asia Minor, Spain, Algeria, Japan, and Australia.

20. The Polar regions are chiefly peopled from the colder temperate zones, and the species from the latter which have spread into them are very variable, but only within comparatively small limits, particularly in stature, color, and vesture. Many of these polar and colder temperate plants are also found, together with other species closely allied to them, on the mountains of the warm temperate, and even tropical zones; to which it is difficult to conceive that they can have been transported by agencies now in operation.

21. The floras of islands present many points of interest. The total number of species they contain seems to be invariably less than an equal continental area possesses, and the relative numbers of species to genera (or other higher groups) is also much less than in similar continental areas.

The further an island is from a continent, the smaller is its flora numerically, the more peculiar is its vegetation, and the smaller its proportion of species to genera. In the case of very isolated islands, moreover, the generic types are often those of very distant countries, and not of the nearest land. Thus the St. Helena and Ascension forms are not so characteristic of tropical Africa as of the Cape of Good Hope. Those of Kerguelen's Land are Antarctic American, not African nor Indian. The Sandwich Islands contain many Northwest American and some New Zealand forms. Japan presents us with many genera and species unknown except to the *eastward* of the Rocky Mountains, in North America.* So too American, Abyssinian, and even South African genera and species are found in Madeira and the Canary Islands; and Fuegian ones in Tristan d'Acunha.

22. There is a strict analogy in this respect between the floras of islands and those of lofty mountain-ranges, no doubt in both cases owing to the same causes. Thus, as Japan contains various peculiar N. E. American species which are not found in N. W. America nor elsewhere on the globe, and the Canaries and Azores possess American genera not found in Europe nor Africa, so the lofty mountains of Borneo contain Tasmanian and Himalayan representatives; the Himalayas contain Andean, Rocky Mountain, and Japanese genera and species; and the alps of Victoria and Tasmania contain assemblages of New Zealand, Fuegian, Andean, and European genera and species. We cannot account for any of these cases of distribution between islands and mountains except by assuming that the species and genera common to these distant localities have found their way across the intervening spaces under conditions which no longer exist.

* Whilst these sheets are passing through the press, I have been informed by Professor Asa Gray that the flora of Japan and N. E. Asia is much more closely allied to that of the Northern United States than to that of America west of the Rocky Mountains.

23. There is much to be observed in the condition and distribution of the introduced or naturalized plants of a country, which may be applied to the study of the origin of its indigenous vegetation. The greater proportion of these are the annual and other weeds of cultivated land, and plants which attach themselves to nitrogenous soils; naturalized perennials, shrubs, and trees occur consecutively in rapidly diminishing proportions. I can find no decided relation between complexity of structure and proneness to migrate, nor much between facilities for transport or power of endurance or vitality in the seed, and extent of distribution by artificial means. I shall return to this subject (which I have elsewhere discussed at length with reference to the Galapagos Archipelago*) when treating of the naturalized plants of Australia.

24. I venture to anticipate that a study of the vegetation of islands with reference to the peculiarities of their generic types on the one hand, and of their geological condition (whether as rising or sinking) on the other, may, in the present state of our knowledge, advance the subjects of distribution and variation considerably. The incompleteness of the collections at my command from the Polynesian islands, has frustrated my attempts to illustrate this branch of inquiry by extending my researches from the Australian Flora over that of the Pacific. I may however indicate as a general result, that I find the sinking islands, those (so determined by Darwin's able investigations) characterized as atolls, or as having barrier reefs, to contain comparatively fewer species and fewer peculiar generic types than those which are rising. Thus, commencing from the east coast of Africa, I find in the Indian Ocean the following islands marked in Darwin's chart† as bounded with fringing reefs or active volcanos, and hence rising:—The Seychelles, Madagascar, Mauritius, Bourbon, Ceylon, the Andamans, Nicobar, and Sumatra; the vegetation of all which is characterized by great diversity and much peculiarity of generic type: whereas those marked as atolls or barrier reefs, as the Maldives, Laccadives, and Keeling Island, contain few species, and those the same as grow on the nearest continents. In the Pacific Ocean, again, the groups of islands most remarkable for their ascertained number of very peculiar generic types are the Sandwich group, Galapagos, Juan Fernandez, Loochoo and Bonin, all of which are rising, and most have active volcanos: those with the least amount of peculiarity are the Society group and Fijis, both of which are sinking. In the present state of our knowledge it is not safe to lay much stress on these apparent facts, especially as the New Hebrides and New Caledonia, which lie very close together, and both, I believe,

* Linn. Trans., xx, 235.

† See his works on volcanic islands and on coral reefs.

contain much peculiarity, are in opposite geological conditions, the Hebrides rising and Caledonia sinking; and the Friendly* and Fiji groups, equally near one another, and with, I suspect, very similar vegetation, are also represented as being in opposite conditions. On the other hand, in the whole of the group including the Low Archipelago and the Society Islands, extending over more than 2000 miles, I observe but one spot,† namely, Elizabeth Island, a mere speck of land, but which is the only known habitat of one of the most remarkable genera of *Compositæ*.‡

25. Many of the above facts in the general distribution of species cannot be wholly accounted for by the supposition that natural causes have dispersed them over such existing obstacles as seas, deserts, and mountain-chains; moreover, some of these facts are opposed to the theory that the creation of existing species has taken place subsequent to the present distribution of climates, and of land and water, and to that of their dispersion having been effected by the now prevailing aquatic, atmospheric, and animal means of transport.

Similar climates and countries, even when altogether favorably placed for receiving colonists from each other, and with conditions suitable to their reciprocal exchange, do not, as a rule, interchange species. Causes now in operation will not account for the fact that only 200 of the New Zealand flowering plants are common to Australia, and still less for the contrasting one that the very commonest, most numerous, and universally distributed Australian genera and species, as *Casuarina*, *Eucalyptus*, *Acacia*, *Boronia*, *Helichrysum*, *Melaleuca*, etc., and all the Australian *Leguminosæ* (including a European genus and species), are absent from New Zealand. Causes now in operation cannot be made to account for a large assemblage of flowering plants characteristic of the Indian peninsula being also inhabitants of tropical Australia, while not one characteristic Australian genus has ever been found in the peninsula of India. Still less will these causes account for the presence of Antarctic and European species in the Alps of Tasmania and Victoria, or for the reappearance of Tasmanian genera on the isolated lofty mountain of Kina-Balou, in Borneo.

* I find that there is a remarkable difference between the floras of the New Hebrides and Caledonia on the one hand, and those of the Fiji islands and those to the east of them on the other. In the former, New Zealand and Australian types abound; in the latter, almost exclusively Indian forms. The differences between the floras of Fiji, Samoa, Tonga, Tahiti, and that of India, are in species and not in genera, and many species are common to all.

† Mr. Darwin has left Aurora Island (another of the group) uncolored, on account of the doubtful evidence regarding it, which however is in favor of its being in the same condition as Elizabeth's Island. From a list of species communicated by Mr. Dana, it appears to contain no peculiar plants.

‡ *Fitchia*. See Lond. Journ. Bot. 1845, iv, p. 640, t. 23, 24. [A specimen of this plant was gathered by Prof. Dana on the mountains of Tahiti.—Eds.]

These and a multitude of analogous facts have led to the study of two classes of agents, both of which may be reasonably supposed to have had a powerful effect in determining the distribution of plants; these are changes of climates, and changes in the relative positions and elevations of land.

26. Of these, that most easy of direct application is the effect of humidity in extending the range of species into regions characterized by what would otherwise be to them destructive temperatures.

I have, in the 'Antarctic Flora,' shown that the distribution of tropical forms is extended into cold regions that are humid and equable further than into such as are dry and excessive; and, conversely, that temperate forms advance much further into humid and equable tropical regions than into dry and excessive ones; and I have attributed the extension of Tree-ferns, Epiphytal Orchids, Myrtaceæ, etc., into high southern latitudes, to the moist and equable climate of the south temperate zone. I have also shown how conspicuously this kind of climate influences the distribution of mountain plants in India, where tropical forms of Laurel, Fig, Bamboo, and many other genera, ascend the humid extratropical mountains of Eastern Bengal and Sikkim to fully 9000 feet elevation; and temperate genera, and in some cases species, of Quercus, Salix, Rosa, Pinus, Prunus, Camellia, Rubus, Kadsura, Fragaria, Æsculus, etc., descend the mountains even to the level of the sea, in lat. 25°. In a tropical climate the combined effects of an equable climate and humidity in thus extending the distribution of species, often amount to 5000 feet in elevation or depression (equivalent to 15° Fahr. of isothermals in latitude), a most important element in our speculations on the comparative range of species under existing or past conditions; and when to this is added that the average range in altitude of each Himalayan tropical and temperate and alpine species of flowering plant is 4000 feet, which is equivalent to 12° of isothermals of latitude, we can understand how an elevation of a very few thousand feet might, under certain climatic conditions, suffice to extend the range of an otherwise local species over at least 28° parallels of latitude, and how a proportionally small increase of elevation in a meridional chain where it crosses the Equator, may enable temperate plants to effect an easy passage from one temperate zone to the other.

27. To explain more fully the present distribution of species and genera in area, I have recourse to those arguments which are developed in the Introductory Essay to the New Zealand Flora, and which rest on geological evidence, originally established by Sir Charles Lyell, that certain species of animals have survived great relative changes of sea and land. This doctrine, which I in that Essay endeavored to expand by a study of the

distribution of existing southern species, has, I venture to think, acquired additional weight since then, from the facts I shall bring forward under the next head of Geological Distribution, and which seem to indicate that many existing orders and genera of plants of the highest development may have flourished during the Eocene and Cretaceous periods, and have hence survived complete revolutions in the temperature and geography of the middle and temperate latitudes of the globe.

28. Mr. Darwin has greatly extended in another direction these views of the antiquity of many European species, and their power of retaining their *facies* unchanged during most extensive migrations, by his theory of the simultaneous extension of the glacial temperature in both hemispheres, and its consequent effect in cooling the tropical zone. He argues that, under such a cold condition of the surface of the globe, the temperate plants of both hemispheres may have been almost confined to the tropical zone, whence afterwards, owing to an increment of temperature, they would be driven up to the mountains of the tropics, and back again to those higher temperate latitudes where we now find most of them. I have already (New Zealand Essay) availed myself of the hypothesis of an austral glacial period, to account for Antarctic species being found on the alps of Australia, Tasmania, and New Zealand; and if as complete evidence of such a proportionally cooled state of the intertropical regions were forthcoming as there is of a glacial condition of the temperate zones, it would amply suffice to account for the presence of European and Arctic species in the Antarctic and south temperate regions, and of the temperate species of both hemispheres on the mountains of intermediate tropical latitudes.

On the other hand, we have sufficient evidence of many of what are now the most tropical orders of plants having inhabited the north temperate zone before the glacial epoch; and it is difficult to conceive how these orders could have survived so great a reduction of the temperature of the globe as should have allowed the preglacial temperate flora to cross the Equator in any longitude. It is evident that, under such cold, the most tropical orders must have perished, and their re-creation after the glacial epoch is an inadmissible hypothesis.*

* The question of the state of the mean temperature of the globe during comparatively recent geological periods is yearly deriving greater importance in relation to the problem of distribution. Upon this point geologists are not altogether clear, nor at one with the masters of physical science. Lyell (Principles, ed. ix, chap. vii) attributes the glacial epoch to such a disposition of land and sea as would sufficiently cool the temperate zones; and he implies that this involves or necessitates a lowering of the mean temperature of the whole globe. Another hypothesis is, that there was a lowering of the mean temperature of the globe wholly independent of any material change in the present relations of sea and land, which cold induced the glacial epoch. A third theory is that such a redisposition of land and sea as would induce a glacial epoch in our hemisphere need not be great, nor necessitate a decrement of the mean temperature of the whole earth.

29. It remains then to examine whether, supposing the glacial epoch of the northern and southern hemispheres to have been contemporaneous, the relations of land and sea may not have been such as that a certain meridian may have retained a tropical temperature near the Equator, and thus have preserved the tropical forms. Such conditions might perhaps be attained by supposing two large masses of land at either pole, which should contract and join towards the Equator, forming one meridional continent, while one equatorial mass of land should be placed at the opposite meridian. If the former continent were traversed by a meridional chain of mountains, and so disposed that the polar oceanic currents should sweep towards the Equator for many degrees along both its shores, its equatorial climate would be throughout far more temperate than that of the opposite equatorial mass of land, whose climate would be tropical, insular, and humid.

30. The hypothesis of former mountain chains having afforded to plants the means of migration, by connecting countries now isolated by seas or desert plains, is derived from the evidence afforded by geology of the extraordinary mutation in elevation that the earth's surface has experienced since the appearance of existing forms of animals and plants. In the Antarctic Flora I suggested as an hypothesis that the presence of so many Arctic-American plants in Antarctic America might be accounted for by supposing that the now depressed portions of the Andean chain had, at a former period, been so elevated that the species in question had passed along it from the north to the south temperate zone;* and there are some facts in the distribution of species common to the mountain floras of the Himalaya and Malay Islands, and of Australia and Japan, that would well accommodate themselves to a similar hypothesis. Of such submerged meridional lands we have some slender evidence in the fact that, in the meridian of Australia and Japan, we have, first, the north-west coast of Australia sinking, together with the Louisiade archipelago to its north; then, approaching the line, the New Ireland group is sinking, as are also the Caroline Islands, in lat 7° N. Beyond this, however, in lat. 15° N., are the Marianne Islands (rising) of whose vegetation nothing is known; in 27° N., the

* The continuous extension of so many species along the Cordillera (of which detailed evidence is given in the Antarctic Flora) from the Rocky Mountains to Fuegia, is a most remarkable fact, considering how great the break is between the Andes of New Granada and those of Mexico, and that the intermediate countries present but few resting-places for alpine plants. That this depression of the chain has had a powerful effect in either limiting the extension of species which have appeared since its occurrence, or in inducing changes of climate which have extinguished species once common to the north and south, is evidenced by the fact that a number of Fuegian and South Chili plants extend northward as alpines to the very shores of the Gulf of Mexico, but do not inhabit the Mexican Andes, whilst as many Arctic species advance south to the Mexican Andes, but do not cross the intermediate depression and reappear in the Bolivian Andes.

Bonin Islands (also rising); and in 30° is Japan, with which this botanical relationship exists.

It is objected by Mr. Darwin to this line of argument (as to that on p. 15, concerning the Pacific Islands), that all these sinking areas are volcanic islands, having no traces of older rocks on them. But I do not see that this altogether invalidates the hypothesis; for many of the loftiest mountains throughout the Malayan Archipelago, New Zealand, and the Pacific Islands, are volcanic; some are active, and many attain to 14,000 feet in elevation, whilst the lower portions of some of the largest of these islands are formed of rocks of various ages.

(*To be continued.*)

ART. II.—*Some General Views on Archæology;* by A. MORLOT.*

A CENTURY scarcely has elapsed since the time when it would have been thought impossible to reconstruct the history of our globe, prior to the appearance of mankind. But, though contemporary historians were wanting during this immense pre-human era, the latter has not failed in leaving us a well-arranged series of most significant vestiges: the animal and vegetable tribes, which have successively appeared and disappeared, have left their fossil remains in the successively deposited strata. Thus has been composed, gradually and slowly, a history of creation, written, as it were, by the Creator himself. It is a great book, the leaves of which are the stratified rocks, following each other in the strictest chronological order, the chapters being the mountain-chains. This great book has long been closed to man. But science, constantly extending its realm and improving its method of induction, has taught the geologist to study those marvellous archives of creation, and we behold him now unfolding the past ages of our world, with a variety of details and a certainty of conclusions well calculated to inspire us with grateful admiration.

The development of archæology has been very similar to that of geology. Not long ago we should have smiled at the idea of reconstructing the by-gone days of our race, previous to the first beginning of history properly so called. The void was filled up, partly by representing that ante-historical antiquity as having been only of short duration, and partly by exaggerating the value and the age of those vague and confused notions which constitute tradition.

* This article is an introduction to a paper entitled, Geologico-Archæological Studies in Denmark and Switzerland, appearing in the *Bulletin de la Société Vaudoise des Sciences Naturelles*, for 1859, and of which a separate edition, comprising the present pages, will be published.

It seems to be with mankind at large as with single individuals. The recollections of our earliest childhood have entirely faded away, up to some particular event which had struck us more forcibly, and which alone has left a lasting image amidst the surrounding darkness. Thus, excepting the idea of a deluge, which exists among so many nations, and therefore appears to have originated before the migration of those same nations, the infancy of mankind, at least in Europe, has passed without leaving any recollection, and history fails here entirely: for what is history but the memory of mankind?

But, before the beginning of history, there have been life and industry, of which various monuments still exist, while others lie buried in the soil, much as we find the organic remains of former creations entombed in the strata composing the crust of the globe. The memorials of antiquity enact here a part similar to that of the fossils; and if Cuvier calls the geologist an antiquarian of a new order, we can reverse that remarkable saying, and consider the antiquarian as a geologist, applying his method to reconstruct the first ages of mankind, previous to all recollection, and to work out what may be called pre-historical history. This is archæology pure and proper. But archæology cannot be considered as coming to a full stop with the first beginning of history. For the further we recede in our historical researches, the more incomplete they become, leaving gaps which the study of the material remains helps to fill up. Archæology therefore pursues its course in a parallel line with that of history, and henceforth the two sciences mutually enlighten each other. But, with the progress of history, the part taken by archæology goes on decreasing, until the invention of printing almost brings to a close the researches of the antiquarian.

To pursue geological investigations we must first examine the present state of our planet and observe its changes; that is, we must begin by physical geography. This supplies us with a thread of induction, to guide us safely in our rambles through the passed ages of our earth, as Lyell has so admirably set forth. For the laws which govern the organic creation and the inorganic world are as invariable as the results of their combinations and permutations are infinitely varied; science revealing to us every where the perfect stability of the causes with the diversity of the forms.

So, to understand the past ages of our species, we must first begin by examining its present state, following man wherever he has crossed the waters and set his foot upon dry land: the different nations, which inhabit our earth at present, must be studied with respect to their industry, their habits and their general mode of life. We thus make ourselves acquainted with the different degrees of civilization, ranging from the highest summit

of modern development to the most abject state, hardly surpassing that of the brute. By that means ethnography supplies us with what may be called a contemporaneous scale of development, the stages of which are more or less fixed and invariable, whilst archæology traces a scale of successive development with one moveable stage, passing gradually along the whole line.*

Ethnography is consequently to archæology what physical geography is to geology, namely a thread of induction in the labyrinth of the past, and a starting point in those comparative researches of which the end is the knowledge of mankind and of its development through successive generations.

In following out the principles above laid down, the Scandinavian savans have succeeded in unravelling the leading features in the progress of pre-historical European civilization, and in distinguishing three principal eras, which they have called the *stone-age*, the *bronze-age*, and the *iron-age*.†

This great conquest in the realm of science is due chiefly to the labors of Mr. Thomsen, director of the ethnographical and archæological museums at Copenhagen‡ and to those of Mr. Nilsson, professor at the flourishing University of Lund in Sweden.§

These illustrious veterans among northern antiquaries have ascertained that our Europe, at present so civilized, was at first inhabited by tribes to whom the use of metals was totally unknown, and whose industry and domestic habits must have borne a considerable analogy to what we now see practised among certain savages. Bone, horn and especially flint were then used instead of metal for manufacturing cutting-instruments and arms. This was the *stone-age*, which might also be called the first great phase of civilization.

The earliest settlers in Europe apparently brought with them the art of producing fire. By striking iron-pyrites (sulphuret of iron) against quartz, fire can be easily obtained. But this method can only have been occasionally used, and seems to have been confined to some native tribes in Tierra del Fuego.‖ The usual mode has evidently been that of rubbing two sticks together. But on further reflection it is easy to perceive that this was a

* Some naturalists see a correspondence of the same sort between embryology and comparative anatomy; for they consider the human embryo as passing during its development through the different stages of the scale of animal creation, or, at least, as passing through the different states of the embryos of the different stages of that scale.

† The history of Danish archæology has been sketched by T. Hindenburg. See *Dansk maanedsskrift*, i, 1859.

‡ *Ledetraad til nordisk Oldkyndighed; Kjoebenhavn*, 1836. Published in English by Lord Ellesmere, under the title, "A Guide to Northern Antiquities. London, 1848."

§ Nilsson: *Scandinaviska Nordens Urinvonare.* Lund, 1838—1843.

‖ Weddell: A Voyage towards the South Pole in 1822–1824. London, 1827, p. 167

most difficult discovery, and must at all events have been preceded by a knowledge of the use of fire, as derived from the effects of lightning or from volcanic action.

The stone-age was therefore probably preceded by a period, perhaps of some length, during which man was unacquainted with the art of producing fire. This, according to Mr. Flourens, indicates that the cradle of mankind was situated in a warm climate.*

The art of producing fire has been perhaps the greatest achievement of human intelligence. The use of fire lies at the root of almost every species of industry. It enables the savage to fell trees, as it allows civilized nations to work metals. Its importance is so great, that deprived of it man would perhaps scarcely have risen above the condition of the brute. Even the ancients were sensible of this, as is witnessed by the fable of Prometheus. As to their sacred perpetual fire, its origin seems to lie in the difficulty of procuring fire, thereby rendering its preservation essential.

In Europe the stone-age came to an end by the introduction of bronze. This metal is an alloy of about nine parts of copper and one part of tin.† It melts and moulds well; the molten mass in cooling slowly acquires a tolerable degree of hardness, inferior to that of steel, it is true, but superior to that of very pure iron. We therefore understand how bronze would long be used for manufacturing cutting-instruments, weapons and numerous personal ornaments. The northern antiquaries have very properly called this second great phase in the development of European civilization the *bronze-age*.

The bronze articles of this period, with a few trifling exceptions, have not been produced by hammering, but have been regularly cast, often with a considerable degree of skill. Even the sword-blades were cast, and the hammer (of stone) was only used to impart a greater degree of hardness to the edge of the weapon.

The bronze-age has therefore witnessed a mining industry, which was completely wanting during the stone-age. Now the art of mining is so essential to civilization, that without it the world would perhaps yet be exclusively inhabited by savages.

* Flourens: *De la longévité humaine*. Paris, 1835, p. 127. "Man, from the construction of his teeth, his stomach, and his intestines, is primitively *frugivorous*, like the monkey. But the frugivorous diet is the most unfavorable, because it constrains its followers perpetually to abide in those countries which produce fruit at all seasons, consequently in warm climates. But, when once the art of cooking was introduced, and applied both to vegetable and animal productions, man could extend and vary the nature of his diet. Man has consequently two diets; the first is primitive, natural and *instinctive*, and by it he is *frugivorous*, the second is artificial, being due entirely to his intelligence, and by it he is *omnivorous*."

† Bronze is still used for casting bells, cannon, and certain portions of machinery. It must not be confounded with common brass, which is a compound of copper and zinc, much less hard, and appearing only in the iron-age.

It is then worth our while to enquire more closely into the origin of bronze.

Copper was not difficult to obtain. In the first place, virgin copper is not particularly scarce. Then, the different kinds of ore which contain copper combined with other elements, are either highly colored, or present a marked metallic appearance, and are consequently easily known; they are besides not hard to smelt, so as to separate the metal. Finally, copper-ore is not at all scarce; it is met with in the older geological series of most countries.

Virgin tin is unknown, but tin ore is heavy, of dark color and very easy to smelt. However frequent copper may be, tin is of rare occurence. Thus the only mines in Europe which produce tin at the present day are those of Cornwall in England, and of the Erzgebirge and Fichtelgebirge in Germany.

But the question arises, whether, previous to the discovery of bronze, man, owing to the great rarity of tin, may not have begun by using copper in a pure state. If so, there would have been a copper-age between the stone-age and the bronze-age.

In America this has been really the case. When discovered by the Spaniards, both the two centres of civilization, Mexico and Peru, had bronze, composed of copper and tin, and used it for manufacturing arms and cutting-instruments in the absence of iron and steel, which were unknown in the New World. But the admirable researches of Messrs. Squier and Davis in the antiquities of the Mississippi valley* have brought to light an ancient civilization of a remarkable nature, and distinguished by the use of raw virgin copper, worked in a cold state, by hammering, without the aid of fire. The reason of its being so worked lies in the nature of pure copper, which when melted flows sluggishly and is not very fit for casting. A peculiar characteristic of the metal, that of occasionally containing crystals of virgin silver, betrays its origin, and shows that it was brought from the neighborhood of Lake Superior. This region is still rich in metallic copper, of which single blocks, attaining a weight of fifty tons, have lately been discovered. There was even found at the bottom of an old mine a great mass of copper, which the ancients had evidently been unable to raise, and which they had abandoned, after having cut off the projecting parts with stone hatchets.†

The date of that American copper-age is unknown. All we know is, that it must reach at least as far back as ten centuries, that space of time being deemed necessary for the growth of the

* Squier and Davis: Ancient Monuments of the Mississippi Valley. Smithsonian Contributions to Knowledge; Washington, 1848. It is one of the most splendid archæological works ever published.

† Lapham: The Antiquities of Wisconsin. Smithsonian Contributions to Knowledge, 1855, p. 76.

virgin forests now flourishing upon the remains of that antique civilization, of which the modern Indians have not even retained a tradition.

It is finally worthy of remark, that the *mound-builders*, as the Americans call the race of the copper-age, seem to have immediately preceded and prepared the way for the Mexican civilization, destroyed by the Spaniards; for, in progressing southwards, a gradual transition is noticed from the ancient earthworks of the Mississippi valley to the more modern constructions of Mexico, as found by Cortez.

In Europe the remains of a copper-age are wanting. Here and there a solitary hatchet of pure copper is found. But this can be easily accounted for by the greater frequency of copper, while tin had usually to be brought from a greater distance, so that its supply was more precarious.

As Europe did not witness a regular development of a copper-age, it seems, according to Mr. Troyon's very just remark, that the art of manufacturing bronze was brought from another quarter of the world, where it had been previously invented. It was most probably some region in Asia, producing both copper and tin, where those two metals were first brought into artificial combination, and where also traces of a still earlier copper-age are likely to be found.

An apparently serious objection might be started here by raising the question, how mines could be worked without the aid of steel. This however is sufficiently explained by the fact, that the hardest rocks can be easily managed through the agency of fire. By lighting a large fire against a rock, the latter is rent and fissured, so as considerably to facilitate its quarrying. This method was frequently employed when wood was cheaper, and is even practised at the present day in the mines of the Rammelsberg in Germany, where it facilitates the working of a rock of extreme hardness.

That metal of dingy and sorry appearance, but more truly precious than gold or the diamond—iron—at length appears, giving a wonderful impulse to the progressive march of mankind, and characterising the third great phase in the development of European civilization, very properly called the *iron-age*.

Our planet never produces iron in its metallic or virgin state, for the simple reason, that it is too liable to oxydation. But among the aerolites there are some composed of pure iron with a little nickel, which alters neither the appearance nor sensibly the qualities of the metal. Thus the celebrated meteoric iron discovered by Pallas in Siberia was found by the neighboring blacksmiths to be malleable in a cold state.* Meteoric iron has even been

* Pallas: *Voyages en Russie*, Paris, 1793, iv, 595. There was but one mass of this meteoric iron; it weighed 1600 pounds.

worked by tribes to whom the use of common iron was unknown. Thus Amerigo Vespucci speaks of savages near the mouth of the La Plata, who had manufactured arrow-heads with iron derived from an aërolite.* Such cases are certainly of rare occurrence, but they are not without their importance, for they explain how man may probably have first become acquainted with iron, and they also account for the occasional traces of iron in tombs of the stone-age, if indeed this fact be well established.

It is notwithstanding evident, that the regular working of terrestrial iron-ore must have been a necessary condition of the commencement and progress of the iron-age.

Now, iron-ore is widely diffused in most countries, but it has usually the look of common stones, being distinguished more by its weight than its color. Moreover its smelting requires a much greater degree of heat than copper or tin, and this renders its production considerably more difficult than that of bronze.

But, even when iron had been obtained, what groping in the dark and how much accumulated experience did it not require, to bring forth at will bar-iron or steel! Chance, if chance there be, may have played a part in it. But as chance only favors those privileged mortals who combine a keen spirit of observation with serious meditation and with practical sense, the discovery was not less difficult or less meritorious. We need not then be surprised, if man arrived but tardily at the manufacture of iron and steel, which is still daily improving.

In Carinthia traces of a most primitive method of producing iron have been noticed. The process seems to have been as follows: on the declivity of a hill was dug an excavation, in which was lighted a large fire; when this began to subside, fragments of very pure ore (hydrous-oxyd) were thrown into it and covered by a new heap of wood. When all the fuel had been consumed, small lumps of iron would then be found among the ashes.† All blowing apparatus was in this manner dispensed with; an important fact when we come to consider how much the use of a blast complicates metallurgical operations, because it implies the application of mechanics. Thus certain tribes in Southern Africa, although manufacturing iron and working it tolerably well, have not achieved the construction of our common kitchen bellows, apparently so simple, they blow laboriously through a tube, or by means of a bladder affixed to it.

The Romans produced iron by the so-called Catalonian process, and the remains of Roman works of that description have been discovered and investigated in Upper Carniola in Austria.‡

* Smithsonian Contributions to Knowledge, vol. ii, art. 8, p. 178.

† Communicated to the author by mining engineers in Carinthia.

‡ *Jahrbuch der k. k. geologischen Reichsanstalt.* Wien, 1850, ii, 199. Carinthia and Upper Carniola formed part of the Roman province *Noricum*, celebrated for its iron.

The Catalonian forge is still used in the Pyrenees, where it yields tolerable results, but it consumes a large quantity of charcoal, requires much wind, and is only to be applied to pure ore, containing but a very small proportion of earthy matter producing scoriæ; for the process consists in a mere reduction with a soldering and welding together of the reduced particles, without the metal properly melting. According to the manner in which the operation is conducted, bar-iron or steel are obtained at will. This direct method dispenses with the intermediate production of cast iron, which was unknown to the ancients, and which is now the only means of producing iron on a great scale.

Silver accompanies the introduction of iron into Europe, at least in the northern parts, while gold was already known during the bronze-age. This is natural, for gold is generally found as a pure metal, while silver has usually to be extracted from different kinds of ore by more or less complicated metallurgical operations—for example, by cupellation.

With iron appear also for the first time in Europe, glass, coined money, that powerful agent of commerce, and finally the alphabet, which, as the money of intelligence, vastly increases the activity and circulation of thought,* and is sufficient of itself to characterize a new and wonderful era of progress. From thence can we date the dawn of history and of science, in particular of astronomy.

The fine arts also reveal, with the introduction of iron in Europe, a new and important element, indicating a striking advance. Already in the stone-age, but more in the bronze-age, the natural taste for art reveals itself in the ornaments bestowed upon pottery and metallic objects. These ornaments consist in dots, circles, and zigzag, spiral, and S-shaped lines, the style bearing a geometrical character, but showing pure taste and real beauty of its kind, although devoid of all delineations of animated objects, either in the shape of plants or animals. It is only with the iron-age that art, taking a higher range, rose to the representation of plants, animals, and even of the human frame. No wonder, then, if idols of the bronze-age, as well as of the stone-age, are wanting in Europe. It is to be presumed that the worship of fire, of the sun and of the moon, was prevalent in remote antiquity, at least during the bronze-age, perhaps also during the stone-age.

The preceding pages constitute a sketch, certainly very rough and imperfect, of the development of civilization. They establish however in a striking manner the fact of a progress, slow,

* "The circulation of ideas is for the mind what the circulation of specie is for commerce, a true source of wealth." C. V. de Bonstetten: *L'homme du Midi et l'homme du Nord.* Genève, 1826, p. 175.

FOSSIL SHELLS FROM NEBRASKA.

Am. Jour. Sci. V. XXIX. No. 85 Jany 1860. PLATE I.

Fig. 1.

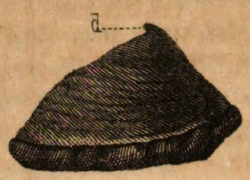


Fig. 2.

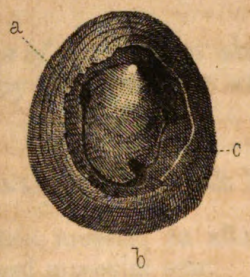
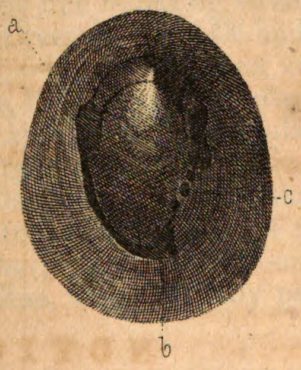


Fig. 3.



Lith by Punderson & Croxand, New Haven Ct

Fig. 1 Anisomyon patelliformis side view.

Fig. 2 Dorsal view of same, with a portion of the shell removed so as to show the muscular scar on the internal cast.

Fig. 3 Internal cast of another specimen of same species, showing muscular scar.

but interrupted and immense, when the starting point is considered. The physical constitution of man has naturally benefitted by it. The details contained in the treatise, of which the present paper forms the introduction, prove that the human race has been gradually gaining in vigor and strength since the remotest antiquity.* The domestic races also, the dog first, then the horse, the ox, the sheep, have shared in this physical development. Even the vegetable soil has been gradually improving since the stone-age, at least in Denmark.

And yet there are persons who deny all general progress, seeing everywhere nothing but decay and ruin, like that worthy specimen of a northern pessimist, who exclaimed, "see how man is degenerated, he has even lost his likeness to the monkey!"

ART. III.—*On a new genus of Patelliform shells from the Cretaceous rocks of Nebraska;*† by F. B. MEEK and F. V. HAYDEN. (With a plate.)

Genus ANISOMYON, M. & H.

Etym. ἄνισος, unequal; μῦών, muscle; in allusion to the unsymmetrical muscular scar.

PLATE I.

Generic characters.—Shell very thin, patelliform, or obliquely conical, with an ovate, oval, or circular base; margins entire; surface nearly smooth, or only marked by obscure lines of growth, crossed on some species by fine radiating striæ; summit more or less elevated, located between the middle and the anterior end, sometimes nearly central,—immediate apex very small, and abruptly curved backwards, but not spiral; interior without a projecting lamina or other appendage. Muscular scar irregularly horse-shoe shaped, enlarged at the extremities, with the open part directed towards the shorter end of the shell; becoming abruptly attenuate, or broken into a row of minute oval or circular spots on the right posterior side;—anterior extremities connected by a slender line, which usually passes across just in front of the summit.

* This agrees perfectly with the testimony of statistics. See Quetelet, *Sur l'homme et le developpement de ses facultés.* Paris, 1835, ii, 271. This work of first-rate merit is very near akin to archæology. Mr. Quetelet has just published a new work which will certainly be even more remarkable than the first, and which the author of the present paper regrets not having had within his reach.

† The specimens belong chiefly to the collections brought from Nebraska by Lieut. G. K. Warren, U. S. Top. Eng. Full illustrations and descriptions of the species will appear in his report.

On the left side of the shell, the anterior extremity of the muscular impression (*a*, fig. 2 and 3, of Plate I.) is generally not so much enlarged as on the right, but sometimes extends slightly farther forward; posteriorly it passes around in the form of a a band to the middle of the slope behind (*b*, fig. 2 and 3), where it is abruptly enlarged and curves upwards. From this point to the larger anterior termination on the right side, there is usually only a slender line (*c*, fig. 2), which is not always quite connected with the enlarged extremity of the band-like part coming around from the left side. Generally this slender line is nearly or quite entire, while in other specimens, even of the same species, it is broken into a series of minute scars as seen at *c*, fig. 3, and in some instances it seems to be entirely obsolete, so as to leave the enlarged anterior extremity on the right, quite isolated.

In most instances, the specimens are found with the small apex (*d*, of fig. 1) broken or worn away, in which condition its former existence would scarcely be suspected. In at least one species (*A. borealis*) this small apex seems to be perforated in the end, the minute aperture being circular, and about large enough to fairly receive the point of a pin. This may be due to accident, but the thickened and smooth margin of this little opening, as seen under a magnifier, has very much the appearance of a natural orifice. We are not sure that this exists in the other species, but suspect it does. In two of the species *A. borealis*, and an undescribed form, there are six equidistant impressed hair lines radiating from the summit down the sides, nearly or quite to the margins, but as there are no traces of such lines on some of the others, presenting the same internal characters, we infer they can scarcely be regarded as a generic character.

From the foregoing description it will be seen that the group of shells we propose to include in this genus, although having the form of *Patella*, present striking differences in the unsymmetrical character of the muscular scar, indicating fundamental peculiarities in the structure of the animal, while they are all much thinner and smoother shells than we usually see in that genus. In some specimens, where there appears to be a complete break in the muscular scar on the right posterior side, there would seem to be some analogy to the genus *Siphonaria*, but as we observe no traces of a siphonal groove passing through this gap, nor any fold in the margin opposite it, and the slender portion of the muscular impression usually passes nearly or quite across, it is scarcely possible any organ such as exists in *Siphonaria*, could have been extruded there.

The more convex species, such as *A. borealis*, are somewhat similar externally to some species of *Hipponyx*, but to say noth-

ing of other differences, the fact that the open extremity of the horse-shoe shaped muscular scar in our shells is always turned towards the shorter end, or in other words, that the apex is placed in front of the middle instead of behind it, shows they have no affinities to that or any allied genus.

They would then seem to be perhaps more nearly related to *Acmæa* and *Gadinia* than to any other of our existing mollusca, since in both these genera the animal is more or less unsymmetrical, the former having the branchial plume exserted from the right side of the neck, and the latter a siphon occupying a groove on the right just in front of the anterior extremity of the muscular scar, which is shorter on that side than on the other. Our shells, however, differ from these genera in the peculiar attenuate or interrupted character of the muscular impression on the right posterior side, and the folding back of the apex.* In the thinness of the shell and the nature of the surface, they are most like *Acmæa*, with which we at first thought them probably identical, but adopting the opinion of M. d'Orbigny that this genus is synonymous with *Helcium* of Montfort, we referred them provisionally to the latter as the older name. Not long afterwards we observed the peculiar character of the muscular impression on an internal cast of one of the species, but at first supposed it merely due to some accident; subsequently however, we ascertained that it exists in five clearly distinct species, and cannot be regarded as an accidental or specific character.

It is probable many of the Cretaceous and Jurassic species that have been referred by different authors to the genera *Patella*, *Acmæa*, *Helcium*, &c., will be found to possess the internal characters of this genus. Judging from the figures of the Cretaceous and Jurassic species of patelliform shells we have seen in published works, specimens showing the muscular scar, have rarely been found. We have observed the characters of this genus in the following Nebraska species:—

ANISOMYON BOREALIS, (=*Hipponyx borealis*, Morton, 1842=*Helcium carinatum*, Meek & Hayden, 1856).
A. SEXSULCATUS, (=*Helcium sexsulcatum*, M. & H.).
A. ALVEOLUS, (=*Helcium alveolum*, M. & H.).
A. PATELLIFORMIS, (=*Helcium patelliforme*, M. & H.).
A. SUBOVATUS, (=*Helcium subovatum*, M. & H.).

Washington, D. C., Nov. 20, 1859.

* Dr. A. A. Gould, the well known conchologist of Boston, to whom we sent sketches of these shells, writes that he concurs with us in regarding them as being clearly distinct from all the recent genera to which such fossil forms are usually referred.

ART. IV.—*General account of the results of the discussion of the Declinometer observations made at Girard College, Philadelphia, between the years* 1840 *to* 1845, *with special reference to the eleven year period;* by A. D. BACHE, Superintendent of the U. S. Coast Survey.

[Communicated to the American Association for the Advancement of Science, by authority from the Treasury Department.]

IT is proposed to give here in outline the results of an investigation of the magnetic observations made with the declinometer, between the years 1840 to 1845, at the Girard College observatory, with special reference to the eleven year period in the amplitude of the solar-diurnal variation and the disturbances of the magnetic declination. Prof. Henry, Secretary of the Smithsonian Institution, has kindly offered to publish the memoir in full in the Smithsonian Contributions to Knowledge. It is my intention to pursue the discussion by taking up the investigation of the lunar influence on the same magnetic element.

In coöperation with the scheme adopted at the British Colonial Observatories, a series of magnetic and meteorological observations were made at the Girard College observatory with instruments purchased under the direction of the trustees of the College, the observations being made under the patronage of the American Philosophical Society, and finally completed for the use of the Topographical Bureau of the War Department. These observations were made under my direction and superintendence. The series commenced in May, 1840, and with short interruptions terminated in June, 1845, thus furnishing a five years series of magnetic observations taken bi-hourly up to Oct. 1843, and after that date hourly. The readings of each magnetic element were united into means, and were also presented graphically (in the fourth volume of the record). This was done under my direction by J. Ruth, Esq., but owing to other laborious duties the record could not be submitted to a more complete reduction. I have now resumed the subject by the assistance of Charles A. Schott, Esq., Assistant in the Coast Survey, by whom, under my immediate direction, and as my assistant in this special matter, the present paper has been prepared.*

Although other magnetic observatories furnish by their judicious geographical location, a basis for the generalization of their results, it is nevertheless desirable to obtain results from intermediate observatories as confirmations or as corrections. In the investigation of the disturbance-law at Point Barrow, when compared with the same at Toronto, a very remarkable mutual

* It may be proper to state that this work has been performed out of Office hours, and at my own expense.

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relation of the law at these stations resulted from such a comparison, and farther examination may bring to light other dependencies of a mutual character.

According to the latest determination the position of the Girard College observatory is in latitude 39° 58′ 23″ (north), and in longitude 75° 10′ 05″=5^h 00^m 40^s·3 west of Greenwich. From it Toronto bears 38° 45′ west of north (true) and is distant about 334 statute miles.

It is proposed specially to investigate the law of the eleven year period, or, as it is more frequently called, the decennial period, there being yet an uncertainty as to its precise length. It is supposed to have some direct or indirect connection with the solar spot period, which correspondence, according to late investigations by Prof. R. Wolf, is so close as even to exhibit analogous disturbances. The following discussion will afford a contribution towards the determination of the epoch of the occurrence of a minimum in certain phases of the magnetic variation and disturbances, corresponding to a minimum of the solar spots. The method of reduction is substantially the same as that adopted by General Sabine. Earlier investigations of Dr. Lamont and those by Mr. Kreil differ from his in not including the discussion of the disturbances in connection with the period in question.

As long as the magnitude of the deflection remains the only criterion by which a disturbance may be recognized as such, the adoption of any limit of deviation from the normal value of the same hour, month and year, must necessarily remain in some measure arbitrary, or, in other words, there must always remain after the separation of the disturbances a certain small amount of their effect in the remaining regular diurnal progression. To effect the separation, Peirce's criterion has been used with entire success. After a preliminary investigation as to the number of disturbances separated, the limit, as pointed out by the criterion, or a deviation of 8 scale divisions (or 3′·6 of arc) has been adopted in the present discussion, as constituting a disturbed observation. Accordingly all observations differing by that amount or more from the mean monthly value of their respective hour were marked by a pencil line. Next a new hourly mean was taken, omitting values so marked, and each observation was again examined in reference to deviation from this new mean. This process was repeated when necessary, so that in all cases, values differing eight scale divisions or more from the mean were excluded. The last mean thus obtained for each observing hour and each month has been called "the normal." These values have been tabulated and are given for each month and year separately, together with such corrections as the omissions or interruptions demanded. The bi-hourly, and afterwards the hourly readings (and their means) were made 19½^m after the hour so as to correspond to an even Göttingen hour (diagram A).

For the purpose of comparing the annual means of the normals, or the mean march of the regular solar diurnal variation for each year, the results have been expressed analytically by means of Bessel's formula, and by the application of the method of least squares.

Probably owing to the several accidental changes in the suspension of the bar, and consequent uncertainty in the precise amount of scale correction, the mean readings of each year, when compared with one another, exhibit differences not actually due to irregularities occasioned by declination changes. Though this question does not directly bear upon the present investigation, which mainly depends on differences of readings, it will be proper to remark that the observed increase, giving the weight $\frac{1}{2}$ to the mean of 1840 and 1845 (on account of incomplete record) is under the supposition of a uniform annual change between these years, equal to 4′·50. According to Mr. Schott's latest investigation of the secular change of the declination in Philadelphia supported by observations between the years 1701 and 1855·7, the annual increase between the years 1840 to 1845 is 4′·98, a result which accords tolerably well with actual observations. According to this formula the declination on the 1st of January, 1843, the mean epoch of the present series, is 3° 32′ West with a probable error of ±10′. This declination corresponds to the scale reading 560·31, which has been deduced by taking into account the weights of the annual means.

The expressions have been thrown into curves (diagram B), and the agreement between computed and observed values is shown by the introduction of dots giving the observed reading. The probable error of any single representation is ±0′·1. By means of the formulæ the following values were computed.

For	Epoch of maximum eastern deflection.	Correspond. scale reading	Epoch of maximum western deflection.	Correspond. scale reading.	Amplitude (in arc.)
	h m	*d*	*h m*	*d*	′
1840	7 26 A. M.	595·67	1 34 P. M.	575·71	9·08
1841	7 49	577·96	1 49	560·21	8·06
1842	7 36	571·24	1 37	553·96	7·83
1843	7 40	569·54	1 24	553·06	7·46^m
1844	7 32	556·50	1 18	539·99	7·51
1845	7 34	536·65	1 16	517·81	8·53
Mean	7 36 A. M. ± 3^m		1 30 P. M. ± 4^m		

The inequality constituting the ten or eleven year period is plainly exhibited in the last column, the progression of the numbers being quite regular; the year 1843 is directly indicated as the year of the minimum range of the diurnal fluctuation. By means of a special formula, deduced by least squares, and

representing a single value within $\pm 0'.11$, the month of May, 1843, is indicated as the epoch of minimum amplitude.

The discussion of the disturbances, as far as they bear on the decennial inequality, next follows, taking in also some collateral results.

The total number of observations for changes of declination recorded and discussed amount to 24,566; of these, according to the preceding investigation, 2,357 were separated as disturbances. There is one disturbed observation in every 10·4 observations. The discussion of the disturbances is divided into two parts, that of the number, and that of the amount of the deflections. Omissions in the record have been supplied by the use of proper ratios showing the law as given by the full periods, and interpolated values are enclosed within brackets. The number of disturbances in each month of the year or the annual inequality in the distribution of the disturbances has been made out for each year, and the means and ratios are also given. The principal maximum occurs in October (at Toronto in September), the secondary in April; the two minima, nearly equal in amount, occur in February and June (the first one in January at Toronto). The ratios of the number of monthly disturbances to the average number are given in the following table, showing the same also divided into westerly and easterly values.

RATIOS.				RATIOS.			
	W.	E.	Total.		W.	E.	Total.
January,	1·27	0·42	0·77	July,	0·58	1·18	0·86
February,	0·70	0·46	0·52m	August,	2·00M	1·14	1·59M
March,	0·83	0·57	0·68	September,	0·93	1·86	1·36
April,	0·95	0·80	$0·91M_2$	October,	1·58	2·50M	2·12M
May,	0·55	0·60	0·58	November,	0·96	1·12	1·08
June,	0·44	0·61	0·53m	December,	1·21	0·74	1·00
				Mean,	1·00	1·00	1·00

The ratios show a general correspondence in the numbers of westerly and easterly deflections; the westerly seem to occur most frequently in August, while the easterly predominate in October; the secondary maximum of both series is in April. For the total number the minima occur in February and June. The following table contains the number of disturbances in each year.

1840	483	weight ½
1841	539	
1842	446	Proportional number of western disturbance 987, of eastern 912; at Toronto the eastern predominate over the western in the proportion of 1·17 to 1.
1843	275	weight ½
1844	308	
1845	264	weight ½

These numbers do not indicate the law of the eleven year period as plainly and systematically as found by the investigation

of the diurnal amplitude, yet giving proper weight, (on account of deficiencies,) the minimum number of disturbances falls in the year 1843.

If we distribute the disturbances, 1,942 in number for the even hours, according to their respective hours of occurrence, we find the following ratios:

Philadelphia mean time.	RATIOS. W.	RATIOS. E.	Total number.
h *m*			
0 (19½)	0·82	1·20	162
2 "	1·18	1·16	189M
4 "	1·08	0·96	168
6 "	1·35	0·80	178
8 "	1·29	0·70	161
10 "	1·32	0·88	178
Noon "	1·13	0·71	150
14 "	0·95	0·67	133m
16 "	1·07	0·78	148
18 "	0·87	0·90	143
20 "	0·40m	1·66M	167
22 (19½)	0·54	1·58	170

The numbers in each column show a regular progression; the disturbances, irrespective of their direction, have a minimum at 2 P. M., and a maximum at 2 A. M., (at Toronto the respective hours are 2 P. M. and 22 P. M.). The principal contrast is between the hours of the day and the hours of the night. In the table given above the most striking result is that the westerly disturbances have their minimum precisely at the hour (8 P. M.) when the easterly have their maximum, and the exact coincidence of this result with that deduced by General Sabine for Toronto is not less remarkable. In connection with this subject, it may be remarked that the same distinguished magnetist found a singular mutual relation to subsist between the phenomena at Toronto and Point Barrow, on the shores of the Arctic sea,—the laws of the easterly deflection at one station being found to correspond at the same local hours with those of the westerly deflections at the other station, and vice versa. This contrast therefore holds good for Philadelphia as well as Toronto.

If we classify the disturbances according to their amount, we obtain the total aggregate and mean values of a single disturbance in the different years as follows:

	Aggregate amount in scale divisions.	Mean value.	Mean value. W.	Mean value. E.
	d	′	′	′
1840	7155·5	6·70	6·52	7·20
1841	7844·4	6·61	5·93	7·07
1842	6019·1	6·11	5·53	6·70
1843	2932·2	4·85	4·85	4·85
1844	4227·3	6·21	6·21	6·21
1845	3521·4	6·02	5·25	6·84

The eleven year period is well marked in the aggregate as well as in the mean values, and the precise epoch of the minimum was found by a special formula. It took place in August, 1843, and as a resulting epoch from this and the previous determination, June, 1843 may be adopted. This is graphically represented on diagram C.

The following table gives the ratios of the aggregate amount of disturbances in each month of the year:

	Total.	W.	E.		Total.	W.	E.
January,	0·72	1·21	0·33	July,	0·87	0·46	1·29
February,	0·54m_2	0·59	0·46	August,	1·61	1·95	1·28
March,	0·66	0·73	0·56	September,	1·56	0·99	2·12
April,	0·94M_2	1·04	0·84	October,	2·06M_1	1·67	2·46
May,	0·56	0·52	0·56	November,	1·06	1·09	1·02
June,	0·42m_1	0·37	0·47	December,	1·00	1·38	0·61

In reference to the first column, the maximum amount of disturbances occurs in October (at Toronto in September); the minimum in June (as at Toronto); the secondary maximum occurs in April (the same at Toronto); the secondary minimum occurs in February (and at Toronto in January). If separated into east and west deflections, maxima occur in September (mean of August and October) and April; and minima in June and January (same as at Toronto).

The arrangement according to the hours of the day gives the following ratios, to which is added, in the last column, the diurnal disturbance variation obtained by dividing the excess of aggregate westerly over easterly values by the total number of days of observation.

Philadelphia mean time.	RATIOS.			Disturbance, variation in arc.
	W.	E.	Both combined.	
h m				′
0 (19½)	0·83	1·24	1·04	−0·16
2 "	1·16	1·10	1·13	−0 01
4 "	1·16	0·92	1·04	+0·05
6 "	1·46	0·67	1·06	+0·24
8 "	1·39	0·67	1·02	+0·22
10 "	1·22	0·77	0·99	+0·13
Noon "	1·03	0·63	0·83	+0·11
14 "	0·98	0·63	0·80m	+0·10
16 "	0·99	0·72	0·85	+0·07
18 "	0·84	0·89	0·84	−0·02
20 "	0·88m	1·88M	1·15	−0·53
22 (19½)	0·56	1·88	1·25M	−0·47

If we compare these ratios with the corresponding numbers in a preceding table showing the bi-hourly distribution in regard to number, we find, irrespective of the direction of the deflections, the 2 P. M. minimum preserved. The maximum is earlier and occurs at 10 P. M. (At Toronto these hours are respectively 1 P. M. and 9 P. M.) At Philadelphia as well as at Toronto the

ratios are nearly invariable from 10 A. M. to 6 P. M., and again from 8 P. M. to 8 A. M. The easterly maximum and westerly minimum at 8 P. M. form again a marked feature.

The law governing the disturbances during a solar day is clearly shown, and is of a systematic character. The diurnal variation caused by the disturbances, if superposed on the regular diurnal variation, would produce what is known by the name "mean diurnal variation." If we plot the disturbance curve on the same scale, or actually superpose it on the curves of the regular diurnal variation, it would hardly show to the eye, and the compound curve of the mean variation would keep within the maximum distance of a dot from the regular curve in the diagram (D). The disturbance variation has but one maximum and one minimum. Its most prominent feature is the easterly deflection at 8 o'clock ($+19\frac{1}{2}^m$) P. M. (at Toronto it is at 9 P. M.); the maximum deflection amounts at that hour to 32″ of arc, and to 45″ at Toronto. The greatest westerly deflection occurs at 6^h ($19\frac{1}{2}^m$) A. M. and amounts to but 14″ (at Toronto the hour is 8 A. M. with 6″, and from a five years series of observation, with 31″ of deflection). The range of the disturbance variation equals 46″. From 3 in the morning till 5 in the afternoon, the mean effect of the disturbances is to deflect the north end of the magnet to the west, and during the remaining hours (principally night hours) to the east. The westerly and easterly disturbance deflections, during a day, balance within 0′·02.

The annual inequality in the amplitude of the diurnal disturbance variation cannot satisfactorily be shown on account of the short and partly interrupted series of observations.

It is my intention to continue the discussion of the observations made at the Girard College Observatory.

After the above was written No. 1185 of the Astronomische Nachrichten came to hand, containing Prof. R. Wolf's interesting results on the close connection of the variation in the frequency of the solar spots, and the corresponding inequality in the amplitude of the diurnal variation of the declination. He deduces for Munich the simple formula, $\beta = 6'{\cdot}273 + 0'{\cdot}051\,\alpha$, where α represents a relative number expressive of the frequency of the solar spots, directly derived from observation, and β the amplitude of the diurnal variation. He found a close correspondence between these phenomena, showing the observed and computed amplitude for the Munich observations between 1835 and 1850. The average length of the solar spot period is reaffirmed to be 11·11 years $\pm$ 0·04 years. For Philadelphia we obtain $\beta = 7'{\cdot}080 + 0'{\cdot}039\,\alpha$, which formula represents the observations as follows:

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Year.	α From solar spot observations.	Amplitude derived from α.	Observed amplitude.	Difference.
		′	′	′
1840	51·8	9·10	9·08	−0·02
1841	29·5	8·23	8·06	−0·17
1842	19·2	7·83	7·83	0·00
1843	8·4	7·41	7·46	+0·05
1844	12·2	7·55	7·51	−0·04
1845	32·4	8·34	8·53	+0·19

The correspondence between the observed diurnal amplitude and the same computed from observations of the solar spots is further exhibited in the annexed diagram (E). The dotted curve is the approximate Toronto curve from observation specially introduced to show the agreement at the epoch of the maximum in 1848. By computation from the solar spot observations, the amplitude at that time would amount to 11′·00, the whole range of the inequality of the diurnal variation would therefore be 11′·00−7′·46=3′·54.

It is much to be desired that this interesting branch of physical enquiry be further studied, as it forms one of the links connecting terrestrial with cosmical phenomena.

ART. V.—*A Visual Method of effecting a Precise Automatic Comparison of Time between distant stations;* by JONATHAN HOMER LANE. (With a plate.)

THE visual apparatus of which I here give a general description was invented several years ago, and is intended to supply the place, under certain circumstances, of the electric telegraph, in the determination of differences of longitude. Although the wires of the electric telegraph, when suspended in the air, appear to leave nothing to be desired, at least for distances of a few hundred miles, in situations where they are available, yet it has appeared to me that the visual method I propose may prove useful in many cases where the stations to be compared, particularly the astronomical stations in a trigonometrical survey, are removed to considerable distances from lines of electric telegraph. In those situations, also, where submarine or subterranean lines take the place of air lines, the visual method, on account of the comparatively slow velocity of the electric signal along the wires of such lines, and the open and irresolvable question whether the signal time might not be greater in one direction than the other, would be capable of furnishing a useful check upon the indications of the electric telegraph.

The general features of the method are the following:

First, an intense light shown at one station, A, and viewed at the other station, B, as a star.

Secondly, a uniform automatic movement at A, made to interrupt the light at regular minute periods of say one sixteenth of a second from the middle of one interruption to the middle of the next.

Thirdly, a uniform automatic movement at B, by which the star of light is made optically to travel around a circle at the rate of one revolution in the same period of one sixteenth of a second, the consequence of which is that the periodical interruption becomes visible to the eye as a break in the luminous circle produced by the motion of the star, and according as such break is seen upon one part or another of the circumference of the luminous circle, the relation of the movement at A to that at B becomes, inside of the recurring period of one sixteenth of a second, known in like manner as if they had been connected by a coupling shaft extended from one to the other.

Fourthly, a supplementary flash of the light at A, occurring at each whole second and during the interval of one of the interruptions before mentioned, which supplementary flash finds itself subject, in the movement at B, to an action by which it would be carried optically around a circle once only in one whole second, and by the position at which it occurs on the circumference of that circle indicates the sixteenths of the whole second. Also, a still further signal, by which account may be taken of the whole seconds, in ways too obvious to require special notice, and which will complete the knowledge of the relation of the movements at A and B to each other, or of the quantity by which one may be in advance of the other in its motion.

Fifthly, any of the known methods of effecting automatic comparison of the movement at either station with the clock at that station, by which means the comparison of the clocks at the two stations will be made to the minutest fraction of a second. Or, a single clock may be used, say at A, and any observation at B can by known methods be automatically referred to the movement at that station, and thus compared at once with the clock at A.

Further, if the automatic movement at B, besides giving optical motion to the star of light shown from A, is simultaneously made to produce periodical interruption of another intense light shown at B and seen at a third station, C, provided with a movement like that at B, the comparison from A to B may be extended directly onward from B to C, and from C onward to a fourth station, and so on, and such is the degree of precision which seems, so far as we can judge without direct experiment, attainable in the comparison between contiguous stations, that the probable error of a single comparison between the extremes of a line of twenty stations may, I believe, be made smaller than the hundredth part of a second.

An arrangement suitable for carrying the above plan into effect is illustrated by a sketch in the accompanying plate. At the station A, the rays of the signal light, diverging from their source L (Plate II, fig. 1, *a*), are converged by a lens C so as to meet and cross each other in the focus F of a telescope. Diverging from this point they traverse the object glass O, and issue from it in a nearly parallel beam directed to the station B. The light thus transmitted is periodically intercepted at the focus F by the projecting teeth of a rotating disc, or signal wheel, S, which is made to rotate uniformly at the rate of one revolution in about one second.

The telescope used at the station B for viewing the light shown at A, is furnished with a terrestrial eye-piece, and at that point where occurs the first image of the object glass, or crossing of all pencils of light that can pass through the telescope, is introduced a refracting glass prism P, shown in section in Fig. 1, *b*. The pencil formed by the light from A, on traversing the refracting prism, is turned aside so that the star image, which otherwise would be formed at *s*, is formed at *s'*, and the displacement is observed by the eye at E. Since this displacement is always in a plane at right angles to the edge of the refracting angle of the prism, if the latter be made to rotate on an axis *zz*, parallel to the axis of the telescope, the displaced star image *s'* will travel in a circle around *s* as a center. If the period of revolution be shorter than the duration of the luminous impression on the eye, and the light be unintermitting, the circle described by *s'* will appear to the eye as a continuous circle of light. The period of one sixteenth of a second may perhaps be taken as sufficiently small for continuity of luminous impression. Accordingly, if the prism be made to revolve about sixteen times per second, and precisely sixteen times during each revolution of the signal wheel S, and if the primary division of the latter be made by sixteen equidistant slotted openings in its border, then the luminous circle, which but for the interposition of the signal wheel would appear continuous and entire, will be seen in part obliterated, as shown in Fig. 3, the luminous part S′ D S′ having the same ratio to the obliterated part S′ G S′, that the width of one of the slotted openings of the signal wheel has to the width of a tooth. And as the luminous arc S′ D S′ appears on one part or another of the circumference, its angle of position, which may be observed by bringing the wire of a position micrometer into coincidence with the extremities of the arc, will determine the angle of position at which the prism arrives simultaneously with the arrival of the signal wheel at a given point of reference.

But for counting the whole sixteenths of a second it will be required to know also the simultaneous angle of position of a

wheel geared with the prism P so as to revolve only once in the time of one revolution of the signal wheel. The circular prismatic piece is of a diameter many times greater than that of the pencil of light, and the latter traverses the former at its border. Around the prismatic piece is fastened a toothed ring, into which gears the driving wheel H from which the prism takes its motion. Between this wheel H and the regulator from which it derives its uniform motion, is interposed a satellite wheel arrangement, by means of which the observer, without disturbing the invariable velocity of the regulator, can set the wheel H, and the parts to which it gives motion, forward or back in their course, and then allow them to proceed *at once* with the same correct velocity as before. In this way the observer will have absolute control of the angle of position of the luminous arc S′ D S′, and it may be agreed that as this angle of position slowly changes in consequence of the want of *perfect* unison between the movements at the two stations, he shall, from time to time, bring the luminous arc back to near coincidence with a standard position, that for instance which is shown in Fig. 3, the exact angle of position to be measured, however, in the manner above mentioned. Provided, then, the arc be not allowed to stray far from its standard position, it will be obvious that one part of the border of the prismatic piece P will never be traversed by the light which passes the sixteen primary openings of the signal wheel and forms that arc. The part thus unused is made with parallel faces, as shown in the figure, and then any supplementary flash of light occurring midway between the primary ones, will pass through the parallel part of P unrefracted, and may be refracted by a second prism P′, that moment interposed. This second prism is made to revolve once in the period of the sixteen revolutions of P, and in the best mode of construction that occurs to me is one of sixteen prismatic pieces P′, P′, P′, &c., so cut out, and attached to the border of a wheel or disc K, made to revolve in that period, that the edges of the refracting angles of all of them shall be parallel to each other, the whole forming the equivalent of a single prism cut into a large toothed wheel. This wheel K, like P, takes its motion from H, and is so geared that during all the intervals of time in which a passing pencil would encounter the refracting part of P, it will have free passage through one of the spaces between the pieces P′, P′, P′, &c., which, during the alternate intervals of time, will in their turn be interposed in the path of the pencil. Any flash of light, therefore, that escapes through any supplementary opening, as *t*, in the middle of one of the sixteen primary teeth of the signal wheel, will, in traversing the telescope at B, be refracted by one of the prisms P′ alone, and not by P. And if it be recollected that the several prisms P′ are in effect parts of one prism, as dis-

tinctly indicated in fig. 1, *b*, and that the action of this differs in no respect from that of P except in its longer period of revolution, and that this period of revolution is the same with that of the signal wheel, it will be obvious that if several supplementary openings be made in the signal wheel, as for instance two, *t* and *u*, in two teeth diametrically opposite to each other, and two others, *v* and *w*, in teeth adjacent to one of these on each side of it, the flashes of light through these openings will be seen by the eye at E to occur at points *t'*, *u'*, *v'*, *w'*, fig. 3, distributed around the circumference of a circle concentric with S' D S' G, and at angular intervals from each other identical with those between the corresponding openings in the signal wheel. It will be further obvious that the observed angle of position of the diameter *u' t'* will depend on the angle of position of the wheel K relatively to that of the signal wheel. The diameter *u' t'* becomes, then, by aid of the more exact indication of S' D S', an index by which we know the required angle of position at which the wheel K arrives simultaneously with the arrival of the signal wheel S at a given point of reference. As the most convenient mode of procedure in practice, the observer at B may operate the satellite wheel until the index diameter *u' t'* is brought to a position, for instance the vertical one in the figure, made to denote zero, and the position micrometer for taking the angle of position of S' D S' may be graduated to thousandths and ten thousandths of a second.

Instead of the above described arrangement there is a modification of it which I am disposed to prefer, the type of which is a pair of telescopes at station B, placed side by side so that one may contain the rapidly revolving prism and the other the more slowly revolving one, each prism being in this case unintermitting in its action, and the supplementary openings of the signal wheel being replaced by the filling up of a single one of the primary openings. The omission of the flash of light from this one would be observable through the slow prism and give the required indication, while it would not probably injure in any material degree the distinctness of the arc of light seen through the fast prism. Instead of a pair of complete telescopes, the equivalent of a pair of eye-pieces with a sliding object-glass to alternate between them at pleasure, would answer the same purpose. In this arrangement no rectification of the prisms by the observer would be necessary, it being always possible to observe the total deviation. This would be a great advantage on a line of very numerous stations, in which case it would, on the first described plan, be a somewhat critical matter to bring all the instruments on the line into the required correspondence for simultaneous observation.

As before intimated, it would be possible to employ but a single clock on the whole line of stations, but as this would require signal observations for every time observation at any other than the clock station, it would be more convenient to employ a clock at every astronomical station.

The question of the feasibility of the process described in this paper will depend primarily on the practicability of securing, with telescopes of moderate aperture, a sufficiency of light for such distances as from fifty to eighty miles, and next on the attainment of sufficient precision of rate in the uniform motion employed. I do not anticipate serious difficulty in either of these things. For the uniform motion, considering especially the light work it will have to do, the Fraunhofer regulator would I presume be everything that is required, or an electromagnetic regulator, similar to that described by me in a paper presented to the American Association at their meeting at Montreal, may be used if found reliable. From what a scientific friend has told me of his experience with distant lights, I think we are justified in anticipating the easy attainment of sufficiency of light.

A similar optical means can also be used for comparing a mean time clock at one station with a sidereal clock at another, by the method of coincidences, without other mechanism than the clocks themselves, though with diminished power of precision on a long line of stations. The pendulum of the one clock is made to carry in the focus of the telescope at its station, an opaque disc with a narrow slit, through which, at each oscillation, a flash of light is allowed to escape to the other station, and through the focus of the telescope at that station oscillates a wire carried by the pendulum of the other clock, which eclipses the flash of light at each coincidence of the two pendulums. Or the pendulum at the observing station may carry a mirror, in which either a flash or an interruption of light from the other station may be observed by reflection, and the coincidence noted when the flash or the break is seen at the same point of the field of view where it is observed with the pendulum at rest.

I have already observed that the visual method proposed in this paper might prove useful as a check, at least, upon the indications of submarine or subterranean lines of electric telegraph. But it seems less liable to uncertainty in its indications than even the air lines, the signals of which occupy a very appreciable and more or less ambiguous time in passing, and therefore on very extensive surveys it would be very instructive at least, and might be found to give increased accuracy, to add to the comparisons made by the telegraph wires, further comparisons by means of a sufficient number of the visual instruments to reach across the whole extent of the survey. In case it should

ever be undertaken, as has been proposed, to measure an extensive arc of the equator, the idea of such a visual method for the accurate determination of the differences of longitude, would be well worth considering.

I will close by suggesting one more obvious application of the method, and that is, the determination of the velocity of light, which, with a sufficiently high velocity of revolution of the prism and signal wheel, might be done with considerable accuracy by transmitting, in the same manner as before described from a second station to a third, a return signal from the second station to the first.

ART. VI.—*On Osmious Acid, and the position of Osmium in the list of Elements;* by J. W. MALLET, Prof. of Chemistry, &c., Univ. of Alabama.

IN most chemical text-books it is stated, on the authority of Berzelius, that there are five oxyds of osmium—OsO, Os_2O_3, OsO_2, OsO_3, and OsO_5—of which however the second and fourth have not been isolated, although compounds containing them are known. To these may be added a blue substance, first obtained by Vauquelin and supposed by Berzelius to consist of OsO united to either Os_2O_3 or OsO_2, and the highest oxyd, probably OsO_5, the existence of which was announced by Frémy in 1854.

While preparing osmium from some black platinum residues I have accidentally obtained, a substance which there is some reason to believe may be osmious acid—the hitherto unisolated teroxyd—mixed indeed with osmic acid, but still permitting certain of its properties to be observed.

Three or four ounces of the platinum residue were treated by a modification of the original process of Wollaston, now seldom adopted. The powder was mixed with three times its weight of nitre, the mixture was fused for some time in an iron crucible, and then poured out upon an iron plate. While still warm the fused cake was broken into fragments and put into a flask fitted with a cork, through which passed a tube two feet long, bent at right angles, and a funnel-tube, the latter drawn out to a very small bore at the lower end, and reaching to the bottom of the flask. The bent tube was well cooled, and *undiluted* oil of vitriol was very cautiously poured, by a few drops at a time, into the funnel.

The acid produced intense heat on coming in contact with the cake of potash salt, and oily drops of a bright *yellow* color began to make their appearance in the cooled tube. These drops very slowly congealed to a solid resembling unbleached bees-wax.

By the time the sulphuric acid had been added in slight excess a considerable quantity of this yellow substance had collected in the tube and in a receiver attached. By gentle heating the whole was obtained in the receiver, and united under a little water to a single mass. Towards the end of the distillation *colorless* needles and fused drops of the well known osmic acid came over, and doubtless a considerable portion of the yellow mass in the receiver consisted of the same.

At first it seemed probable that the yellow color of the latter was due merely to some impurity, and it was therefore cautiously resublimed, but it again collected of the same tint as before. It appeared to be even more fusible and volatile than osmic acid; it took a long time to congeal under a stream of cold water flowing over the outside of a tube in which it had been melted.

The water in which it was fused acquired a bright yellow color, and gave off fumes, the odor of which seemed to me somewhat different from that of osmic acid, and which irritated the eyes so insufferably that it was scarcely possible to finish work with the acid and put it up for preservation. It was removed as a single cake from the water, and sealed up hermetically in a glass tube which had been previously cleansed with care from all traces of dust or other organic matter. The water in which it had been fused was mixed with caustic potash, and gave a solution of very dark brown-red color, such a tint as would probably result from a mixture of the red* *osmite* of potash discovered by Frémy with the orange-brown *osmiate* of potash.

The sealed tube containing the fused cake or stick of yellow acid was allowed to remain upon a table exposed to the direct rays of the sun. The acid immediately began to sublime upon the sides of the tube, not in long needles and prismatic crystals like osmic acid (which seems to be monoclinic), but in feathery crusts like sal-ammoniac, which under a lens had somewhat the appearance of minute octahedrons grouped together. The color was still bright yellow, but in a short time the sublimed acid began to turn black, and in twenty-four hours the whole inner surface of the tube was perfectly black and opaque. A tube containing pure colorless osmic acid has been exposed in a similar way to the sun for three weeks without any such blackening taking place. A tube closed by a cork, or one from which dust has not been carefully removed will often cause osmic acid to turn dark, but never exhibits anything like the absolute blackness and opacity of the whole tube noticed in the present instance.

* A rose-red color is also characteristic of the salt supposed by Berzelius to be the ammonio-*terchlorid* of osmium, corresponding in the chlorine series to *osmite* of ammonia.

It is easy however to imagine the cause of this change undergone by the yellow acid if it be in fact the teroxyd of osmium (mixed with osmic acid). The teroxyd probably broke up into osmic acid and one of the lower oxyds of osmium or perhaps the metal itself. We might have

$$2OsO_3 = OsO_4 + OsO_2$$
$$5OsO_3 = 3OsO_4 + Os_2O_3$$
$$3OsO_3 = 2OsO_4 + OsO,$$

or,
$$4OsO_3 = 3OsO_4 + Os.$$

In order to ascertain, if possible, which of the above changes had taken place, the tube was opened two or three months after it had been sealed, and the contents were examined. The fused stick of acid was found to be black and partially friable; on heating in another glass vessel most of it sublimed, leaving a little black powder behind, and condensed in needles, still slightly yellowish, but differing little in appearance from common osmic acid. The inner surface of the original tube was found coated with a thin filmy, adherent crust, of a black color and considerable lustre. This was scraped off, and a portion of it gently heated in a stream of dry carbonic acid gas until all traces of adherent osmic acid were driven off. After cooling, the carbonic acid was replaced by dry hydrogen, and heat was again applied. Water condensed on the tube beyond the heated part, thus proving that an oxyd of osmium, not the metal, was under examination. Replacing again the hydrogen by oxygen, osmic acid was produced and carried off with the stream of gas. The black powder scraped off from the original tube was heated with hydrochloric acid, and seemed to be but slowly acted on; the acid however assumed a green color, and hence it is probable that the osmium existed as protoxyd.

It is not easy to see, without further investigation, how osmious acid could have replaced in part osmic acid in the attempt to prepare the latter as above described. Is there a particular stage of the decomposition of nitre by heat at which osmium may replace nitrogen in nitrite of potash (KO, NO_3)? From the relations of the two elements, to be noticed presently, this would seem probable, and in fact Frémy has noticed the crystallization of osmite of potash from a solution in hot water of the fused cake of nitre and iridosmium. A reason for osmic acid (OsO_4) being usually obtained from the latter, instead of osmious (OsO_3), might perhaps be found in the fact that the chemists who of late years have worked upon osmium recommend the use of nitric or nitro-muriatic acid to neutralize the potash—sulphuric acid, to which Wollaston had recourse in his early experiments, is now seldom employed. Thomson, in his "Chemistry of Inorganic Bodies," published many years ago, observes that osmic acid has sometimes a tint of yellow.

It does not seem likely that the cork closing the neck of the flask used for distillation had anything to do with the production of osmious acid, if such took place; the cork itself did not show any appearance of being acted on, and there was no blackening of its surface until some time after the experiment was ended.

The reduction of osmic acid generally results in the formation of the basic oxyds; Berzelius, however, observed that on adding sulphurous acid to a solution of osmic acid the latter passed through various shades of color—*yellow*, *orange-yellow*, brown, green, and at last blue; he attributed these tints to the successive formation of sulphates of the bin-oxyd, sesqui-oxyd, and blue oxyd; but may not the first step in the reduction have been osmious acid, giving the yellow color?

Another and altogether different view of the nature of the volatile yellow substance above described was suggested as possible by some remarks of Claus in a recent paper on the tendency to reduction of salts of iridium (Ann. d. Chem. u. Pharm., Aug., 1858, S. 129). This author has shown that the platinum metals fall naturally into these groups, in each of which are contained two metals resembling in general habit and relations each other more closely than members of the remaining groups. Platinum and palladium constitute the first of these pairs, iridium and rhodium the second, osmium and ruthenium the third. The atomic weight of the first-mentioned member of each pair is higher than (nearly double) that of the second.

In the paper quoted Claus remarks that the metal of lower atomic weight in each of these groups is much more easily reduced than the other from superior to inferior grades of combination with chlorine; thus the bichlorid of palladium is reduced with much greater ease to proto-chlorid than is the corresponding compound of platinum; and, for the same season, probably, the bi-chlorid of rhodium is not known, but only the sesqui-chlorid, while both salts of iridium can be easily obtained. On this same principle Claus explains the fact that no oxyd of ruthenium homologous with osmic acid has been obtained, while he gives the following reasons for suspecting the existence of such an oxyd: "This opinion is based upon the fact, that in my preparation of compounds of ruthenium, which can be obtained only by energetic processes of oxydation, the material worked upon, notwithstanding all my care and economy, gradually diminished, and yet I have never succeeded in collecting a volatile product. Once only, when I had fused ruthenium, perfectly free from osmium, with caustic potash and nitre, dissolved the mass in water, and decomposed it with nitric acid, I observed a peculiar odor, quite distinct from that of osmic or nitrous acid; and afterwards, having covered the beaker, which was smeared on the edge with tallow, with a plate of glass, I

remarked an unmistakable blacking of the tallow, caused by the reduction of a volatile metallic compound."

It seemed possible that the volatile yellow substance to which the present paper refers might have been an acid oxyd of ruthenium*—RuO_3, RuO_4, or RuO_5—and reducible with extreme facility, Claus and others having already noticed the reducing effect of light upon salts of the platinum metals. A portion of the crust from the sides of the tube of yellow acid was carefully examined for ruthenium, the various tests given by Claus as well as that recently proposed by Dr. Gibbs being made use of, but no proof of the presence of this metal could be obtained.

The properties of osmium and its compounds are very remarkable, and render it a matter of no little interest to trace the analogies of this rare substance and fix its place among the other elements. It is described in most chemical works along with platinum and its associated metals, mainly on the ground of community of origin, for in many respects it is unlike the platinum, palladium, rhodium, &c., with which it always occurs in nature. All these metals are commonly thought of as very infusible, of great density, very slightly affected by reagents, and very easily reduced from their compounds to the metallic state; when more closely examined they are found to differ from each other in many of their other properties. The arrangement by Claus of the platinum metals in these groups, each containing one metal of high and one of low atomic weight, viz.

Platinum,	Iridium,	Osmium,
Palladium,	Rhodium,	Ruthenium,

has been alluded to above; the two members of each group are more closely related to each other than to any of the rest. Osmium and ruthenium are clearly the most electro-negative of the series. Graham has inferred the isomorphism of platinum, palladium, iridium and osmium, from the fact that their potassio-chlorids all crystallize in the form of the regular octahedron; the corresponding compound of ruthenium has since been added to the list, while that of rhodium is still unknown. The occurrence of two salts under the same form, *in the regular system*, of

* If such a compound exist, an explanation may be found for the process by which Frémy has obtained a lower oxyd of ruthenium—probably the bin-oxyd—in crystals. He roasts the powder of platinum-residue in a stream of air drawn through a porcelain tube at a bright red heat; osmic acid volatilizes, and is said to carry with it *mechanically* the oxyd of ruthenium, which deposits upon fragments of porcelain placed in the cooler part of the tube. But the oxyd is in distinct crystals, and can therefore scarcely be conceived of as a powder borne along in a merely mechanical way by a stream of vapor; and, moreover, there is no reason for oxyd of ruthenium only being so borne along, while other substances of no greater density remain behind. Is it not more likely that a volatile and very easily reducible homologue of osmic acid is formed, and almost immediately afterwards decomposed, depositing the bin-oxyd of ruthenium?

course does not of itself suffice to establish the relation of isomorphism between them; iridio-chlorid of potassium seems however to be capable of crystallizing in all proportions with the platino- and osmio-chlorids.

The interesting fact has been discovered by Claus that osmio-cyanid and ruthenio-cyanid of potassium are strictly isomorphous with the well-known ferro-cyanid, crystallizing with it in all proportions, and even giving very similar precipitates with various metallic solutions; so that, in these double cyanids, osmium and ruthenium are capable of taking the place of *iron*.

In the greater number of its relations, however, osmium presents itself as a member of the *arsenic group* of elements. This has been noticed by some recent authors, as by Prof. Dana in the arrangement of the elements adopted in his System of Mineralogy, and by Prof. Miller, who says in his lately published Elements of Chemistry that "it presents more analogy with arsenic and antimony than with the noble metals." Frémy too compares osmium in platinum ore to arsenic in the native arseniurets.

Nitrogen, phosphorus, arsenic, antimony, and bismuth are generally recognized as forming a distinct and natural group of elements, and into this group it seems from many considerations that osmium, and probably ruthenium, ought to be introduced. They have some analogies with other natural families, just as arsenic is allied to sulphur in native sulph-arseniurets, and nitrogen and chlorine exhibit some resemblance in the nitrates and chlorates, but here appear to lie their closest relations. It may be interesting to notice some of the principal points of resemblance to or difference from this group.

Iridosmine occurs in crystals closely related in form to those of arsenic, antimony, and bismuth in the metallic state. The analyses of iridosmine are not yet sufficiently numerous or accurate to enable us to decide upon its normal composition, but it seems probable that the two metals occur in variable proportions, and are in this mineral isomorphous, thus establishing, as noticed by Dana, a connection between the arsenic group and that of the distinctly basic metals, as the arsenic and sulphur groups are united through homœomorphous bismuth, tetradymite, and tellurium. Dana places iridium in the same section with iron, among the metals whose most stable grades of oxydation are the prot-oxyd and sesqui-oxyd, but the statement of Claus that the *bin-oxyd* of iridium is the most stable and easily prepared compound with oxygen would remove this metal, as also perhaps platinum and palladium, from the iron section to that containing tin and titanium, and the propriety of this transfer may be supported by the relationship of Frémy's crystallized oxyd of ruthenium (doubtless the bin-oxyd) examined

by Sénarmont: This was found to be homœomorphous with stannic and (the rutile form of) titanic acid. The bi-chlorid of tin and potassium too is reported as crystallizing in regular octahedrons, like the corresponding salts of iridium, platinum, and palladium.

The arsenic section, as given by Dana, includes nitrogen, phosphorus, arsenic, antimony, bismuth, osmium, and tellurium. The last-named is marked as doubtful, and should decidedly be placed with sulphur and selenium, to which it is analogous in by far the greater number of its compounds.

In one of the interesting memoirs lately published by Dumas on the numerical relations subsisting among the atomic weights of the elements, the arsenic series is thus given:

	Atomic weights.
Nitrogen,	14
Phosphorus,	14+17=31
Arsenic,	14+17+44=75
Antimony,	14+17+88=119
Bismuth,	14+17+176=207

and the parallelism of this series with that of chlorine, iodine, &c., is supposed to be shown in the following lines:

F (19)	Cl (35·5)	Br (80)	I (127)
N (14)	P (31)	As (75)	Sb (122)

in which a common difference of 5 is assumed between the two members in each of the vertical columns (a difference not strictly brought out in the case of phosphorus and chlorine), and in which antimony is given a higher atomic weight than in the preceding table. Osmium is not included, but in a supplemental note since published we find it placed, with an equivalent somewhat higher than that usually adopted, in the *sulphur* group, serving to complete the following two lines of equivalents:

Mg (12·25)	Ca (20)	Sr (43·75)	Ba (68·5)	Pb (103·5)
O (8)	S (16)	Se (39·75)	Te (64·5)	Os (99·5)

between the paired members of which a common difference of 4 is supposed to exist.

Let osmium and ruthenium be brought into the arsenic group, and the series of atomic weights will then stand thus:

	Atomic weights.
Nitrogen,	14
Phosphorus,	14+17=31
Ruthenium,	14+17+22=53
Arsenic,	14+17+44=75
Osmium,	14+17+66=97
Antimony,	14+17+88=119
Bismuth,	14+17+176=207

The atomic weights of ruthenium and osmium are here assumed as 53 and 97, numbers not differing more widely from those commonly received—52·2 (Claus) and 99·6 (Berzelius)—than do several of those assumed by Dumas. Our knowledge of these two equivalents is based upon very limited data, and can but be looked on as approximative merely. As regards osmium, Frémy says that in several experiments he has obtained an equivalent number lower than that given by Berzelius, and the vapor-density of osmic acid, which we shall notice presently, points to an equivalent close to 97. A re-determination of this equivalent is very much to be desired.

Taking the series as given above, we find ruthenium and osmium to fall in between phorphorus and arsenic—arsenic and antimony; the numbers from phosphorus to antimony increasing by 22—44—66—88, just as in the following group given by Dumas:

	At. wts.
Chromium, - - -	26
Molybdenum, - - -	26+22=48
Vanadium, - - -	26+44=70
Tungsten, - - -	26+66=92

and we may arrange the two series in parallel lines,

P (31)	Ru (53)	As (75)	Os (97)
Cr (26)	Mo (48)	V (70)	W (92)

These numerical relations are of very little importance in themselves, when we employ the small numbers of the hydrogen scale of equivalents, and especially when we permit ourselves to alter the numbers themselves to any extent, however small, but they acquire more interest when they present us with groupings of elements which we acknowledge on other grounds to be naturally related. In such cases, when the homology is distinctly marked, we may even be justified in taking some liberties for the moment with the numbers standing, often with but slender evidence to support them, for the equivalents of the less-known elements; and we may, perhaps, thus be directed to errors of determination which future experiments will clear away.

The bodies named in each of the two lines just given are homologous in many respects besides that of atomic weight, and a connection between the two series, through vanadium, has lately been shown by Schafarik. There is a clear resemblance running through the formulæ and properties of their oxyds. In the chromium series—a very natural one—the most important oxyds are the metallic acids of the composition MO_3; we have also in each case a bin-oxyd, MO_2; but the sesqui-oxyd is prominent only in the case of chromium itself, and indicates the relation of this metal with iron.

In the arsenic series the known oxyds are the following:

	P_2O					
NO	PO	RuO	AsO(?)	OsO	SbO(?)	BiO(?)
		Ru_2O_3(?)		Os_2O_3(?)		
NO_2		RuO_2		OsO_2		
NO_3	PO_3	RuO_3	AsO_3	OsO_3	SbO_3	BiO_3
NO_4	PO_4(?)			OsO_4	SbO_4	BiO_4
NO_5	PO_5		AsO_5	OsO_5	SbO_5	BiO_5(?)

The prominent compounds in the table are the acids, MO_3 and MO_5; with respect to the separate columns the following facts are noticeable.

The oxyds of nitrogen are well known; the regularity observable in this column causes it to be frequently used as an illustration of the "law of multiples." NO and NO_2 are usually said to be neutral, but the latter plays the part of a base in contact with sulphuric acid, as in the crystals of the oil of vitriol chambers, and possibly the former may do so too in the nitro-sulphates (KO, NO, SO_3 and NH_4O, NO, SO_3?) obtained by Davy by bringing nitric oxyd in contact with an alkaline sulphite. NO_3 and NO_5 are well-known acids. It is doubtful whether hypo-nitric acid (NO_4) is capable of combining with bases and forming salts; in contact with the alkalies it yields a mixture of nitrites and nitrates, yet, *when out of contact of bases, it seems to be a body of more stability than either* NO_3 *or* NO_5 (*anhydrous*).

In the column of the oxyds of phosphorus, we have first the very anomalous sub-oxyd (P_2O), which is probably the only marked exception to the homology running through the whole table. Before the discovery of red (amorphous) phosphorus by Schrötter this substance was, no doubt, to some extent confounded with phosphoric oxyd, and may even now throw some doubt upon the cases in which the latter seems to have been obtained pure and to have yielded a formula supported by trustworthy analyses. PO, unlike the other protoxyds of the series, is usually considered an acid, but as it has not been obtained in the separate state, and all the hypophosphites contain water, it may be reasonably assumed that the formula of the acid should include hydrogen. PO_4 is doubtful; this may, perhaps, be the composition of Pelletier's phosphorous acid, produced by the slow combustion of phosphorus, a body which undergoes no further oxydation by prolonged exposure to the air, and which, in contact with bases, yields mixed phosphites and phosphates. The last term in the column—phosphoric acid—is well known.

The existence of a distinct protoxyd of arsenic, as of antimony and bismuth, is doubtful. Arsenious acid is *a feeble, volatile, me-*

tallic acid—feebler in its relations as an acid than arsenic acid, and volatilizing at a lower temperature than the latter. Arsenious acid, moreover, volatilizes at a temperature below that required by metallic arsenic.

In the antimony column, the oxyd SbO_3 is usually viewed as a weak base, but seems also to be capable of uniting as a feeble acid to the alkalies, and even of expelling carbonic acid from their carbonates (Liebig). The iso-dimorphism of SbO_3 and AsO_3 is well established. SbO_3 is volatile at quite a moderate temperature, while metallic antimony requires at least a white heat to vaporize it. SbO_5 is a body of distinctly acid properties. *Both* SbO_3 *and* SbO_5 *are converted by heating in the air into* SbO_4 —the so-called antimonious acid—which seems therefore to be the most stable oxyd when strong bases and acids are not present. It is most probable that, as Frémy maintains, SbO_4 is not itself an acid, but that a so-called alkaline antimonite is in fact a mere mixture of an antimoniate with the compound of antimonic oxyd and alkali ($2SbO_4 = SbO_3 + SbO_5$).

In the bismuth column, the teroxyd is homologous as a base with teroxyd of antimony, but shows little tendency to play the part of an acid with even the strongest bases. This oxyd and the metal itself are volatile at high temperatures. BiO_4 also seems to be devoid of acid properties, but the compound BiO_5 probably exists, and is homologous with SbO_5, forming alkaline salts of little stability.

Comparing now ruthenium and osmium with the above recognized members of the arsenic group, we find first that both metals form protoxyds, which are feeble bases, as are probably the corresponding compounds of the other members of the group. We next meet with the sesquioxyds, whose formula is exceptional in the series, but for neither metal has this grade of oxydation been obtained in the free state and pure, and in the case of osmium its existence may be gravely doubted. Anhydrous Ru_2O_3 is supposed by Claus to be formed during the roasting of metallic ruthenium in the air at a high temperature, but only on the ground that the absorption of oxygen slackens when *about* enough has been taken up to form this compound, and that the proportion necessary for the binoxyd is never fully attained. Claus, however, describes a sesquichlorid with which double salts are formed by the chlorids of potassium and ammonium, and we must therefore assume a sesquioxyd also. Sesquioxyd of osmium is quite unknown in the separate state, and the belief in its existence is founded solely upon the preparation by Berzelius of a dark brown substance, supposed to consist of the sesquioxyd united to ammonia, which, dissolved in hydrochloric acid, yields a brown compound, supposed to be the sesquichlorid of osmium and ammonium. Neither of these, however,

can be crystallized, nor has the constitution assigned to either been supported by an analysis. The so-called ammonio-sesquioxyd detonates when heated, (sometimes with much violence, as I have noticed in removing by heat the deposit of this substance which forms on the end of a retort-neck during the distillation of osmic acid into a receiver containing ammonia,) and hence is probably analogous to fulminating platinum, containing perhaps the binoxyd of osmium. The binoxyd itself is a feeble base, the characteristic color of whose salts in solution is yellow as is the case with the corresponding compounds of iridium. Similar remarks apply to the binoxyd of ruthenium—probably the body obtained, as we have shown, by Frémy in crystals. The teroxyd of osmium is the body supposed to have been isolated in the experiment described at the beginning of this paper. Its position as a feeble acid, capable, however, under some circumstances, of playing the part of a base, its fusibility and volatility (greater, apparently, than those of osmic acid, as nitrous acid is more fusible and volatile than hypo-nitric), its probable crystallization in octahedrons of the regular system, in which arsenious acid and teroxyd of antimony are also found, all tend to indicate homology with the other teroxyds of the arsenic group. The general relations of ruthenic acid, so far as these are known, place it in a similar position. Just as we find hyponitric acid (NO_4) and antimonious acid (SbO_4) to be the most stable of the higher oxyds of nitrogen and antimony, so the well known osmic acid (OsO_4) seems to be the grade of oxydation which osmium most readily assumes and retains when not in contact with bases. OsO_3 and OsO_5 (the latter as described by Frémy) seem scarcely capable of existing in the separate state; when set free from their salts they soon pass into OsO_4; while it may as well be doubted that the latter ever exists as a distinct acid in combination with bases as that NO_4 or SbO does so. No so-called osmiate has ever been analyzed; the saturating capacity of the acid, if it be such, is unknown; when free and in solution in water it has no acid reaction, it does not displace carbonic acid from the carbonates, and it is itself expelled by heat from most of its supposed compounds, and is separated in part by water even from potash and soda. No compound of OsO_4 with a base has been obtained in crystals, while Frémy states that he has crystallized the alkaline salts of both OsO_3 and OsO_5. RuO_4 and RuO_5 are as yet unknown.

The tendency throughout the whole arsenic group is manifestly to the production of the acid compounds MO_3 and MO_5, the former the more fusible and volatile body, the latter the stronger acid. In addition we have some cases of the protoxyd (MO), a feeble base, and the binoxyd (MO_2), a body of still more feebly basic properties, verging upon the acids. All other grades of

oxydation, so far as they exist at all, may perhaps be correctly viewed as compounds of the preceding *inter se.* The stability of the oxyd (MO_4) in the separate state is remarkable—its formula is one of rare occurrence.

The affinity of all the elements of the group for oxygen is considerable; it is so even in the case of osmium and ruthenium, usually placed among the noble metals. Dumas (*Traité de Chim. app.*) states that osmium does not oxydize at common temperatures, nor even at 100° C., but I have obtained conclusive evidence that oxydation may go on slowly even at the ordinary atmospheric temperature. The paper label and the cork of a tube containing pure metallic osmium have in the course of several years become blackened, precisely as organic matter is by the fumes of osmic acid, the black tint on the paper decreasing from the mouth of the tube along the outside. A piece of white paper in which some black platinum residue had been wrapped, was strongly stained in the immediate neighborhood of the powder in the course of a few weeks. *The same effect is distinctly observable even upon the paper label placed inside a tube of native iridosmine* (Siberian) *in the usual coarse grains*—a specimen which has lain among other minerals, and has never been placed near any artificial preparations of osmium. Osmium, like arsenic and antimony, is clearly capable of slowly taking up oxygen at common temperatures. At a red heat, roasting in a current of air affords, as is known, a good method of obtaining osmic acid from the iridosmine of platinum residues—just as by similar roasting arsenious acid is prepared from the native arseniurets.

It would be a matter of much interest to compare osmium with its supposed homologues under circumstances in which we should expect it to play an electro-negative part. Frémy has announced his belief in the existence of an *osmiuretted hydrogen*, but such a body has not yet been isolated and described. Compounds of the metal with ethyl, methyl, &c., would be well worth examination, and it is not unlikely that such might be prepared from a body which in some states of combination exhibits such a high degree of volatility.

The earlier experiments of Deville and Debray upon the platinum metals seemed to have shown that both osmium and ruthenium could be volatilized, at exceedingly high temperatures, without previous fusion; if this were confirmed, a strong point of resemblance with arsenic would be made out, but it appears from a more recent paper that osmium at least may be fused and obtained as a perfectly compact mass, the apparent volatility of the metal being due doubtless to previous oxydation, the crucibles used being permeable to air. We have seen, as regards arsenic and antimony, that their oxyds are more volatile than the metals themselves.

It is lately stated that osmium may be obtained in crystals by the same means as those used for boron and silicon, but I have as yet seen no account of the form which it assumes.

Deville has furnished another interesting fact with respect to osmium, by determining the density of the vapor of osmic acid, which he has found = 8·88. This, if we take the generally received atomic weight for osmium, gives the atomic volume $\frac{131·6}{8·88}$=14·82, indicating a condensation to 2 vols. If we now calculate back to the theoretical atomic weight we get (14·57× 8·88)−32=97·38, a number closely approaching 97, which, as we have seen, brings the equivalent of osmium into simple and harmonious relation with those of the other elements of the arsenic group.

The specific gravity of fused metallic osmium having been lately determined by Deville =21·4, there can be little doubt that all the metals of the platinum family possess the same atomic volume when in the free state, about 4·6 or 4·7; the specific gravity of ruthenium is not yet known with accuracy, but such experiments as have been made render it improbable that it will prove an exception. This number is about one-fourth the mean of the at. vols. of the long recognized members of the arsenic group, but these latter differ so widely among themselves* that the comparison is of little or no value. It would be desirable to get a good determination of the density of osmic acid in the solid state, so that its at. vol. might be calculated and compared with that of antimonious acid.

The specific heat of osmium, so far as its value as a physical character goes, *opposes* the introduction of this element into the arsenic group. It has been determined by Regnault =·03063; multiplying now by the equivalent 97, we have the product, 2·9711, thus placing osmium in the list of the elements (including the majority) for which the product of sp. ht. by at. wt. is nearly 3, while for phosphorus, arsenic, antimony and bismuth the product thus obtained is twice as great, or about ·6. In this respect, however, osmium probably resembles nitrogen—the latter examined, as it necessarily is, in the gaseous form.

It is to be hoped that the conducting power for heat and electricity of compact osmium will soon be examined; nothing is as yet known of these characters.

* Phosphorus,	$\frac{31}{1·83}$ (*Schrötter*)	=16·94
Arsenic,	$\frac{75}{5·67}$ (*Herapath*)	=13·23
Antimony,	$\frac{119}{6·7}$ (*Karsten*)	=17·76
Bismuth,	$\frac{207}{9·8}$ (*Marchand & Scheerer*)	=21·12

Lastly, as regards the magnetic relations of the element—it is placed, with some doubt, by Faraday in the paramagnetic class; the metal and its protoxyd were found to act feebly in this sense, while pure osmic acid is said to have shown itself clearly *diamagnetic.* The strongly diamagnetic character of phosphorus, antimony and bismuth would render a re-examination of this point interesting. Arsenic, however, is said to be very feebly diamagnetic, and is placed by Faraday close to osmium in the list of metals examined, though on the opposite side of the line of magnetic neutrality or indifference.

Reviewing, now, the united physical and chemical characters of osmium, and comparing them with those of the generally recognized members of the "arsenic group," we are, I think, justified in concluding that here this curious metal should be placed in a natural arrangement of the elements—while important distinctions seem to separate it from some, at least, of the platinum metals, with which it is usually associated and described.

Tuscaloosa, Ala., Nov. 1, 1859.

ART. VII.—*The Comas and Tails of Comets;* by Prof. W. H. C. BARTLETT, U. S. Military Academy at West Point.

COMETS have, at all times, been objects of curiosity and wonder; and the question in regard to the nature of their luminous appendages, has exercised the speculative ingenuity of philosophers from the earliest records of astronomy. Everything written about them is read with interest, and the most extravagant theories in respect to their constitution and the laws of their being find a ready favor with the public. They are still among the enigmas of the heavens. Among the recent and remarkable efforts at solution, is one by the ablest mathematician of the country, perhaps of the age: and granting the premises, there is no avoiding the conclusions of the comprehensive and searching analysis for which this eminent man is so remarkable. But the assumption, that the attractive energy which summons a comet from the depths of space to the presence of the sun, retains its nature unchanged and strengthens with the diminution of distance for a part of the approaching mass, and yet reverses its character and becomes repulsive for another part, in order to obtain material to build up the tail, appears so unsupported by the analogies of nature as to give to his results the taint of improbability. Indeed, a theory which demands such an exercise of faith in matters of science, and from such friends, can only inspire doubt, and should yield the place it has too long occupied

to some other, founded in better ascertained laws of matter. The question is not one of pure mathematics, but of physics.

The material elements of all bodies of which we have any knowledge, are united by some conditions of aggregation, determined by the reciprocal action of molecular forces; and the circumstances of their relative motion will come from the equation

$$\Sigma m.\left[\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right]=0;$$

in which m is the mass of an element, xyz its coördinates of place, X Y Z the sums of the components of impressed accelerations in the direction of the axes $x\,y\,z$, respectively.

The conditions of aggregation may be expressed in some functions of the coördinates of molecular places. As three coördinates determine the place of a single molecule, there will be three times as many coördinates as molecules; and if μ be the number of molecules and λ the number of equations that give the conditions of aggregation, then will $3\mu-\lambda=n$ be the number of coördinates which, if given, would reduce the number of unknown coördinates to the number of equations. These unknown coördinates could then be found, and the places of the molecules at the corresponding instant determined.

Denote the λ coördinates by $x\,y\,z$, $x'\,y'\,z'$, &c., and the n coördinates by $\alpha\,\beta\,\gamma$, $\alpha'\,\beta'\,\gamma'$, &c. The former of these coördinates, as also the forces, may be expressed in functions of the latter, and both eliminated from the general equation of motion. And if $\xi\,\eta\,\zeta$, $\xi'\,\eta'\,\zeta'$, &c., be the increments of $\alpha\,\beta\,\gamma$, $\alpha'\,\beta'\,\gamma'$, &c., at any instant and due to any transmitted initial disturbance, it is easily shown that

$$\begin{aligned}
\xi &= \Sigma\,\mathrm{R}.\mathrm{N}_{\xi}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\eta &= \Sigma\,\mathrm{R}.\mathrm{N}_{\eta}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\zeta &= \Sigma\,\mathrm{R}.\mathrm{N}_{\zeta}.\sin\left(t.\sqrt{\varrho}-r\right),\\
\xi' &= \text{\&c.}\qquad\text{\&c.}\qquad\text{\&c.}
\end{aligned}$$

In which there are n terms comprehended by the sign Σ, and in which ϱ will, in general, have different values from one term to another. When these values of ϱ are real and positive, the different terms in the values of $\xi\,\eta\,\zeta$, &c., will disappear periodically, the precise times of disappearance being given by

$$t.\sqrt{\varrho}-r=a\pi;\quad t'\sqrt{\varrho'}-r'=a'\pi;\quad\text{\&c., \&c.}$$

or

$$t=\frac{a\pi+r}{\sqrt{\varrho}};\quad t'=\frac{a\pi+r'}{\sqrt{\varrho'}};\quad\text{\&c., \&c.}$$

in which a is any whole number. The intervals of disappearance will be

$$\frac{\pi+r}{\sqrt{\varrho}};\quad\frac{\pi+r'}{\sqrt{\varrho'}};\quad\text{\&c.}\quad\text{\&c.};$$

and if these intervals be commensurable, all the terms will dis-

appear simultaneously, and $\xi\,\eta\,\zeta$, &c., reduce to zero, at equal intervals of which the duration is

$$t_i = \frac{\pi + r}{\sqrt{\varrho}} \times \frac{\pi + r'}{\sqrt{\varrho'}} \times \&c. \times \&c.$$

The vast atmosphere of ether which pervades all space is ever busy transmitting luminiferous waves from the sun and other heavenly bodies. Its molecules are ever on the move with velocities and in orbits determined by the relative places and intensities of existing wave sources. Any cause which will perturbate the etherial molecules of any limited volume of ether from these orbits, regarded as initial, and by the quantities $\xi\,\eta\,\zeta$, $\xi'\,\eta'\,\zeta'$, &c., will make such volume *self-luminous;* and if the perturbation be great enough, it will be visible from all directions.

Comets are known to exist in a state of great tenuity, their densities being almost insignificant in comparison with that of the fleecy clouds that float in the upper atmosphere. The luminiferous waves from the sun, entering such bodies with great ease, their intromitted greatly preponderate over their reflected components. The former of these components modify and determine the internal motions of comets, and make them *self-luminous.* The internal cometary elements become so many centres of disturbance. They throw their waves in all directions, and are simultaneously sources of molecular perturbations to the surrounding ether, each giving rise to a term $\mathrm{R.N.}Sm\,(t.\sqrt{\varrho} - \gamma)$, in the general value of the perturbating functions $\xi\,\eta\,\zeta$, &c., and thus making the ether also *self-luminous.* The degree of illumination will vary with the maximum values of the perturbating functions. These will result, in any case, from the extent of the initial disturbance and the distance, at right angles thereto, over which the disturbance may have been propagated; decreasing, according to the principle of wave divergence, as the square of this distance increases. The components of the initial disturbance perpendicular to the line drawn from the comet to the sun, is, from the principle of wave propagation, much greater than in any other direction; and hence the much greater extent of the illumination on the side of the comet opposite the sun. The comet's head can have no phases, from its self-luminosity; neither can the coma and tail have sharply defined outlines, from the gradual degradation of molecular perturbations towards their borders.

This view denies the presence of cometary material in the coma and tail altogether, and regards these appendages but as phenomena due to the reciprocal action of the etherial and cometary molecular forces. According to it, the coma and tail become, as it were, *a luminous shadow,* a part of which is literally "cast before," and the dark cap which envelopes the head and

stretches away through the tail, a region of *wave interference.* No wonder, then, that comets turn their tails from the sun, and, at perihelion, whisk them, though of enormous lengths, through celestial arcs well nigh equal to a semi-circumference, in a few hours. This is no more surprising than that opaque bodies throw their shadows from the luminous sources whose light they intercept. The curvature, which is so remarkable a feature in the tail, is but the simple effect of the comet's orbital velocity, and the progressive motion of light.

If the principles here cited be well founded, then will the zodiacal light find an easy solution; and the great oblateness of its spheroidal figure must be taken as evidence that the component molecular motions in the sun are greater in the direction of the solar axis than in any other.

West Point, Oct. 25th, 1859.

ART. VIII.—*On Sodalite and Elæolite from Salem, Massachusetts;* by J. P. KIMBALL, Ph.D.

FOR a knowledge of this locality of the occurrence of the two rare silicates, sodalite and elæolite, we are greatly indebted to Gilbert L. Streeter, Esq., of Salem, as well as to several other gentlemen of the same city. Fortunately, Mr. Streeter very carefully observed their mode of occurrence, and, together with G. F. Cheever, Esq., and Rev. S. Johnson, Jr., collected choice specimens of them. The best of these are in the possession of the Essex Institute, Salem, to the curator of which, Dr. Henry Wheatland, I owe in a great measure the privilege of examining them.

The locality in which the minerals were found is "a pit or quarry, a short distance below the Almshouse upon the road passing along the northern side of the Neck, towards Hospital Point."* They were first noticed in a "block of compact syenite resting upon the bank, the end of which presented a beautiful coloring of blue and greenish white, with specks of black. Upon examination these conspicuous minerals were seen to be in a vein, a portion of which was connected with the block of syenite."†

Mr. Streeter subsequently discovered what undoubtedly was the continuation of the same vein. This traversed an erratic block of the same rock, imbedded in the drift, of which the small block, just mentioned, was a fragment. The vein is described to have been about six feet in width, and to have dimin-

* G. L. Streeter: Essex Institute Proceedings, ii, 153. † Ib.

ished in thickness "wedge-like to a mere line at the termination." Although this vein-stone was identical in its character with that of the smaller block, its yield of fine specimens of sodalite and elæolite was less abundant than that of the latter. Unfortunately enough, the discovery of the minerals was not made in time to rescue this precious vein-stone from the hands of the quarrymen. Large masses of it including, it is believed, the best specimens, had been carted away and buried deep beneath a littoral road along Collins' Cove. The quarry was opened in the autumn of 1855. I visited it last spring when it was not being worked, and found amongst the debris of the quarry very good specimens of both minerals. At that time a portion of the boulder which contained the vein was still left. This I identified as a syenitic porphyry (Quarzfreier Porphyr of Senft). It is characterized by remarkably entire crystals of oligoclase of a greenish color, which make up the base of the rock. Thickly disseminated through the base are minute grains of hornblende and scales of mica.

Besides the sodalite and elæolite, the vein-stone is composed of orthoclase for the greater part; biotite in black tabular prisms; small crystals of zircon in octahedral prisms; fine stellate brownish-yellow flakes of xanthosiderite; and (probably) albite in small, irregular, reddish, granular masses.

At Litchfield, Me.—the only other known locality in America where sodalite and elæolite occur together—these minerals are further associated with cancrinite and, as at Salem, with zircon; but instead of occupying a vein as in the Salem instance, exist as accidental constituents of a granitic rock composed of quartz, feldspar and black mica,* thus constituting a miascite analogous to that of the Ilmen mountains.† The Litchfield rock, to be sure, is found only as erratic blocks; but the absence of cancrinite in the Salem boulder, and the dissimilarity between this and the Litchfield rock as to petrographic character, tend to preclude the possibility of the two having a common source.

Sodalite.—The sodalite from Salem has quite the same character as that from Litchfield and the Ilmen Mts., with specimens of which I have been able to compare it, excepting that the former in common with the elæolite, is contaminated with minute particles of what appears to be mica, thus rendering it very difficult to glean perfectly pure mineral for analysis. It is in crystalline, sub-translucent masses having an indistinct cleavage. Its lustre is greasy, and its color beautiful lavender blue.

Three separate determinations of its specific gravity were made with different portions of the mineral, giving the results as follows: 2·294, 2·303, 2·314.

* J. D. Whitney: Poggendorff's Annalen, lxx, 434.

† Senft: Classification und Beschreibung der Felsarten, 218.

Two portions of the mineral were used for the analysis. The one portion was treated with nitric acid, and the chlorine determined as chlorid of silver. The other portion was treated with diluted hydrochloric acid, whereby the silica, alumina, lime and soda were determined according to the customary methods.

Prof. J. D. Whitney,* in his analyses of the sodalite and its associated minerals from Litchfield, has so fully observed their chemical properties as to render superfluous here any remarks on the same subject.

Calculating all the sodium as soda, the following results are obtained:

Silica,	37·33
Alumina,	32·70
Oxyd of iron,	*trace.*
Soda,	24·31
Chlorine,	6·99
	101·33

But on the other hand, assigning to the percentage of chlorine enough of sodium to form chlorid of sodium in accordance with von Kobell's formula of this mineral ($\dot{N}a^3\bar{S}i+3\dot{A}l\bar{S}i+NaCl$), we have 18·17 for the percentage of soda in combination with silica.

Hence the analysis will stand thus:

Silica,	37·33	
Alumina,	32·70	
Soda,	18·17	
Sodium,	4·57	NaCl
Chlorine,	6·99	NaCl
	99·76	

These results are sufficiently in agreement with the established formula.

Elæolite.—The elæolite from Salem possesses all the constant physical properties of this variety of nepheline. Its color is dull green, its lustre greasy, and its fracture sub-conchoidal. It is sub-translucent, and in structure massive. Its specific gravity is 2·629. Its chemical composition I find to be as follows:

	I.	II.
Silica,	44·31	44·07
Alumina,	32·80	
Peroxyd of iron,	*trace*	
Lime,	·40	
Soda,	16·43	
Potash,	5·50	
Ignition,	1·47	
	100·91	

Schenectady, N. Y., September, 1859.

* Poggendorff's Annalen, lxx, 433.

ART. IX.—*Description of Nine new species of Crinoidea from the Subcarboniferous Rocks of Indiana and Kentucky;* by SIDNEY S. LYON and S. A. CASSEDAY.

PTEROTOCRINUS. *Lyon and Casseday.*

Asterocrinus, Lyon, Geol. Rep. Ky., vol. iii, p. 472.

SINCE our description of Asterocrinus was published in the 3d vol. of the Kentucky Report, we find the name to have been appropriated previously. We therefore propose Pterotocrinus as the name of our genus, which has now four well authenticated species.

Generic formula.

Basal pieces,	2	Mouth central,	1
Radial pieces, 1st series,	5	Column round, (?)	
" " 2nd series,	10	Arms ciliated and single,	20
" " 3rd series,	20	Wings or lobed pieces,	5 variously formed.
Anal piece,	1 or more.		

Pterotocrinus depressus, n. s.

Body, depressed, subconical, twice as wide as high; below the free arms it presents the form of a very shallow, flattened cup, the pieces composing it smooth and of equal thickness, in some cases nearly a plane, the margin curving suddenly upward at the junction of the arms; the vault rises from the arms at a very low angle, rapidly increasing towards the centre, where it is nearly perpendicular. *Column*—very small, formed near the body of circular pieces of unequal size and thickness. *Basal* pieces, two, similar in size and shape; not prominent. When joined, they together form an irregular pentagon, raised a little above the general surface of the cup; slightly indented at their junction with the column, which has one or more pieces buried in the pit in which it is attached.

Radial pieces of the first series, five; subquadrangular, similar in form and size, nearly twice as long as high. Radial pieces of the second series, ten, subquadrangular, differing slightly both in form and size, resting near the centre of the outer margin or the first radials, a little more than half covering them. The radials of the third series are twenty in number, differing slightly in size, subquadrangular, nearly twice as wide as high, ten of them resting upon the radials of the second series, the other ten resting partly on these and partly on the first radials. The radials of the third series support from four to five brachials of quadrangular form, four times as wide as high; from the summits of this last series the arms, which before had been horizontal, become quite erect, and are composed of a double row of pieces which join by angular sides in the center of the arms.

Anal piece, one; obtusely angular below, fitting into a depression of the basal pieces, rounding to a point above, rising above the first radials.

The vault is divided into five triangular spaces by the wings, five in number; each space is covered with seven pieces of a compressed hexagonal form, three rising from the arms from the first row; upon these rest two; in the angular space at the summit of these rests the sixth, similar in size to those below it; it is squarely truncated above, thus becoming pentangular; its upper margin supports a small quadrangular piece; there are some quite small pieces above this last, in our specimen, probably one row which may form the mouth; between each of the five fields of the vault is a long lanceolate piece as wide as the other pieces of the vault, and about three times as long as those in the fields; the wings are attached to these long pieces by an articulating joint, and extend beyond the arms which embrace the vault, diminishing in thickness from their attachment outward, and terminating in a thin knife-like edge, equal in width to twice the height of the vault, the upper and lower margins nearly parallel; obtusely rounded at their outer extremity.

The arms are single, twenty in number, lying in sets of four between the wings, which, being placed immediately above the centre of the first radials, divide them into pairs, two from each of the adjacent radials falling into the spaces between each pair of wings. The arms are provided with a row of cilia for each side, formed of short joints, placed immediately in contact one above the other, filling the length of the arms which rise above the wings, about one sixth of their length. The dermal coating has been lost from all the specimens we have seen. The sides of the wings are strongly marked by a muscular attachment, by which they were moved. (?)

By reference to the figures it will be evident that this differs from all heretofore described species; by the bipartite basal pieces it is related to *Dichocrinus*.

Dimensions of Specimen of Medium Size.

Height of base,	·20	inches.
" " vault,	·30	"
Diameter of arms,	·75	"
Length of "	·70	"
Greatest width across the wings,	1·69	"
Height from base to top of wings,	·85	"

Geological Position and Locality.—Several specimens have been found in the third intercalated limestone, of the Millstone grit, Grayson Springs, Grayson County. Same horizon, near Dr. Baker's furnace, Edmonson County, Ky.

Pterotocrinus pyramidalis, sp. nobis.

Body without the arms and winged appendages:—the vault is a pyramidal pentagon, nearly twice as high as the greatest diameter at its junction with the calyx. The calyx is vasiform, the rim of the vase slightly reflected downwards: four times as wide as high. Columnar pit deep, irregularly oval, the longest diameter transversely disposed with reference to the anal side. Columnar facet round, perforation not visible (in any specimen that has come under our observation).

Basal pieces, two; prominent, outer margins thick and rounded, joined by a straight line to each other, the posterior margin having a deep angular notch, while the anterior side is but slightly indented at the

junction of the pieces; both pieces are irregularly pentagonal, nearly of the same form; that on the right of the anal piece being a little the largest.

First radial pieces, five; broad, sides diverging from below upwards, three times as wide as high, the ends of the upper margins parallel to the lower; about one-half of the length of the upper margin circularly depressed for the reception of two of the second radials to each.

Second radial pieces, ten; small, obscurely quadrangular, resting in the circular depression at the summit of the first radials, each pair being separated one from the other by a very minute anomalous piece, which rests on the center of the first radials between them; it is nearly round, and rises under the suture which marks the separation of the second radials, and is about one fourth the height of those pieces.

Third radials, twenty; small, differing both in size and form; nearly as high as wide, divided into five groups, disposed on a curved line, the exterior pair of each group resting partly on the first radial and partly against the oblique outer margins of the second radials. The central pair of all the groups rest upon the second radials, against those on either side and against each other. The articulating surfaces to which the arms were attached are slightly concave; the upper side deeply indented by a perforation into the body, partly in the arms and partly in the pieces immediately above them.

Anal piece, one; large, as compared with other species of this genus, long, hexagonal, obtusely pointed below, resting in the deep angular notch in the basal pieces; diminishing gradularly upward, supported on either side by the first radials; acutely pointed above, reflected toward the body, and supporting on either side one of the third radials.

Our specimens are weathered to such an extent that all surface markings (if they ever existed) have been removed.

Summit.—The summit above the arms is divided into five fields, nearly of the same form and size, that above the anal side being a little the largest; they are covered by hexagonal or pentagonal pieces higher than wide. The largest field has four pieces in the first row, the other fields having only three each; the outside of each of the first series is articulated with a piece at the base of the wings, the second range and the superior margins of the third radials. Dividing the first range of pieces are five projecting angular pieces inserted between the groups of the first range, touching the third radials at one of its angular points, rising from them by a line slightly curved outward; they articulate with the first and second range of the five fields and with the wings, in the same manner as *P. depressus* (nobis). The second range consists of three long pentangular pieces, in the field above the anal piece, and two to each of the others. The third range consists of one lanceolate piece to each of the regular and two (?) to the irregular side. Between the fields of the summit is a broad articulating surface, about as wide as the pieces covering the fields, formed by the reflected margins of those pieces, and the supporting piece at the base of the wings. *Wings*—none have been found attached; great numbers are found loose, which we refer to this species. They are of various forms. The articulating surface applied to our specimen precisely fills and fits the articulating surface upon it; they are thick near the junction with the body, curved both above and below, gradually running to a point, five times as long as thick, broad on the upper surface,

gradually thinning downward; the outer end round and pointed; they are frequently found bifurcate near the end and double pointed: affixed to the specimen they radiate regularly and horizontally, the points being about as high as the summit of the specimen.

Mouth, central.

Arms, twenty; form unknown.

Column, unknown.

Dimensions.

Greatest diameter of calyx, - - -	·80 inch.
" " " basal pieces, - -	·49 "
" " " columnar facet, - -	·05 "
Height of calyx, - - - -	·18 "
Height of specimen, small part lost, - -	·75 "
Length of wing articulations, with body, -	·45 "
Width " " " " -	·12 "
Height of first radial pieces, - - -	·12 "
Width " " " - - -	·35 "

Geological Position and Localities.—Found only in beds near the top of the third limestone of the Millstone grit series of Edmondson, Grayson and Breckinridge counties, Kentucky.

Fragments very abundant, good specimens rare. Beds from one to two feet thick are found composed of a mass of the remains of this crinoid cemented together, forming a distinctive and characteristic bed of the 3rd limestone.

Pterotocrinus rugosus, nobis.

The condition of our specimen is such that a particular description cannot be made: the arrangement of the parts, however, is evidently quite similar to that of *P. depressus*. The basals, first, second and third radials, are present, together with parts of the wings and a portion of one of the arms. This species differs remarkably from *P. depressus*, in the greater thickness of the pieces, prominence of the base, the knobby protuberances upon it and upon the first radials, the depth of the columnar pit, as well as by its roughness and more robust appearance.

Geological Position and Locality.—A single crushed and imperfect specimen was found in the lowest siliceous mud bed,* at the Falls of Rough Creek, Breckinridge county, Ky. Fragments of this species are quite abundant.

The beds at Rough Creek, Grayson Springs, Grayson county, and Baker's Furnace, Edmondson county, are doubtless the equivalents of each other. In the western edge of Breckinridge county, they are separated by a thick sandstone, where the upper division of the limestone appears to be the equivalent of the beds above enumerated. The size and proportions of this species is about the same as *P. depressus*.

Zeacrinus ovalis, sp. nobis.

Body—When the arms are closed the body is ovoid, the length being equal to about twice the diameter; concave at the base.

Basal pieces, five; minute, forming a pentagon, slightly indented at the sutures marking the division of the pieces. When the column is present, the basals and about half of the subradials are concealed. *Sub-*

* Equivalent to limestone No. 3, Millstone grit.

radial pieces, five; four of which are of the same form and size; lanceolate, obtusely pointed at the superior extremity; the fifth is large, irregularly pentagonal; one of its sides supports two of the anal pieces. *First radials*, five; differing considerably in size; the anterior one being symmetrical and the largest, the other four are unsymmetrical, the inferior side toward the anal field being longer than the inferior anterior side; they diminish regularly from the anterior to the posterior side. The *second radials* in all but the anterior ray are pentagonal, twice as wide as long, on the upper beveled sides of each supporting two arms. The second radial of the anterior ray is twice as wide as high, supporting on its upper side a third radial similar in size and form to the second radial of the other four rays, and like them it also supports a pair of arms. The *arms* are ten in number. The antero- and postero-lateral rays are composed of three quadrangular and one pentagonal piece each; one of the antero-lateral rays has the same form and number of pieces as the other and the anterior rays are composed of two quadrangular and one pentagonal piece each, somewhat larger than the pieces of the other rays. The inner upper margin of the last piece in all the rays support each one division of the ray, which is composed of about forty-five quadrangular pieces each, gradually diminishing in size to the end of this division of the ray, which is thus terminated in a point. The outer margins each support a branch which is bifurcated on the fourth, fifth or seventh piece, similar in form and arrangement to the first branch of the rays, the pieces being nearly as high and about half as wide as those below. The inner beveled upper margin of each last piece supports a branch which is of the same form as the inner branch below, composed of quadrangular pieces, terminating at the same height as the other branches. The outer margin of the second bifurcation of the anterior ray has in like manner an undivided branch of the same form and length; all the other rays are again bifurcated on the fifth or sixth piece, each division bearing two branches similar in form to the upper part of the inner divisions below; the anterior ray in its last division consisting of six fingers, all the others having eight fingers each, 46 fingers in all. The *fingers* are ciliated their entire length with a row on their inner margins.

Anal pieces, eight; the two inferior pieces rest partly on the subradials and partly against the first radials on either side, one rising nearly as high as the upper margin of the first radial against which it rests; upon these two rest two of the second range, and on the left side one quite minute, making the upper margin of the field nearly horizontal; the last two support one subquadrangular piece (the largest in the anal field) above which are three of the same form rapidly diminishing in size, thus running the anal field to a point.

Columns delicate pentagonal (near the body), composed of alternately larger and smaller thin pieces.

Dimensions.

Total length,	1·25 inches.
Greatest diameter,	·62 "
Height of calyx,	·05 "
Greatest diameter of calyx,	·50 "

Length of finger,	first	bifurcation,	-	-		·94	inch.
"	"	second	"	-	-	·80	"
"	"	third	"	-	-	·45	"
Depth of columnar pit,			-	-	-	·05	"

Geographical Position and Locality.—Found in the third limestone of the Millstone grit beds of Grayson, Breckinridge, Edmondson and Todd counties, Kentucky.

It has not been found in any other geological horizon in the Millstone grit beds of Western Kentucky, and may be classed amongst the distinguishing fossils of this particular bed of limestone.

Remarks.—Our species, in the arrangement of the calyx, approaches *Z. Worthenii* (Hall), from which it differs in having three instead of four primary radials; also in the number of pieces in the first branches of the rays. It differs from *Z. magnoliæformis* (Troost) in the arrangement of the calyx, the first radials and subradials being much shorter in our species.

Cyathocrinus dekadactylus, nobis.

Calyx, vasiform, spreading rapidly from the base, comprising all the pieces up to the second radials. The pieces forming it are thick, tumid, with long strong arms. Vault unknown. Column obtusely pentangular, covering nearly one half of basals; articulating surfaces crenulated on their borders.

Basal pieces, five; pentangular, forming together a marked stellate figure; upper facets prolonged into nearly acute angles; under surface slightly excavated.

Sub radials, five; hexagonal or heptagonal, somewhat irregular in size; they alternate with the basals; very tumid and thick.

Radials, five; the first are large, various in their shapes, generally septangular, sometimes broader than high, in other instances as high as broad; they support each two other radials, the first of which are thin, parallelogramic pieces much rounded on their outer surfaces; the last radial pieces are pentagonal, rather acuminate, more than twice as wide as high, and support on their bevelled edges two arms each, making $5 \times 2 = 10$ arms in all.

Arms.—The arms are apparently without any subdivisions throughout their whole length. They come off immediately from the radials; the first two armpieces are anchylosed, the arms becoming free and separate on the second armpieces. They are composed of thick pieces, rounded on their backs, about equal in size, and regularly superimposed upon each other. They are about one half as large as the last radials, and have a shallow sulculus (ambulacral groove,) (?) on their inner surface.

Anal piece, one; trapezoidal in shape, the upper margin reflected, pentangular, the middle portion of the piece intumescent. In two well preserved specimens we cannot trace any pieces superposed upon the anal piece.

At the juncture of any three pieces composing the calyx, there is a depression, which attains its greatest depth along the suture lines of the pieces. These depressions are an easily distinguishable character of this

species. The surface markings and proboscis (if one existed) are not preserved in any examples before us.

The spaces between the radials supporting the arms are quite extended and present the form of a Doric arch.

Geological Position and Locality.—Sub-Carboniferous beds of Montgomery county, Indiana. Rather rare.

Cyathocrinus hexadactylus, sp. nob.

Body, compressed, crateriform, robust, with long arms; surface covered with minute granulæ, arranged apparently in rows parallel to the margins of the pieces. The column is comparatively small, composed of alternating thick and thin pieces; the articulating surfaces are crenated, the larger and thicker pieces project over the thinner ones, the projecting edges being distinctly rounded.

Basis—Composed of five sublanceolate pieces forming together an irregular pentagonal figure. The columnar pit is small, occupying about one-third of the basis, slightly depressed, with a large central opening.

Subradials—Three of them are nearly equal in size, hexagonal, generally higher than wide; the remaining two are larger than the others, not constant in their shape, being quadrangular, pentangular and hexangular in different individuals. They alternate with the basals, are very convex, from their centres project two obscure folds which proceed to the radial pieces, where they join similar folds, thus enclosing a triangular depression, which is considerably deepened at their junction with any two radial pieces. These plicatures are faintly marked, and can be seen only on specimens when in a good state of conservation.

Radials—The first are large, wider than high, fitting into the retreating angles of the subradial pieces, equal in size; near the middle of the upper surfaces of each, and at the juncture of the two adjoining folds from any two adjoining subradial pieces, come off the remaining radials, which are two in number, small, the last axillary. The second radials fit into a facet which is scooped out of the upper part of the face of the first radials, and are bordered by a small emarginate plicature, slightly raised and reflected outwards.

Anal pieces, two; the first is obscurely pentangular, the fifth facet being hardly perceptible; it is hemmed in entirely by the surrounding pieces, much smaller than they are; the second is irregularly shaped, about the size of the subradials, and bears upon its upper edge an arm.

We have never been able to procure a specimen which had a vault preserved, and can therefore give no description of it. We doubt the existence of a proboscis. The figure* is drawn the size of nature.

Arms.—Following the large first radials are three small, rounded pieces, two quadrangular, the third axillary, giving off two branches, which are long, tapering, composed of a single row of stout pieces; three branches give off at intervals on either side, long filamentous pinnulæ, which extend the full length of the arms. These are composed of small pieces resembling those forming the arms, and are similarly arranged. There are five pairs, rising from the regular rays. From the anal field rises a single, long, tapering arm, not bifurcated as the others are.

* To be given hereafter.

None of the family of Cyathocrinidæ have yet been described in which the anal field always bore an arm. We supposed at first that this sixth arm was only an anomalous condition of the individual, but having in our possession six or seven specimens, all showing this arrangement, its actuality is placed beyond cavil.

Hall has described (Iowa, vol. ii, p. 625, pl. 18, fig. 7, 8,) *C. spurius*, to which our species is most closely related; it differs in the sixth arm, which alone constitutes it a distinct species. See also *P. Meekianus*, Shumard (Geol. Rep. Missouri, p. 188, pl. A, fig. 7, a, b).

Geological Position and Locality.—Found in sub-carboniferous beds of Clear Creek, Hardin county, Ky., and Montgomery county, Indiana.

Actinocrinus Indianaensis, nobis.

Calyx, subglobular, ornamented with hieroglyphic sculpture. Proboscis long and thin, arms very long. articulations similar in size.

Basal pieces, three; large, extending beyond the supra columnar oint. The margin of the basis projected into a flange.

Radial pieces, 5×3; the first is large, heptagonal, its central portion marked by a double sigmoid carina; its upper facet is angularly notched, into which depression the second radial pieces fit; they are longer than wide, pentagonal, supporting the third radials, which are axillary. These last give off on either facet two radials of the second series, the last of them axillary, and support two rows of three brachials each, the last ones of which are large, tumid, and quite protuberant; here the free arms come off. This is the case except in the antero-lateral ray, where the radials of the second series support only one row of brachials on each beveled edge, thus giving $4\times4+2=18$ pairs of arms.

A prominent carina commencing on the first radial piece extends over the middle of all the pieces, distinctly marking the ramifications of each ray.

Interradials.—Three are interposed between each two rays, except of course, on the anal side, (a large hexagonal one followed by two smaller ones) their middle portions rise into mammiform protuberances.

Anal pieces, seven; hexagonal, of nearly the same size. First one, followed by a row of three, then two; above them a single one, which completes the pyramid. The ornature of these pieces is the same as that of the interradials. We have one specimen showing elaborate hieroglyphic markings; doubtless this was the character of the original ornature of the surface.

Vault—about equal in size to the calyx, composed of many small polygonal pieces, which are intumescent, sometimes prolonged into small sharp thorns. The whole is surmounted by a thin, long proboscis, composed of very minute pieces, and closed at its extremity, where it widens out into a fusiform shape.

Arms.—When in a normal condition there are eighteen pairs of arms, long, dividing into two branches almost immediately after leaving the brachials, each branch running out to the end without further bifurcations. They are formed of a double row of pieces, which at their juncture present a serrated appearance; towards their outer extension they become gradually smaller, until at their end they are quite attenuate and generally curved inward. They are deeply sulcate on their inner surfaces,

and bear on their lower outer edges long filamentous frimbriæ, which are set closely together, one to each piece forming the arm; these are composed of five or six joints, each having a direction outwards or downwards. *De Koninck and Le Hon* figure (Recherch. Crinoid. foss., Pl. IV, fig. 3,) a bundle of arms which so nearly resemble the arms (with head attached) of one of our specimens, that we cannot but regard them as perfectly identical. They refer them to *Actinocrinus stellaris*, to which species they certainly do not belong. A *stellaris* has 20 arms, (our species 18) arranged in 5 rays of 4 each, with comparatively large interstitial spaces, while the arms figured are thickly crowded together, coming off as they did in almost a closed circle, without any interstices of moment to divide them into separate bundles of arms.

Geological Position and Locality.—Sub-carboniferous beds of Montgomery county, Indiana. It is quite an abundant fossil, and generally found in a good state of preservation.

Actinocrinus Coreyi.

Body, globular; the vault about one third higher than the calyx, tumid; vault surmounted by a large knob.

Basal pieces, three; forming together an irregular hexagon, scooped out, producing a depression occupying about one-half of the superfices.

Radials.—The first are large, irregularly hexagonal, wider than high; their upper surfaces are hollowed, receiving the second radials, which have a corresponding convexity; they are thin but wide, flabelliform, and bearing the third radials, which are flattened pentagons, axillary, and support on each beveled edge three unsymmetrical brachial pieces, five or six times wider than high; thence the free arms.

Interradials, three; the first is very large, generally an elongated octagon; above this are two irregular, thin, high pieces, each having a small depression near the center of their lower surfaces, and interposed between the arms.

Anal pieces, seven; the first is large, having a depression corresponding to that described in the radials; in a row with the first radials, but a little larger than them, hexagonal, its vertical diameter is greatest on its three upper facets, bearing three pieces; they are smaller than the first anal piece; the one on the superior facet is irregularly hexagonal, the lateral ones are long, rather unsymmetrical, and adjoining the neighboring radials and brachials, which nearly enclose them; upon these are three considerably smaller polygonal pieces, which complete the calyx. All of the pieces described above are tumid, without any visible external markings.

This intumesence renders the sutures of the pieces very marked. The arms are ten in number, very strong and thick, coming off in five pairs; we cannot describe them farther, as they are not preserved, and this is the only example we have of this species.

Vault.—The vault is surmounted by a very prominent knob, from which are projected in the directions of the arms, five plicatures, formed of four or five (generally four) massive tumerous pieces; they are much larger than any of the other pieces of the vault, with the exception of the central knob. Between each two of the plicatures is an interradial field, containing from eight to ten polymorphous pieces; they are generally ar-

ranged in a pyramidal form; often the vertex being a piece considerably larger than any of the others; the basal pieces of the pyramid are in most cases thinner and longer than any of the others; these, as well as the remainder of the pieces of this species, are turgid and massive, and like those of the calyx, are destitute of ornament.

On the side of the vault, above the anal field, is a considerable ovoid intumescence, composed of nearly thirty small pieces, whose surfaces are quite plain and level in contrast to the other pieces of the vault; they are arranged in nearly parallel rows, as follows, commencing with the lowest: 1–3–4–5; on the fifth row (which nearly completes this field—there are but two more rows) supervenes an ovoid opening, about one line in length, without a proboscis. This is the only opening upon the vault.

We have named this elegant species after Mr. O. W. Corey, to whom we are much indebted for many favors.

Dimensions.

Diameter of base,	·32 inch.
Greatest diameter,	1·30 "
Height to arms,	·35 "
Total height of body,	1·10 "
Diameter of axillary articulation,	·30 "

Geological Position and Locality.—Rare in sub-carboniferous limestone near the top of the knob stone bed, Hardin and Allen counties, Ky., and same geological horizon, Washington county, Indiana.

GENUS ONYCHOCRINUS, nov. gen. *Lyon & Casseday.*

Generic Formula.

Basal pieces,	3
Subradial,	5
Radial,	5×5
Brachial,	4 or 6×5
Interradial,	20 or 25×4
Anal,	5 to 7
Interaxillary,	1 to 3×5
Arms,	5 pairs.

Generic description.—This genus, in the shape of some of the pieces and in its general form, resembles more closely *Forbesiocrinus* than any other; yet they are so widely different in other respects that it will require no great perception to distinguish between them. The column near the calyx is cylindrical, large, composed of very thin articulations similar in size to each other; perforation small.

The radials are large, and form, together with the brachials, a continuous line: the arms are quite robust, furnished with strong pinnulæ.

Interradial fields, triangular in general shape; an anomalous one on the anal side. Anal field—long and narrow: one to three small interaxillary pieces on each ray.

Basal pieces small, subradials large, pentangular, alternating with the radials.

Onychocrimus exculptus, n. s.

Calyx vasiform, spreading rapidly to the base of the free arms, together with the arms resembles much the talons of a bird, whence its generic name; surface ornamented with minute granules.

Basal pieces, three; their under surfaces concave, forming a saucer-shaped depression, which was wholly filled by the column; they are low, rather thick, upper surfaces prolonged into an obtuse angle; the column facet marked by small short striæ on the outer margin.

Subradials, five; large, four are pentagonal, two of which are larger than the remaining two; their upper articulating surfaces form quite sharp angles; the fifth is hexagonal, its superior surface parallel to the inferior, smaller than the remaining pieces.

Radials, generally five in each ray; the first row are very large, heptagonal, except in the postero-lateral rays, where they are hexagonal, having but one facet on the side next the first anal piece instead of two; their lower surfaces rest on the retreating angles formed by the subradials, with which they alternate; their superior margins are horizontal, on which are imposed the second row of radial pieces; these are smaller than the first, hexagonal, nearly twice as wide as high. The third and fourth rows are similar in form, but become gradually smaller; the fifth rows are heptagonal, axillary, and support on each beveled edge a row of two or three brachials, which are smaller than the radials, obscurely hexagonal, bounded by wave-like lines; near the center of their inferior borders they are prolonged into minute uvulæ, or little tongue-like projections, similar to those found in some species of *Forbesiocrinus*, (and described by Hall as patelloid pieces,) which fit into corresponding depressions in the adjoining pieces.

Upon the last brachial pieces rest two arms; they are as long as the body, robust, composed of exceedingly stout pieces, grooved by a deep sinus, decreasing gradually in size to their outer extension; they are similar in form to the brachials described above; from either side of these arms, alternately disposed, are stout, short pinnulæ, composed of thick pieces stretching in a direction outwards and upwards, without tentaculæ. Commencing with the first two pieces of the arms, they come off in two pairs, then on the next two pieces come off again two pairs, but on the alternate side from the first two; this arrangement is continued throughout their whole length, the pinnulæ becoming smaller and more closely crowded together toward the termination of the arms.

Interradials.—These vary from twenty to twenty-five, according to the age of the individual. The first is large, septagonal, situated between the first two radials of each row, followed by three pieces about half the size of the first; the middle piece of the three is an elongated pentagon, smaller than the other two, which are hexagonal, upon the outer upper facet of each of which, and lying against the radials, are one or two smaller pentagonal pieces; then follow (in the specimen we have before us) fifteen yet smaller pentagonal pieces; they are arranged in the form of a hemispherical arch, depressed towards the disc of the stomach, the sides of the arch being extended up along the radials and brachials as far as the commencement of the free arms.*

* See also under description of Vault.

Interaxillaries.—In a line with the radial, and between the two opposite arm pieces, is one to three quite small pentagonal interaxillary pieces; sometimes two yet smaller pieces occur.

Anal pieces, one; small, quadrangular; one is superimposed on the subradial, followed by three pieces of like form and size, and similarly disposed.

Vault.—The vault of this remarkable crinoid resembles so much some of the *asteriada* that we may consider it as one of the connecting links between the crinoidea and the star-fishes. The specimens in our possession do not show distinctly the whole of the vault, so that a description must necessarily be imperfect until better examples are found. The plates which we have described above as interradials, form the greatest portion of the perimeter of this upper surface; the remainder of the perimeter is formed of a row of pieces lying on the anal side; their relative position is as follows: viewing it from the anal side, in the position in which the animal grew, we see a row of small pieces (interradials?) which extend up along the radials and brachials of the left ray, fitting into the serrated depressions existing at the junctures of any two pieces. This is found only on the left ray, the pieces forming the right being squarely truncated and without other pieces attached in any way to them.

The general surface of the vault is depressed, the edges being raised and curved inwards. From the centre, and in the direction of the arms, extend five rays, composed of two rows of large granular pieces, one row alternating with the other. We cannot discover pores in any of these pieces, which are most probably analagous to the *ambulacra* of the *asteriada.*

The interstitial pieces lying in the fields bounded by the five rays of larger ones are very small, granulose, and arranged without any apparent order.

It is impossible, from the fragmentary portions in our cabinets, to trace the farther similarity between this genus and the star-fish, as the central portions of all are so concealed that we cannot make them out clearly.

Geological Position and Locality.—Found at Clear Creek, Hardin county, Ky., in sub-carboniferous rocks near the upper part of the sandy mud-beds of the knobstone. In beds of similar age in Montgomery county, Ind. Good specimens are rare.

Louisville, Ky., Nov. 1, 1859.

ART. X.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

I HAVE recently undertaken to bring the theory developed in a previous number of this Journal,* in an article entitled "Dynamical Condition of the Head of a Comet," to the test of numerical computations, by determining, by calculation, the theoretical dimensions of the great Comet of 1858. The more important results may be briefly stated; the complete discussion is reserved for a subsequent number of the Journal.

* Vol. xxvii, [2], 86.

It appears from the investigation that, confining our attention to the outer bright envelope, the process of ejection of nebulous matter from the nucleus was mostly confined to a certain portion of its illuminated side lying nearest to the sun, and that the limiting angle of inclination of the jets to the line connecting the centre of the nucleus with the sun, was 25°. From this circumstance it resulted that the envelope had nearly a circular form. The lateral dispersion of these luminous jets, as they were flowing away into space, under the influence of the sun's repulsion, or, in other words, the breadth of the tail, was partially due to the directions, more or less inclined to the radius vector, in which they originally issued from the nucleus; but another and highly efficient cause coöperated to produce that result. If, as we conceive, certain portions of the cometic matter, at the surface of the nucleus, were brought by some action of the sun into the condition to be repelled by both the nucleus and sun, we may make two suppositions with regard to the forces of repulsion thus developed; that the force exerted by either body was of the same intensity for all the particles acted upon, or that it varied from one particle to another. It is not easy to decide, upon *à priori* grounds, which of the two suppositions is the most probable. The latter is certainly no less so than the former. If we adopt this as a fundamental hypothesis, we have an efficient cause in operation adequate, in connection with that already mentioned, to the development of the tail of the comet, in all its vast proportions; and which may incidentally have produced the special phenomena observed,—as the supernumerary tails, and the alternate bright and dark bands seen to traverse a certain portion of the principal tail. The ejected particles that are unequally repelled, by both the nucleus and sun, do not part company in consequence, while they are in the vicinity of the nucleus, nor materially while within the limits of the envelope, for the reason that the ratio of the repulsive forces of the two bodies remains constantly the same; but as soon as they pass out of the sphere of influence of the nucleus they are analyzed by the solar repulsion, and driven off by it into space, in separate and diverging paths. The various susceptibilities to repulsion possessed by the particles have accordingly no sensible effect upon the dimensions or form of the envelope, but may give rise to a wide lateral dispersion of the flowing streams that make up the tail of the comet. The particles that are most energetically repelled go to make up the preceding or convex side of the tail.

I have made the calculations, regarding the solar repulsion as varying between certain prescribed limits. The determinations of the breadth of the tail, at various points of its length, accord with the results of observation; at the same time that the tail is found to have the form and positions actually observed.

The supernumerary tails observed were but lines of receding

particles subject to much greater forces of repulsion than the other particles ejected from the nucleus. All such collections of matter would, of necessity, be in advance of the principal tail, and lie in a curve that would approach more nearly to a straight line. Their position makes known the intensities of the repulsive forces to which they owe their separate existence.

An interesting result of the investigation is that the alternate bright and dark bands so distinctly seen to traverse a certain portion of the tail of Donati's comet, in nearly parallel directions, on the evening of Oct. 10th, had each the position of a line connecting particles which started from the region of the nucleus at a certain previous date, and at the same instant of time. They accordingly find their natural explanation in corresponding alternations in the quantity of nebulous matter given off simultaneously from the nucleus. The most probable cause for such alternations of discharge that can be conjectured is that the nucleus turns about an axis, and so presented periodically different sides to the sun, which were unequally influenced by his inciting action. If this be the true explanation of the phenomenon, we have in the observed distance between contiguous bright bands, the means of determining the period of rotation; or, at least, the shortest interval of time in which the rotation can be completed. If we take this distance at 1°, the period of rotation comes out about 24 hours.

There was a special cause of lateral dispersion at work in the case of the cometary particles that, on their return, came very near the nucleus. Such streams of particles must have been repelled off obliquely, and may very well have presented the appearance of luminous jets issuing from the sides of the nucleus, and have formed curved terminations to the inner envelope. From the dispersion thus produced resulted an absence of matter, or a dark space, behind the nucleus, whose varying boundary was determined by the intersections of lines of particles unequally repelled by the sun.

The indications are, that the formation and gradual expansion of one envelope after another, may have arisen from the process of ejection beginning in all instances high up in the photosphere surrounding the nucleus, and gradually extending downward to the vicinity of the solid surface. It appears, upon investigation, that if this descending action were to proceed according to a certain uniform law, the outline of the envelope would recede from the nucleus at a uniform rate. This process of evolution of cometary matter, in whatever it may consist, is probably auroral in its origin and character, and has its counterpart on both the earth and sun.*

New Haven, Nov. 30, 1859.

* To render the investigation more complete, I have considered the case of the cometary matter being projected from the nucleus, without experiencing any repulsion from its mass.

Art. XI.—*Geographical Notices.* No. X.

The Mountains of Western North America.—Having previously spoken of the publication of Warren's memoir to accompany the map of the western territory of the United States, and also of the map to which it refers, we now copy the following paragraphs from the conclusion of the Memoir, in order to meet the erroneous opinions which are prevalent in respect to the "Rocky Mountains," and the erroneous presentation of their direction which is given in most of the popular maps.

"The mountains in our territory west of the Mississippi river, from where they rise above the horizontal strata of recent geological formations on the east to their disappearance under the waters of the Pacific Ocean, form a nearly continuous mass of upheaved ridges, with occasional intervening level plateaus. The direction of the central line of this mass between the 32d and 49th parallels of north latitude, is about north 20° west. The greatest width perpendicular to this direction is along the line passing from the vicinity of San Francisco through that of the Great Salt Lake to Fort Laramie. This distance is about 1,000 miles, or, if we include the Black Hills of Nebraska, 1,125 miles.

"The great mountain mass, of which that in our territory forms but a part, extends with varying breadth nearly on the line of a great circle of the globe from Cape Horn north to Behring Straits, and thence south along the western part of Asia to the island of Sumatra. Its length is about 240 degrees, or 18,560 miles, being two-thirds of the circumference of the earth.

"The area occupied by and included in this mountain mass in our territory, is about 980,000 square miles. Large as this is, it is probably only a small portion of the upheaved formations between the 32d and 49th parallels. A few ridges and peaks projecting above the surface of the Pacific as islands, or above the level tertiary and cretaceous strata of the eastern plains, give evidence of the existence of vast areas whose extent must forever remain unknown. Throughout the portions now visible, proofs are abundant of great abrasions; in some cases whole ridges even, having been swept away or broken into separate portions.

"Already enough has been learned to establish the existence in these mountains of the equivalents of many of the geological formations; and it is probable, when investigations have been carried to the same extent as in the civilized portions of the earth, that the geologist will find here new and still more complex fields for research.

"The classification of the separate parts of this mountain mass, so as to present its physical characteristics clearly to the

mind, is a great desideratum. It has in part been attempted at various times, but as yet unsuccessfully from the want of sufficient information; the theorist's idea being often proved to be wrong by new discoveries almost as soon as uttered." * * *

"The publication of the Pacific Railroad maps will probably change some of the former ideas of these mountains, and give rise to new speculations as to their directions, equivalents, and connexions of different parts. Every one knows how easy it is to generalize ideas where facts are few, and in accordance with this, those who have travelled most in the region have theorized the least, having seen the immensity of the subject and the difficulties which must be overcome to comprehend it. Those who have investigated merely the travels of others, have had only the imperfect representations of the latter on which to theorize.

"It may not be inappropriate here to give some of the general ideas which have successively prevailed in regard to these mountains.

"In the earlier periods of North American discovery it was known that there were mountains in the interior at its northern and southern parts, and rivers flowing from them to the two great oceans east and west. It was natural to connect these mountains by hypothesis, and to consider them as one great chain, separating the sources of these streams. Such an idea prevailed at the time of Humboldt's New Spain. Even now many well informed persons consider that a road has but one mountain summit to cross from the Mississippi river to the Pacific Ocean.

"When, after the publication of the charts of Vancouver, map makers became aware of the extent of the mountains near the Pacific coast, nothing seemed more natural than to suppose two great mountain chains—one near the Pacific and one in the interior. If this theory were true, we should find a great longitudinal valley between the ranges similar to that separating the interior mountains from the Alleghanies, and we should have but two mountain summits to pass between the Mississippi and the Pacific. This idea is practically as erroneous as that of one summit, although it still prevails. Such a prominent place did this longitudinal valley hold, in the opinions of geographers of earlier times, that we find in Humboldt's New Spain: 'M. Malte Brun has started important doubts concerning the identity of the Tacouche Tesse and the river Columbia. He even presumes that the former discharges itself into the Gulf of California: a bold supposition, which would give the Tacouche Tesse a course of an enormous length. It must be allowed that all that part of the west of North America is still but very imperfectly known.'

"The explorations of Lewis and Clarke proved that the Tacouche Teche did not empty into the Gulf of California, and

that it was probably the source of the Columbia. Without considering the character of the pass of the Columbia river through the Cascade range, the belief now became general that the overland route in this latitude crossed but one summit, and was therefore more favorable than any other. This erroneous idea with some still prevails.

"The idea of rivers traversing great mountain chains, now known to be so common in the mountains west of the Mississippi, was so repugnant to the opinions of even philosophers in earlier times, that we find Humboldt saying, 'every geographer who carefully compares Mackenzie's map with Vancouver's will be *astonished* that the Columbia, in descending from the Stony mountains, which we cannot help considering as a prolongation of the Andes of Mexico, should traverse the chains of mountains which approach the shore of the great ocean, whose principal summits are Mount St. Helen and Mount Rainier.'" * *

"The distinguished explorers, Lewis and Clarke, having determined that the Columbia river broke through the Cascade range, considered from the size of the Willamette at its mouth, that it also broke through this chain, having its source in the Rocky mountains, near the position of the Great Salt lake. We then see the American maps representing mountains surrounding the valleys of the Columbia and Colorado, and separating them from that of the San Joaquin and Sacramento. On the English maps of that date, the Sierra Nevada is not represented, and two or three great rivers are made to flow from large lakes in the interior to the Pacific; nearly all of their compilers making false applications of the principles of hydrography laid down by Humboldt.

"The first map which represented these rivers and lakes correctly was that of Captain Bonneville, of which I have given a reduced copy. There we see the Great Salt lake and Bear river and Utah lake forming one basin; to the west lies the Mary or Ogden's river, with its lakes forming another enclosed basin; the San Joaquin and Sacramento rivers in their right position; and the Willamette reduced to its proper length. The positions given on this map are not geographically correct, nor are their many mountains indicated; but it gives the first correct idea of the hydrographic character of the country; and by giving too little rather than too much, escapes the errors into which others had fallen.

"The explorations of Captain Frémont fixed these great rivers and basins in their proper geographical positions; but his maps have given rise to many erroneous impressions in regard to the mountain ranges. Still, making a 'false application of the principles of hydrography,' he represented all the basins as if surrounded with mountains or 'rims,' and thus introduced mountain chains which have no existence in nature.

"Since Frémont's expedition began, a large portion of the area of these mountains in the territory of the United States has been examined, and many new attempts have been made to systematize the knowledge acquired. The most important theory advanced is that of parallelism in the ranges, the foundation of which I shall briefly indicate.

"On the map of Lewis and Clarke the Rocky mountain ranges are represented parallel to each other with a northwest trend. That this was their theory is evident, from the fact that they indicated the Black Hills about the source of the Shyenne as having this same trend, though they never saw them, and only knew of their existence from hearsay.

"The maps of Captain Frémont showed a parallelism and general north and south direction of the mountain ranges from the Wasatch, east of the Salt Lake, to the Sierra Nevada, including all the numerous intermediate ranges.

"The maps made by Major Emory, near the 32d parallel, and in New Mexico, showed again a remarkable parallelism of the mountain ridges, those in this latitude having a northwest trend nearly parallel to the Rocky mountains, as shown by Lewis and Clarke.

"The maps of Lieutenants Abert and Peck, of Lieutenant Simpson, of Lieutenant Beckwith, Lieutenant Williamson, and Lieutenant Parke, have all shown a local parallelism to exist in different parts of the mountains. The systems of ridges have courses varying from a few degrees north of east to north 45° west.

"The idea has lately begun to prevail that this local parallelism is the characteristic of the great mountain mass throughout its whole extent. Whether this idea has been true or not it has been attended with some practical advantages. Instead of one or two main summits for an overland road to pass, it shows us that we must expect many. On every route explored across the continent, at least four well-defined summits have been discovered, and on some of them many more. Some of these ridges enclose interior hydrographic basins. Others are traversed by rivers, but the passes thus made are generally impracticable, and, for the purposes of travel, might almost as well never have existed.

"In many places, however, the mountain ridges have not this local parallelism, of which a few instances will be cited. The Uintah mountains, east of the Great Salt Lake, trend nearly east and west; the Wind River mountains about north 45° west; and the Humboldt range about north 20° east; these three ranges being comparatively near to each other.

"Humboldt, in speaking of the Sierra Nevada, says, 'it soon separates into three branches.'

"Lieutenant Abbot, in the sixth volume of the Pacific Railroad Reports, says: 'Shasta Butte, although generally considered a peak of the western chain of the Sierra Nevada, is, in truth, the great centre from which radiate, beside several smaller ridges, the Cascade range, the Coast range, and the western chain of the Sierra Nevada.'

"There are many other portions of this mountain region from which the ridges seem to radiate. Such as Long's Peak, the junction of the Sierra Nevada, and Coast ranges in Southern California, &c., as is evident on an inspection of the map. The parallel system of ridges has been considered a matter of importance, as being in accordance with some supposed laws of mountain formation, but that of centres of upheaval are not less consistent with those laws. At any rate it does not appear that we are at liberty to assume a parallelism of ridges till examination has shown this to be the case.

THE SOURCES OF THE NILE.—At a recent meeting of the Royal Geographical Society of London (May 9), Captain Speke gave the following narrative of his journey with Capt. Burton into the interior of Africa. His remarks were called out by a paper of Mr. Macqueen's, the object of which had been to prove that the mountain Kilimandjaro is actually snow covered but that it has no connection with the River Nile.

Capt. Speke's remarks are thus reported in the Proceedings of the Society, vol. iii, No. 4:

"After arriving at Zanzibar, we had to wait a considerable time, some months, until the masika, or rainy season, would be over, before we could penetrate into the interior. It was generally advised that we should do so. During the interim Captain Burton and myself made a short coast tour, first to Mombas, and then proceeded down the coast to Pangani. Leaving that place, we ascended the Pangani river, and arrived at Chongwe, a small military station belonging to Prince Majid. Here we were supplied with a small escort of Belooch soldiers, who accompanied us across some hills, by an upper route, to Fuga, the capital of Usambara, where we were hospitably entertained by King Kimwere, a great despot reigning there. After visiting him for one day, the shortness of our supplies compelled us to retrace our steps by a forced and rapid march, following down close along the banks of the same river until we again arrived at Pangani. Thus ended our initiatory tour in Eastern Africa. The rainy season or masika was spent by us at Zanzibar, in constructing the equipment of a caravan. There is a singular tribe of negroes in the interior of Africa, called Wanyamuezi—the literal translation of which signifies people of the moon. These strange people are professionally voluntary porters: they annually bring down ivory to the coast in barter for themselves, or otherwise

for the Arabs. At the close of the rainy season Captain Burton and myself left Zanzibar, with a caravan mustering about eighty men, having previously sent on some supplies in anticipation of our arrival. Unable to collect a sufficient caravan for the conveyance of our kit, we purchased a number of donkeys (about thirty). Thus completed, and with an escort of twelve Belooch soldiers, given us by Prince Majid, we commenced our journey westward, and arrived (by slow degrees travelling over a low alluvial plain, up the course of the Kingani river) at Zungomero, a village situated under the coast range, which struck us as bearing a good comparison with the western ghauts of India. We might call this range the Eastern Ghauts of Africa. There we were detained by severe illness a considerable time. Afterwards we crossed these eastern ghauts, the maximum altitude of which I ascertained to be about 6000 feet. On the western side of this longitudinal chain of hills we alighted on an elevated plateau, an almost dead flat, ranging in level from 3000 to 4000 feet above the sea. Here we had cold easterly winds, continuing through the entire year. Proceeding onwards, we arrived at the Tanganyika Lake, called by the Arabs Sea Ujiji, a local name taken from the country on the eastern margin of the lake, whither they go to traffic for ivory and slaves. This lake is in a singular synclinal depression; I found its elevation to be only 1800 feet; whereas the surrounding country (the plateau), as I said before, averaged from 3000 to 4000 feet. The lake is encircled at its northern extremity by a half-moon shaped range of hills, the height of which I estimated (for I could not reach its summit) to be at least 6000 feet. They may extend to a height much greater than that; however, we could not take any observations for determining it. After exploring this lake we returned by the former route to Unyanyembe, an Arab depôt, situated in latitude 5° south, and about 33° east longitude. My companion, Captain Burton, unable to proceed farther, remained here; whilst I, taking just sufficient provisions for a period of six weeks, made a rapid march due north, to latitude 2° 30′ south; and there discovered the southern extremity of the Nyanza, a lake, called by the Arabs Ukerewe, a local name for an island on it, to which the merchants go in quest of ivory. The altitude of this lake is equal to the general plateau (4000 feet), even more than the average height of all the plateau land we traversed. In reverting to the question asked, why I consider the Lake Nyanza to be the great reservoir to the Nile, my answer is this: I find, by observation, that its southern extremity lies in east longitude 33°, and south latitude 2° 30′. By Arab information, in which I place implicit confidence, I have heard that the waters extend thence, in a northerly direction, certainly from five to six degrees. Notwithstanding they can account for

a continuous line of water to this extent, no one ever heard of any limit or boundary to the northern end of the lake. A respectable Sowahili merchant assured me that, when engaged in traffic some years previously to the northward of the line and the westward of this lake, he had heard it commonly reported that large vessels frequented the northern extremity of these waters, in which the officers engaged in navigating them used sextants and kept a log, precisely similar to what is found in vessels on the ocean. Query, Could this be in allusion to the expedition sent by Mahamad Ali up the Nile in former years? Concerning the rains which flood the Nile, the argument is simple, as I have said before: a group of mountains overhang the northern bed of the Tanganyika Lake. The Arabs assure us that from the north and northeastern slopes of these hills during the rainy season immense volumes of water pour down in a northeasterly direction, traversing a flat marshy land, intersected by some very large, and many (they say 180) smaller streams. Again, on the western side, we hear from Dr. Krapf, that the snow-clad mountain, Kœnia, is drained by rivers on its western slopes in a direction tending to my lake.

"During the rainy season, which I know, by inspection, commences in that region on the 15th of November, and ends on the 15th of May, the down-pour is pretty continuous. Supersaturation, I should imagine, takes place later on the northern than on the southern side of the aforesaid moon-shaped mountain, systematically in accordance to the ratio of seasonal progression; but this, in so mean a distance, could not be very great. Suffice it to say, that I saw the Malagarazi river, which emanates from near the axis of these hills, to be in a highly flooded state on the 5th of June. The Nile at Cairo regularly swells on the 18th of June.

"Farther, it would be highly erroneous to suppose that the Nile could have any great fluctuations from any other source than periodical rains. Were the Nile supplied by snow, as some theorists suppose, its perennial volume would ever be the same. There would be no material fluctuations observable in it, in consequence of its constant and near approximation to the path of the sun.

"By these discoveries, the old and erroneous hypothesis of a high latitudinal range of mountains extending across the continent of Africa from east to west, in the vicinity of the line, and known as the Mountains of the Moon, is therefore now annihilated. However, it is worthy of remark, that the crescent-shaped mountain, which we visited to the northward of the Tanganyika, lies in the centre of the continent of Africa, immediately due west of the snowy peaks Kilimanjaro and Kœnia, and is west beyond the Unyamuezi, or Country of the Moon.

The Wanyamuezi tribe has from time immemorial been addicted to journeying, and at all periods has constantly visited the eastern coast of Africa. It would not be beyond legitimate and logical supposition, to imagine that these hills, lying beyond their Moon Country, should have given rise to the term Mountains of the Moon, and from misunderstanding their relative position with the snowy Kœnia and Kilimanjaro, should have been the means of misguiding all ancient inquirers about that mysterious mountain.

"My positions were fixed by astronomical observations, certainly under painful and considerable difficulties, owing to my constantly impaired general state of health: weakness and blindness not being the least of these difficulties which I had to contend with. My latitudes were taken by the altitude of stars, at nearly every stage, on an average from ten to fifteen miles apart. I also fixed some crucial stations, the principal points for delineating the country by lunars, on which I place great reliance, as the means of the masses of them which I took show so little deviation. The intermediate distances I compassed very closely; the altitudes of the country I traversed I determined by boiling thermometer, on which I also place very great reliance. We had a thermometer and pedometer, and several chronometers. The performance of these instruments was anything but satisfactory; indeed, finally, I had to rig up a string and bullet pendulum to beat time whilst taking my lunars in the latter stage of the journey. There now can be no doubt that this great lake, the Nyanza (Captain Speke now pointing to the map) is the great reservoir of the Nile, and that its waters indubitably extend northwards from the position visited by me on its southern extremity to $3\frac{1}{2}°$ north latitude, lying across the equator, and washes out that supposed line of mountains, called the Mountains of the Moon, which stands so conspicuously in all our atlases."

BAIKIE'S NIGER EXPEDITION.—The latest intelligence we have received from this important expedition is contained in the following extract from Sir R. I. Murchison's late anniversary address before the Roy. Geog. Soc. of London:

"The unfortunate shipwreck of the *Pleiad* on the rocks near Rabba, and the check given to the expedition under Dr. W. B. Baikie, which left England early in 1857, were alluded to in my last year's address. I now learn from Mr. D. T. May, R.N., who has returned to England, that less than twenty miles above Rabba the River Niger, or Quorra, divides into several rocky, intricate channels. Consul Beecroft in the *Ethiope*, in 1845, safely navigated the most available of these passages; but the voyagers of 1857 were not so fortunate, and the steamer was totally lost on the rocks. Most of the property was, however,

saved, and the neighboring bank became the head-quarters of the expedition for a whole year. The rocks forming the banks of the river where the shipwreck took place are composed of highly-inclined strata of hard sandstone. All the specimens of this rock which I have examined, whether brought home by Mr. May or sent by the Admiralty, belong to the same light-colored, hard, sub-crystalline, pinkish sandstone, with very fine flakes of white mica; the successive layers (which are much foliated) being strikingly covered by thin elongated crystals of black tourmaline. The rock has altogether the appearance of having undergone considerable metamorphosis, and much elevation and disturbance. Geodes of pure white quartz, with large micaceous coatings, also occur. As soon as the party had become somewhat settled, it was determined to make a direct overland communication by Yóruba with Lagos, and Mr. May offering himself for this service, accomplished it satisfactorily, as explained in a notice laid before the Society. In the mean time Lieut. Glover made journeys up the river, visiting Wawa and Busa, and definitely ascertained the impracticability of navigating the river for a few miles beyond the spot of the encampment, a waterfall at Waru being an impassable barrier even for canoes in any season.

"Mr. May having waited on the sea-coast, expecting another steamer from England, at last returned to the encampment through Yóruba, and then set out on a more extended journey, with a view to exploring the country, and of establishing postal communication in a line from Lagos to the confluence. Having first travelled to Hadan (the road between Lagos and Hadan being well known and used), he passed eastward, and journeyed for many weeks through the previously unvisited districts of Ife, Ijesha, Igbouma, Yagha, &c., being warmly received, and observing everywhere that the people were quiet, orderly, and industrious; though these good qualities are here and there broken in upon by marauding or slave-catching armies, sent into the Yóruban country by powerful neighbors. The details of this journey were communicated to the Foreign Office in January last, and will, I presume, soon reach the Society.

"Approaching to within fifty or sixty miles of the confluences of the Quorra and Chadda Rivers, Mr. May was compelled to alter his route, and proceed northwards, visiting the ruined famous town Ladi, crossing the Quorra at Shaw, and journeying thence on the north side of the river through Núpe to Rabba.

"Lieutenant Glover had during this time also visited the coast by Mr. May's first route, and was now there waiting to pilot up the river the steamer which was at last coming to the relief of the party. Dr. Baikie and the other members of the expedition had been chiefly employed during the year in culti-

vating a good understanding with their neighbors, reducing their language, &c., whilst the energies of Mr. Barton were amply occupied on the botany of this part of Africa. In October, 1858, just a twelvemonth after the settlement of the expedition at the spot in question, the *Sunbeam* steamer arrived, the whole party were then embarked, and proceeded down the river to Fernando Po, there to recruit the health of the officers and men, and make arrangements for farther exploration. During the twelvemonth's residence in Núpe the most friendly relations were maintained with the king, his brother, and chiefs, and the natives generally; supplies being often received overland from Lagos.

"At Fernando Po (November, 1858), a re-organization having taken place, and the preparations being completed, the party again set out, now in the steamer *Rainbow*, built and sent for the purpose, and endeavored to re-ascend the river. But it was then found that this vessel, which draws four feet of water, could not ascend the Niger even in the month of January; the waters subsiding until June, when they increase. In consequence, the party was obliged again to return to the sea, and since have set out upon the land-journey from Lagos to Rabba (upon the route opened up by Mr. May), whence it is purposed to proceed with an expedition, the friendly objects of which must by this time have made a due impression on the native chiefs, and from which we may anticipate the gain of much knowledge when all the acquisitions of Dr. Baikie and his associates are unfolded."

KHANIKOFF'S EXPEDITION IN CENTRAL ASIA.—At a recent meeting of the American Oriental Society in New York, the Corresponding Secretary, Prof. Wm. D. Whitney of Yale College, presented a letter from the Chev. N. Khanikoff, dated Kerman, Persia, April 7–19th, 1859, in which he speaks as follows of the journey which he has just made.

"I have just completed, or nearly so, a very interesting journey through Khorassan, Western Afghanistan, and Northern Seistan. The whole region traversed by the scientific expedition which I have had the honor to conduct has been carefully surveyed, the situation of its principal points has been fixed astronomically, for more than thirty points ascertainment has been made of the magnetic coördinates, and of the intensity of magnetism corresponding therewith, and the profile of the territory has been determined by barometrical observations and trigonometrical measurements. The botanical researches have been made by Prof. Bunge, and the geological investigations by M. Göbel; the oriental literature, archæology, and numismatics have fallen to my share, and I hope soon to have the pleasure of communicating to the Society a succinct view of the results at which I have arrived." D. C. G.

ART. XII.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—(2d ARTICLE.)

IN our last Number* we gave some observations of this grand auroral exhibition, from a number of stations widely distant from each other. We now put on record some facts observed respecting the influence of the Aurora upon the wires of the electric telegraph. We hope in our next Number to be able to communicate additional intelligence respecting this Aurora.

1. *Observations made at Boston, Mass., and its vicinity by* GEORGE B. PRESCOTT, *Telegraph Superintendent.*

My attention was first called to the possibility of the Aurora Borealis affecting the telegraph wires in 1847, while operating the Morse (electro-magnetic) telegraph at New Haven; but I was not fortunate enough to observe it until the winter of 1850. At this time I became connected with Bain's (electro-chemical) telegraph in this city, and observed some effects of the aurora; but, owing to the feeble displays, only to a limited extent.

In September, 1851, there was a remarkable display of the Aurora Borealis, which completely took possession of all the telegraph lines running out of the city, and effectually prevented any business being done over them during its continuance.

The following winter there was another remarkable display, which occurred upon the 19th of February, 1852. I furnish from data recorded in my journal at that time the following particulars in regard to this phenomenon.

The system of telegraphing used upon the wires during the observations of February, 1852, was Bain's electro-chemical. The circuit was what is known as the open circuit,—that is, the key, which throws the current from the battery upon the line, was always open when a message was being received from a distant station, and the current passed through the chemically prepared paper to the earth without uniting with the home battery. Each station was furnished with its own battery, the negtive pole of which was invariably connected with the earth, and the positive, by the depression of the key, with the line.

The line extended in a direction nearly northeast and southwest. The paper was prepared with a solution of cyanid of potassium, made after the following recipe. Six parts prussiate potassa dissolved in water; two parts nitric acid; two of ammonia. This solution will scarcely color the paper, while it will render it quite sensitive to the action of the electric current. The stylus was made of No. 30 iron wire. A battery of ten

* Vol. xxviii, p. 385–408.

cups Grove, with the line well insulated, will decompose the salts, and uniting with the iron stylus, leave a bright blue mark upon the paper, at a distance of 230 miles.

The positive pole only produces a colored mark; the negative bleaches the paper.

When there is no electric current upon the wires, the pen leaves no impression upon the paper; but the slightest current will produce decomposition; and the color of the mark depends upon the strength of the current.

Free or common electricity produces no color upon the paper. It emits a bright spark in passing from the stylus to the moistened paper; produces a quick, sharp noise, like the snapping of a pistol and disappears. This effect is totally unlike that of the Aurora Borealis, as will be seen from the following.

Thursday, February 19, 1852. Towards evening a blue line appeared upon the paper, which gradually grew darker and larger, until a *flame of fire* followed the pen, and burned through a dozen thicknesses of the prepared paper. The paper was set on fire by the flame, and produced considerable smoke. The current then subsided as gradually as it came on, until it entirely disappeared, and was then succeeded by a negative current, which bleached instead of colored, the paper; this also gradually increased, until, as with the positive current, it burned the paper, and then subsided, to be followed by the positive current again. This state of things continued during the entire evening, and effectually prevented any business being done over the wires. The current came in waves of varying intensity—light at first, then stronger, until having attained to the volume and intensity of at least two hundred Grove cups, it subsided, and was followed by a current of the reverse polarity. This *invariably occurred*, and may be set down as an established fact, that the currents from the Aurora Borealis always change their polarity during every wave.

I have seen the auroral current produce magnetism, decompose chemicals, and produce heat and fire.

The effects of the magnetic storm of August 28th, 1859, were apparent upon the wires during a considerable portion of Saturday evening, and during the entire day, Sunday. At 6 P. M. the line to New Bedford (60 miles in length, running a little west of south) could be worked only at intervals, although, of course, no signs of the Aurora Borealis were visible to the eye at that hour. The same was true of the wires running east through the state of Maine as well as those running north to Montreal. The wire between Boston and Fall River had no battery connected with it on Sunday, and yet there was a current upon it during the entire day, which caused the keepers of the electro-magnets to open and close as the waves came on and receded.

Upon the lines which had batteries connected Sunday evening, it was observed that the poles changed during every wave of the aurora—each wave occupying from fifteen seconds to half a minute. When the poles of the Aurora were in unison with the poles of the battery upon the line, the effect was to increase the current; but when they were opposed, to neutralize it. I will give my proofs of this farther on. It is to be observed that the effects I have illustrated in relation to the Aurora of August 28th, 1859, were observed upon the Morse (electro-magnetic) system. The same were, however, observed upon the House and Hughes lines running out of the same office.

It is not true that there is any difference in the effect produced upon the wires by the Aurora Borealis, whether they run east and west, or north and south. Lines running to every point of the compass diverge from the office here and were equally affected. Even the short wire running from the office in State street to the observatory in Cambridge (five miles long) was sensibly affected.

In an article which I published in the Boston Journal, August 31st, I stated that the current from the Aurora Borealis could have been used for telegraphic purposes, but I did not imagine it would be so soon verified by the actual fact.

On Friday, September 2d, 1859, upon commencing business at 8 o'clock A. M. it was found that all the wires running out of the office were so strongly affected by the auroral current as to prevent any business being done, except with great difficulty. At this juncture it was suggested that the batteries should be cut off, and the wires simply connected with the earth. The Boston operator accordingly asked the Portland operator to cut off his battery and try to work with the auroral current alone. The Portland operator replied, "I have done so. Will you do the same?" Boston operator answered, "I have cut my battery off and connected the line with the earth. We are working with the current from the Aurora Borealis alone. How do you receive my writing?" "Very well indeed," rejoined the Portland operator; "much better than with the batteries on. There is much less variation in the current, and the magnets work steadier. Suppose we continue to work so until the Aurora subsides?" "Agreed," said the Boston operator. "Are you ready for business?" "Yes; go ahead," was the reponse. The Boston operator then commenced sending private dispatches, which he was able to do much better than when the batteries were on, and continued to use the wire in this manner for about two hours, when, the Aurora having subsided, the batteries were resumed.

While this singular phenomenon was taking place upon the wires between Boston and Portland, the operator at South Brain-

tree—Miss Sarah B. Allen—informed me that she was working the wire between that station and Fall River—a distance of about forty miles south—with the auroral current alone. Since then I have visited Fall River and have the statement verified by the intelligent operator upon the railroad line at the dépôt in that village.

The office at the dépôt is about half a mile from the regular office in the village. The battery is kept at the latter place, but the operator at the dépôt is provided with a button or switch, by which he can throw the battery off the line, and put the wire in connection with the ground at pleasure. The battery at the other terminus of the line is at Boston, but the operator at South Braintree is furnished with a similar switch, which enables her to dispense with its use at pleasure. There are no intermediate batteries; consequently if Fall River operator puts his wire to the earth, and the South Braintree operator puts her wire to the earth, the line is without battery, and of course, without an electrical current.

Such was the state of the line upon the 2d of September last, when for nearly two hours, they held communication over the wire with the aid of the celestial batteries alone!

I have restricted myself in this article to facts observed by myself. I have stated nothing which I am not absolutely certain of, and which, if necessary, can be proved by a number of reliable witnesses.

2. *Observations made at White River Junction, Vt., communicated by* J. H. NORRIS, *Telegraph Superintendent.*

During the forenoon of Sept. 2d, an unusual current of varying intensity was present most of the time on the wires of the Vermont and Boston telegraph. The polarity of this current appeared to change frequently, sometimes being opposite to and nearly or quite neutralizing the battery current when an attempt was made to use the line; at other times much increasing the force of the battery current. The auroral current produced the same marks upon our chemical paper (we use the Bain or chemical system of telegraph) as those produced by the use of the battery. Signals and messages were transmitted between Boston and Manchester *by the sole use of the auroral current.*

3. *Observations made at Springfield, Mass., by* J. E. SELDEN.

On the evening of Aug. 28th, upon the Boston and New York circuit, at one moment there was a very heavy current on the wire, and the next none at all. On the Albany and Springfield circuit, *a flash* passed across from the break key of the telegraph apparatus to the iron frame, the flame of which was about half the size of an ordinary jet of gas. It was accompanied by

a humming sound similar to a heavy current passing between two metal points almost in contact. The heat was sufficient to cause the smell of scorched wood and paint to be plainly perceptible.

[The observations at Springfield, as well as those at New York and Washington were communicated by Messrs. Lewis and Lovett of the New York telegraph office.]

4. *Observations made at New York by* J. C. Crosson, *Telegraph operator.*

On the evening of Aug. 28th, at 7½ o'clock, I experienced considerable difficulty in working, on account of the variation of current. I could work south by constantly altering the adjustment of my magnets, but the magnetism on the eastern circuit was so nearly destroyed that I could do nothing. About ten o'clock I could see nothing of the Aurora in the southern hemisphere, yet the same variations of current were manifest upon the line for an hour afterward. There was during this time a very strong turning current from the east, which resembled a reversed current so much that I disconnected my battery and put on a 'ground,' but I could not then get magnetism sufficient to work a simple armature. At $12^h\ 30^m$ the current from the east assumed a new feature, producing enough magnetism to work quite well, yet wavering and varying in intensity.

5. *Observations made at Philadelphia, communicated by* H. Emmons Thayer, *Telegraph Superintendent.*

On the evening of Aug. 28th, about 8 o'clock, we lost current on all our four wires running from Philadelphia to New York, and we had strong circuit as if from a near ground connection; but there was no interruption on wires running south to Baltimore and Washington. At $9^h\ 10^m$ the wires were relieved to a great extent from the influence of the Aurora, giving us our usual working current.

On testing wires at 8 o'clock on the morning of Sept. 2d, I found two of our wires, those running *via* Camden and Amboy to New York, strongly under the influence of an Aurora. The effect was different from that of Aug. 28th. There was an intensity of current which *gave a severe shock* when testing, giving a reversed current, neutralizing our batteries, and destroying magnetism. On removing the batteries, we had a very strong circuit, giving powerful magnetism, but could not raise New York. On the line running from this city to Pittsburgh, the operator, Mr. Steacy, succeeded in transmitting a business message to Pittsburgh *wholly on the auroral current.* The current was changeable, suddenly disappearing and returning at intervals of from five to ten minutes. The signals were distinct and

the conversation lasted four or five minutes, the operators exchanging remarks as to the singularity of the phenomenon. At 9 A. M. all the wires were relieved from the effects of the Aurora, and worked well as usual.

6. *Observations made at Washington, D. C., by* FREDERICK W. ROYCE, *Telegraph operator.*

On the evening of Aug. 28th I had great difficulty in working the line to Richmond, Va. It seemed as if there was a storm at Richmond. I therefore abandoned that wire, and tried to work the northern wire, but met with the same difficulty. For five or ten minutes I would have no trouble, then the current would change, and become so weak that it could hardly be felt. It would then gradually change to a 'ground' so strong that I could not lift the magnet. The Aurora disappeared at a little after 10 o'clock, after which we had no difficulty. During the auroral display, I was calling Richmond, and had one hand on the iron plate. Happening to lean towards the sounder, which is against the wall, my forehead grazed a ground wire. Immediately I received *a very severe electric shock,* which stunned me for an instant. An old man who was sitting facing me, and but a few feet distant, said that he saw *a spark of fire* jump from my forehead to the sounder.

7. *Observations made at Pittsburgh, Pa., communicated by* E. W. CULGAN, *Telegraph manager.*

During the Aurora of Aug. 28th the intensity of the current evolved from it varied very much, being at times no stronger than an ordinary battery, and then suddenly changing the poles of the magnets it would sweep through them, charging them to their utmost capacity, and compelling a cessation of work while it continued.

On the morning of Sept. 2d, at my request the Philadelphia operator detached his battery, mine being already off. We then worked with each other at intervals as long as the auroral current continued, which varied from thirty to ninety seconds. During these working intervals we exchanged messages with much satisfaction, and *we worked more steadily when the batteries were off than when they were attached.*

On the night of Aug. 28th the batteries were attached, and on breaking the circuit there were seen not only sparks (that do not appear in the normal condition of a working line) but at intervals regular *streams of fire*, which, had they been permitted to last more than an instant, would certainly have fused the platinum points of the key, and *the helices became so hot* that the hand could not be kept on them. These effects could not have been produced by the batteries.

ART. XIII.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part I.

(1.)

THE determination of the chemical equivalents of the simple substances seems with each new approach to accuracy, to destroy more and more the numerical relations once supposed to exist between the equivalent numbers of certain series of elements nearly related to each other by their properties. If we except the series formed by oxygen, sulphur, selenium and tellurium, there probably remains none of those usually recognized in which the numerical relation is rigorously exact. Chlorine, bromine and iodine are represented by the numbers 35·5, 80 and 127, where the difference between the first and second term is 44·5, between the second and third, 47. Lithium, sodium and potassium have the numbers 6·95, 23, 39·2; difference of first and second terms, 16·05, of second and third, 16·20. Calcium, strontium and barium have 20, 43·77 and 68·6; difference of first and second, 23·77, of second and third, 24·83. It can be demonstrated that other relations exist, approaching quite as near to exactness as these, and some where the accuracy is perfect.

Few of the so-called elements present more directly marked analogies than nitrogen, phosphorus, arsenic and antimony, and the very interesting discoveries of Cahours and Hofmann respecting the phosphorus bases have shown that phosphorus stands in every respect intermediate between nitrogen and arsenic, forming compounds of the type $3(C_4H_5)PHCl$, &c., like the nitrogen compounds as well as those of the type $3(C_4H_5)PO_2$, &c., like those of the arsenic and antimony groups. These authors further observe that the equivalents of phosphorus, arsenic and antimony differ by nearly the same number (44 to 45), but that nitrogen does not exhibit this relation.*

Beyond the fact of the approximate equality of these two differences, the analogy has never been extended. The following considerations will show that this relation not only extends to nitrogen but may be carried with exactness to other elements.

If we form a descending arithmetical series beginning with antimony =120·3, and diminishing by a common difference of 45 (45·3 in one instance, 44 in several) we shall find that such a series does not cease with the third term, P=31, but gives for a fourth −14, the exact equivalent of nitrogen with a negative sign. The fifth term will be −59, the exact equivalent of tin, with a negative sign. The sixth will be −104, or very nearly the equivalent of lead (also with a negative sign). The seventh

* Kopp u. Will, Jahresbericht für 1857.

—149, very nearly the double of the equivalent of arsenic, a previous term in the same series. These results are exhibited in the following table.

Differences.	Calculated equivalents.	Received equivalents.
	120	Sb = 120·3
45	75	As = 75
44	31	P = 31
45	— 14	N = 14
45	— 59	Sn = 59
45	—104	Pb = 103·5
45	—149	2As = 150
45	—194	—— ——
45	—239	2Sb = 240·6
45	—284	—— ——
44	—328	= 2×164
44	—372	—— ——
44	—416	2Bi = 416

It will be seen presently that the number 164, the eleventh term in the above table, occurs also in the ascending positive series, and may represent the equivalent of a metal existing but as yet unknown.

If we examine the position occupied by antimony, arsenic, phosphorus and nitrogen in the electro-chemical scale of Berzelius we shall find that in proportion as their equivalent numbers diminish, their properties become more and more electro-negative; a corresponding change is also visible in the organic radicals which these elements are capable of forming by their union with carbon and hydrogen. The passage from the positive to the negative sign in the interval between phosphorus and nitrogen is accompanied by a marked change in the nature of the organic radicals into which these elements enter—$3(C_4H_5)N$ does not possess the power of combining directly with oxygen, chlorine and sulphur which $3(C_4H_5)P$, $3(C_4H_5)As$, $3(C_4H_5)Sb$ exhibit in so high a degree. The methyl compounds show the same differences as the ethyl. Standing between nitrogen and arsenic, phosphorus is every way more closely allied to the latter of

these substances, not only by the analogies of its organic radicals, but also by the polybasic nature of phosphoric acid.* Although tin and lead represent the further members of the same series in reference to their equivalent numbers, it is evident that the increase of electro-negative relations does not extend to them. Moreover bismuth, antimony, arsenic, phosphorus and nitrogen at their maximum of oxydation combine with five equivalents of oxygen and chlorine, whereas tin unites with but two of each, and lead at most with two of oxygen and one of chlorine.

Again, if we begin with phosphorus, and form an ascending series with a common difference of 44 (except in one instance), we shall find both the number 164, the double of which constituted the eleventh term of the preceding table, and also the equivalent of bismuth, the double of which formed the thirteenth term of the same table.

Differences.	Calculated equivalents.	Received equivalents.
	31	P = 31
44	75	As = 75
45	120	Sb = 120·3
44	164	— ——
44	208	Bi = 208

These four elements exhibit strong analogies and are all isomorphous with each other.

If, taking mercury as a starting point, we subtract the number 44 from each term to find the following one, we shall obtain the series—

Differences.	Calculated equivalents.	Received equivalents.
	100	Hg = 100
44	56	Cd = 56
44	12	Mg = 12
44	−32	Zn = 32·6

The salts of the protoxyds of the three last of these metals are isomorphous. It may seem forced to place mercury in the same group, but its analogies with zinc are perhaps as strong as those which it exhibits with silver, next to which it has usually been classed, principally because the oxyds of both metals are reduced by heat. Mercury, like zinc, cadmium and certain other metals, is capable of replacing an atom of hydrogen in hydrid of ethyl, C_4H_5H, and of producing in this manner the conjugate

* See the very interesting paper of Cahours and Hofmann above referred to.

organic radical mercur-ethyl, C_4H_5Hg, analogous to zinc-ethyl, C_4H_5Zn. This compound has recently been isolated by Buckton.* Silver does not appear to be capable of a similar substitution; when chlorid of ethyl is made to react upon zinc-ethyl the products are ethyl, chlorid of zinc and metallic silver. It has lately been shown by Hallwachs and Schafarik that magnesium forms an ethyl-magnesium.† These metals are likewise all volatile, and it has been shown by H. St.C. Deville that magnesium, like the others, is readily distilled.

It is not a little curious that the numerically negative members of this series lead into the positive members of the foregoing; if we continue the subtraction of 44, we find for the fifth number 76, or nearly the equivalent for arsenic, for the sixth 120, very nearly that of antimony, for the seventh 164, corresponding, as before remarked, with a possible undiscovered metal, and for the eighth 208, or exactly the equivalent of bismuth. The two series thus naturally lead to each other. The equivalent for arsenic given by L. Gmelin, viz. 75·4, accords more nearly with some of these series and renders them more exact than that adopted by the Jahresbericht which has been here employed.

The members of these two analogous series are further united by the fact that all of them, eleven in number, are capable of uniting with the hydro-carbons of the methyl, ethyl, &c. type to form powerful organic metals, and that this capacity appears to be limited to these elements alone.‡

(2.)

The magnesia group includes a well marked natural family of metals, whose oxyds having the constitution RO are related with each other by isomorphism. The equivalents of these metals, according to the most recent determinations, are as follows:

Magnesium,	12	Chromium,	26·7
Manganese,	27·5	Zinc,	32·6
Iron,	28	Cadmium,	56·
Cobalt,	29·5	Copper,	31·7
Nickel,	29·6	Lead,	103·5
Uranium,	60		

[Some of the elements here enumerated are found also in the foregoing or following series. It need scarcely be remarked that no absolute classification into groups is possible; lead for instance is in some of its combinations isomorphous with the barium group, in others with the magnesia, copper in grey cop-

* See this Journal, [2], xxviii, 146. † This Journal, [2], xxviii, 147.

‡ There appears some reason to believe that an ethyl-aluminum may exist, which would form an exception to this law. If so, the same property may possibly be extended to others of the metals of the earths.

per ore is isomorphous with silver, while its carbonate, sulphate and seleniate are isomorphous with those of the magnesia group.] The equivalents of the above metals are related in the following manner by the number 44:

Cu 31·7	Zn 32·6	Cd 56
Mg 12·	Mg 12	−Mg 12
43·7	44·6	44
U 60	U 60	U 60
Mn 27·5	Fe 28	Co 29·5
87·5, ½=43·75	88, ½=44	89·5, ½=44·75
U 60	U 60	Pb 103·5
Ni 29·6	Cr 26·7	−U 60
89·6, ½=43·5	86·7, ½=43·35	43·5
Cd 56	Cd 56	Pb 103·5
Zn 32·6	Cu 31·7	Fe 28
88·6, ½=44·3	87·7, ½=43·9	131·5, ⅓=43·8

and so likewise by adding to the equivalent of lead, the equivalents of manganese, cobalt, nickel, and chromium, we obtain in each case a number almost exactly equal to three times the number 44.

To sum up: with Cu and Mg, Zn and Mg the sum of each pair is 44 nearly.

With Cd and Mg, Pb and Ur, the difference of each pair is 44 or nearly.

With U and Mg, U and Fe, U and Co, U and Ni, U and Cr, Cd and Zn, the mean term is 44 or nearly.

With Pb and Mn, Pb and Fe, Pb and Ni, Pb and Co, Pb and Cr, the sum of each pair is three times 44 nearly.

It has been before pointed out that the strong analogy existing between Mg, Cd and Zn extends to their equivalents, that of Mg being added to that of Zn gives the number 44 nearly, subtracted from that of Cd, 44 exactly.

(3.)

The following metals may be classed together as tending to form acids:

Tin, 59	Vanadium, 68·8
Titanium, 25	Molybdenum, 48
Tantalum, 68·6	Tellurium, 64
Tungsten, 92	Niobium, 48·82*

Relations depending upon the number 44 exist between these equivalents.

* By the recent determination of H. Rose, Poggendorff, civ, ext. in this Journal, [2], xxvii, 127.

If from Ta 68·6 we subtract Ti 25, the remainder is 43·6, 44 nearly.

If from V 68·8 we subtract Ti 25 the remainder is 43·8, 44 nearly.

If from W 92 we subtract Mo 48 the remainder is 44 exactly.

If from W 92 we subtract Nb 48·82 the remainder is 43·18.

If to Te 64 we add Ti 25 the sum 89 is twice 44 nearly.

If to Te 64 we add Ta 68 the sum 132·6 is three times 44 very nearly.

If to Te 64 we add V 68·8 the sum 132·8 is three times 44 very nearly.

If the equivalent of Sn=59 be multiplied by $\frac{3}{4}$ the result is 44·25, in other words 3Sn=4×44·25.

If we add Sn 59, Ta 68·6, W 92, V 68·8 and Te 64, the result is 352·4, 8×44=352.

If we add Ta 68·6, Nb 48·82, Sn 59, we have 176·42, 4×44=176.

If we add Ta 68·6, Mo 48, Sn 59, we have 175·6, 44×4=176.

If we add W 92, 2Te 128, we have 220, 5×44=220.

If we add Ta 68·6, Mo 48, 2Sn 118, V 68·8, Nb 48·82, we have 352·22, 8×44=352.

These analogies, though many are very complex, approach exceedingly near to absolute exactness; in the last for instance the difference amounts to only $\frac{1}{176}$ of the whole amount.

(4.)

If commencing with gold, Au=197, we form a diminishing series with a common difference of 44·5, we shall find for its terms—

Differences.	Calculated equivalents.			Received equivalents.
	197	-	-	Au=197
44·5	152·5	-	-	— ——
44·5	108	-	-	Ag=108
44·5	63·5	-	-	Cu = 63·4

The equivalent of Cu has been here taken at double that usually employed, or that which results from taking the first oxyd of copper as CuO, a view formerly entertained by Berzelius, L. Gmelin and other distinguished chemists.* It is doubtful if this be not the true equivalent; in grey copper ore, if we consider Cu as 32·7 we must admit that one equivalent of silver is replaced by two of copper, which is improbable. Moreover the two sulphides, AgS and CuS (Cu=63·4) are isomorphous.†

* Thomson, Inorganic Ch., i, 592.

† L. Gmelin, Handbook, i, 95. The recent experiments of Cannizaro give additional reason for doubling the equivalent of copper.

Viewed in this light the above series approaches very near to exactness, and may probably be wholly so, for the equivalent of copper is by no means positively known. Dumas' recent researches were too unconcordant to permit him to offer a positive opinion. It will be shown further on that these metals, as well as many others here grouped together by relations depending upon a number approximating to 44·5, are also united by analogies of atomic volume.

The second number in the above series, 152·5, does not correspond with the equivalent of any known element, and it, like the number 164, which occurs twice in the nitrogen series, may represent the equivalent of some elementary body as yet unknown. In the second part of this paper, an additional argument will be presented in favor of the possible existence of two elements having these equivalent numbers.

The same relation may be extended with more or less approximation to the platinum group, which naturally subdivides itself into two families: the members of each family have very nearly the same equivalents, and the difference between the equivalents of the two families approaches to 45.

Rhodium,	52·2	Platinum,	98·7
Ruthenium,	52·2	Iridium,	99
Palladium,	53·3	Osmium,	99·6

The difference between the equivalents of platinum and palladium is 45·4, the rest vary a little more.

The elements, carbon, boron and silicium, are united by the number 44 in the following manner: if we take Gerhardt's equivalent for carbon Ɵ=12, the sum of the equivalents of these three substances amounts to 44 exactly.

Carbon (Ɵ),	12
Boron,	11*
Silicium,	21·
	44

With respect to the metals of the earths and alkaline earths, magnesium and calcium have been considered elsewhere, yttrium, erbium and terbium, and the cerite metals have not yet been sufficiently examined, the equivalents of the three first have in fact not yet been determined, thorium exhibits a relation which will be hereafter pointed out; there remain therefore glucinum, aluminium and zirconium. The equivalents of these metals, viz. 4·7, 13·7 and 22·4 are evidently too small to follow precisely the same law which governs the various series previously considered, but that it extends also to them becomes evident

* As determined by Deville by analyses of bromid of boron.

by multiplying their equivalents by 5, when they form a series analogous to the foregoing.

Differences.		
	23·5 - -	5G = 23·5
45		
	68·5 - -	5Al = 68·5
45		
	113·5 - -	5Zr = 112·

(5.)

If we take the equivalent of nitrogen, add to it three times 45 and divide by 8, we shall have nearly the equivalent of fluorine, and by continuing nearly in the same manner, we obtain those of chlorine, bromine and iodine.

$$\frac{14+3\times45}{8} = 18{\cdot}62, \quad \text{Fl} = 19$$

$$\frac{14+6\times45}{8} = 35{\cdot}5, \quad \text{Cl} = 35{\cdot}5$$

$$\frac{14+14\times45}{8} = 80{\cdot}5, \quad \text{Br} = 80$$

$$\frac{2\times14+22\times45}{8} = 127{\cdot}25, \quad \text{I} = 127$$

(6.)

Several elements, taken in pairs, have equivalents which differ from each other by a number approximating to 45.

Na	23	+ 45·6 =	Ba	68·6
Be	4·7	+ 43·3 =	Di	48
Ti	25	+ 43·6 =	V	68·6
Cl	35·5	+ 44·5 =	Br	80
Al	13·7	+ 45·9 =	Th	59·6
Zr	22·4	+ 46·4 =	Ba	68·6

(7.)

At the commencement of this paper reference was made to the well known numerical relations existing between Li, Na, K; Ca, Sr, Ba; Cl, Br, I. Inexact as are these (with the exception of the first which approaches tolerably near), the strong analogies which exist between the substances themselves, give to their relations an interest which they would not otherwise possess, and it is to be remarked that they all belong to that class which it has been the object of this paper to develop, viz., those depending upon the number 44–45.

1. The equivalent of sodium is well known to be nearly a mean between those of lithium or potassium—if the equivalent

of lithium be added to that of potassium, or if that of sodium be doubled, a number is obtained in either case approaching nearly to 45.

2. The equivalent of strontium is nearly a mean between those of calcium and barium: Sr=43·75, half the sum of Ba 68·6 and Ca 20 is 44·3; in either case 44 nearly.

3. The equivalent of chlorine, 35·5, subtracted from that of bromine, 80, gives 44·5. The equivalent of bromine subtracted from that of iodine gives 47.

(8.)

With respect to three of the groups already considered, a further relation exists. For greater clearness the groups in question have been arranged in the following table:

Group A.			Group B.		
Differences.			Differences.		
	100	Hg=100		208	Bi =208
44	56	Cd= 56	44	164	— —
44	12	Mg= 12	44	120	Sb =120·3
44	−32	Zn = 32·6	45	75	As = 75
			44	31	P = 31
Group C.			45	−14	N = 14
	92	W = 92	45	−59	Sn = 59
44	48	Mo= 48	44·5	−103·5	Pb =103·5

If to the equivalent of the substances forming series A, we add successively to each term the equivalent of fluorine, we shall obtain four corresponding terms of series B.

Mercury	100	Cadmium	56	Magnesium	12	Zinc	−32·6
Fluorine	19	Fluorine	19	Fluorine	19	Fluorine	19
	119		75		31		−13·6
Antimony	120·3	Arsenic	75	Phosphorus	31	Nitrogen	14

Further: If to two corresponding terms of series A we add the equivalent of chlorine, we shall obtain the two terms of series C.

Cadmium	56	Magnesium	12
Chlorine	35·5	Chlorine	35·5
	91·5		47·5
Tungsten	92	Molybdenum	48

And if to two terms of the same series A we add the equivalent of bromine, we also find the terms of series C.

Magnesium	12	– Zinc	–32·6
Bromine	80	Bromine	80
	92		47·4
Tungsten	92	Molybdenum	48

If we add respectively the four terms of series A with the four corresponding terms of series C, we shall in each case obtain a number which is very nearly twice forty-four; that is, the mean of each pair of series is 44 nearly.

– Zinc	–32·6	Magnesia	12	Cadmium	56	Mercury	100
Antimony	120·3	Arsenic	75	Phosphorus	31	–Nitrogen	–14
	88·7		87		87		86

It will be remarked that where a series leads to the equivalent of an element, but with a negative sign, that negative sign has been in all cases preserved in the further examination of its numerical relations.

(9.)

It is evident that the number 44—45 plays an important part in the science of stoichiometry, and the relations which depend upon it are supported, in some cases at least, in a remarkable manner, by analogies of atomic volume. That such analogies are a support becomes evident from the following considerations.

Solids and liquids are very far from being governed by the laws which determine the combinations of gases, in volumes either equal or having some very simple relation to one another. Therefore, if we find that in some few instances such a relation does hold good with solid substances, we may naturally expect to find a close relationship existing between those substances thus united. We may even be permitted, by way of hypothesis, to advance a step further, and finding that a given volume of silver unites with a given volume of oxygen, and that the same volume of gold unites with precisely the same volume of oxygen, to conjecture that gold may differ from silver only by a third substance, which unites with the silver without increasing its volume, or affecting the amount of oxygen which it is capable of saturating, but which, on the other hand, alters its chemical equivalent, its specific gravity, and other physical characters.

Moreover, if we find that by subtracting from the chemical equivalent of silver, half the difference between the equivalents of silver and gold, we obtain the equivalent of a third metal, copper ($Cu=63·4$), which also, under equal volumes, combines with a quantity of oxygen expressed by a very simple relation

with that capable of saturating gold and silver, we may at least speculate that the three may form a series consisting of two substances combined in different proportions. It is true that we must be extremely cautious about venturing upon hypotheses involving a compound constitution of bodies which all our efforts have hitherto proved ineffectual to decompose, but on the other hand it must be admitted that when we find so-called elements arranging themselves into a series of terms having a common difference, and when we find the terms of these series united by equality or simple relation of atomic volume, we cannot grant that their elementary nature has been absolutely established.

The following substances combine relations of chemical equivalents already pointed out, with analogies of atomic volume:

Differences of Equivalents.		Atomic† volume.	Relation of At. vol.
45*	Nitrogen, - - -	14·42	- 2
44	Phosphorus (vapor), -	7·01	- 1
	Arsenic (vapor), - -	7·07	- 1
44·5*	Lead, - - -	9·09	- 1
	Tin, - - -	8·09	- 1
45*	Nitrogen,		
45	Phosphorus, - -	17·7	- 2
44	Arsenic, - - -	12·58	- $\frac{3}{2}$
45·3	Antimony, - -	18·—	- 2
87·7 or twice 43·85	Bismuth, - - -	21·18	- $\frac{7}{3}$

(Where phosphorus, arsenic, &c., are compared in the solid state, the unit of relation is of course different). It has been already remarked that in point of chemical relations generally, lead and tin are less closely united with the series than the other members composing it, but the relation between the atomic volumes of lead and antimony, the latter almost the last term at the other end of the series, is almost absolutely exact. Nitrogen is of course omitted in the second table, as we do not know what would be its atomic volume in the solid state.

* In the cases of nitrogen, tin and lead, the equivalents are taken with a negative sign, as before explained.

† The numbers here given for the atomic volumes are calculated from the specific gravities adopted in Gmelin's Handbook, and the latest and most reliable determinations of chemical equivalents.

Differences.		Atomic Volume.	Relation of At. Vol.
89 or twice 44·5	Gold, - - -	10·26	1
	Silver, - - -	10·35	1
44·6	Copper (Cu=63·4), -	7·27	¾

It will be observed that the difference between the equivalents of copper and silver approaches very near to absolute exactness with half the difference between the equivalents of silver and gold, and as the equivalent of copper is by no means positively settled, the relation may be rigorously exact. If we take the mean between the number adopted by L. Gmelin and that adopted by the Jahresbericht (always considering cuprous oxyd as CuO), we shall have for the difference between the equivalents of copper and silver the number 44·5, half the difference between the equivalents of silver and gold, with mathematical exactness.

Difference.		Atomic volume.	Relation of At. vol.
44	Tungsten, - -	5·28	1
	Molybdenum, -	5·58	1

In the series Hg, Cd, Mg, Zn, similar analogies are not well marked: the atomic volumes of the three first metals are not very far apart,

	Atomic volume.
Hg, - - - -	7·37
Mg,* - - -	6·85
Cd, - - - -	6·48

but the atomic volume of zinc differs considerably; it is 4·72.

In each of these different series, each term differs in its equivalent by the number 45 or a number approaching very near to 45, and yet the addition of this large amount of matter is in most cases accompanied by no change in volume, or when a change takes place, it is expressed by some simple relation to the original volume. Some of these relations of atomic volume are well known and are only here presented in view of the confirmation which they afford of the series here established, but it is believed that the connection of the atomic volume of copper with those of gold and silver, and of those of tin and lead with those of the elements of the antimony group are pointed out for the first time.

Ammermüller has noticed a fact not wholly dissimilar from this in the case of protoxyds of copper, mercury, tin and lead, which combine with a second equivalent of oxygen without change of atomic volume, the density alone increasing. But according to L. Gmelin, the specific gravities on which he based

* Taking the sp. gr. of magnesium at 1·75 as determined by Deville.

his calculations are too unreliable to render the fact well established.

The numbers adopted for the equivalents in the foregoing calculations are those obtained by the latest and most reliable determinations; they are taken from the table contained in the Jahresbericht der Chemie of Kopp & Will for 1857, published in August, 1858, and the last which the author has been able to obtain at the time of concluding this paper, and have been in no case altered or modified in the slightest degree with a view to preserve or increase numerical relations, which by slight changes of this kind can be often rendered much more symmetrical. Dumas, in one of his highly interesting papers on this subject* (Comptes Rendus, XLV, 709, extracted in Kopp and Will's Jahr. 1857) in his series $a+xd+yd'$, adopts the equivalents N=14, P=31, As=75, Sb=119, Bi 207 (see Jahresbericht, p. 35, where the equivalent of Bi is erroneously printed 108, by substitution of the values given for a, x, y, d, and d', 207 is obtained): whereas the equivalent of Sb as lately found by R. Schneider, confirmed by H. Rose, and adopted by Kopp & Will is 120·3. In another place (Comptes Rendus, XLVII, 1027) Dumas has taken the equivalent of the same metal at 122, thus adopting alternately the numbers 119 and 122, neither the true one, according to the exigencies of the two series. The equivalent number of bismuth in the series just mentioned is taken at 207, whereas it should be 208. In the series $a+xd$ we find Mg=12, Ca=20, Sr=44, Ba 68, Pb 104—the last three should be Sr 43·77, Ba 68·6, Pb 103·5. So with Li, Na, K, V, Zr, &c.

In the foregoing tables the calculated and received equivalents are placed by side of each other for comparison. The differences rarely exceed the possible errors in the determination of chemical equivalents, respecting some of which there is still much doubt. Dumas, in the paper above referred to, gives the results of many new determinations by himself, and arrives at the number 26 for both chrome and manganese, instead of the ordinarily received Cr=26·7, Mn=27·5. For copper his results disagreed too much to lead him to any positive conclusion.

The analogies here presented, all depending upon the same or approximately the same number, extend therefore—

To the series Pb, Sn, N, P, As, Sb, Bi.
To the series Hg, Cd, Mg, Zn.
To the series Au, Ag, Cu.
To the magnesia group, including Mn, Fe, Co, Ni, U, Co, and some of the metals also classed in the three preceding series.

* The interesting paper of Prof. Cooke (Memoirs Amer. Ac., 2d ser., vol. v) to which the author's attention has been called since concluding this paper, will be more particularly referred to in the Second Part.

To the metals belonging to the group Ti, Ta, W, V, Mo, Te and No; Sn belongs also to this series as well as to the first.
To the platinum group, Rh, Ru, Pd, Pt, Ir, Os.
To ☾, B, Si.
To G, Al, Zr.
The differences between Cl and Br, Br and I, approximate to the same number, as likewise do the relations between Li, Na and K, and between Ca, Sr and Ba.

This relation, therefore, extends to no less than forty-eight of the elementary bodies: to all except those as yet imperfectly understood, most of which may yet range themselves under the same law, and except the oxygen group, oxygen, sulphur, selenium and tellurium, substances which stand alone and unmistakably apart from the other elements.

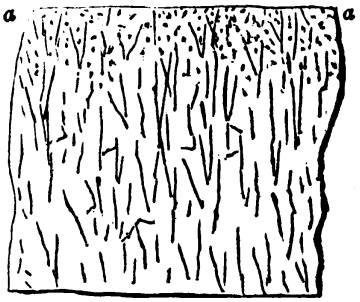
Philadelphia, Nov. 10, 1859.

ART. XIV.—*Remarks on the Dissolution of Field Ice;* by CHAS. WHITTLESEY, of Cleveland, Ohio.

THE interesting paper of Col. Totten, U. S. A., in the November number of this Journal for 1859, upon the rapid disappearance of ice in the northern lakes, recalls some observations that I had an opportunity to make on Lake Superior a few years since.

On the 8th of March, 1855, the inhabitants of Eagle River, a village in Haughton County, situated upon the most northerly part of Point Kewenaw, were engaged in procuring ice for their summer use. The severity of winter in that latitude (47° 22′ north) had so far relaxed, that the surface of the field was slightly porous from the direct action of the sun. There had been no rain; the atmosphere was clear and cool, but on the sunny side of houses and other objects the snow melted rapidly in the day time.

Below the soft and moist surface, at a few inches in depth, the ice was solid and pure to the bottom, its thickness being thirty inches. The blocks which the people were cutting out, were taken about 1000 feet from the shore. One of them nearly in the form of a cube, of thirty inches on each face, was suffered to lie upon the unbroken ice, its natural surface uppermost, as represented in the figure here inserted.



Block of ice 30 inches thick.
a a, upper surface.

I was thus enabled to take a direct view of the progress of its decay, as no doubt others have done many times, upon these lakes. As the force of the sun increased, the porous part on the

surface increased rapidly in depth, lines or planes of separation extending downward from it into the hitherto transparent and homogeneous mass. There were not at any time horizontal planes visible, indicating layers or lamination, in the original structure. A thin film of matter followed each newly formed crevice downwards, and bubbles of air rose continually through the same to the surface. These planes of division converged below, giving the block the appearance above represented, of inverted spikes or rudely formed pyramids, with their bases upward.

By ten o'clock A. M. the upper half of the block was divided in this manner. The figures were somewhat regular and were principally triangular and rectangular, reminding me of the imperfectly columnar red trap of the north shore of Lake Superior. By noon the block was so far disintegrated that it fell to pieces under a single blow, and remained a pile of roughly formed spikes, pyramids and prisms of various lengths. After this as so much new surface was exposed to the sun it melted very fast. The newly cut ice was still solid and clear except a few inches at the surface.

There seemed to be in the block that had so suddenly lost its form and solidity, a process of contraction, arising from an increase of temperature. I presume that this appearance can be thus accounted for. No doubt the planes of division existed in the solid ice, as results of the crystallization in freezing. The general law of structure in all masses slowly crystallizing from a state of fusion is the production of a prismatic structure perpendicular to the cooling surfaces. Basalt assumes its polygonal figures in obedience to the same law, and the structure of ice is quite in accordance with it. Its effects are not wanting even in some pastes, like starch and domestic cake.

This structure exists often where it is concealed. An ingot of block tin shows no crystalline structure, but by slow fusion the amorphous parts melt and run out leaving a skeleton of crystalline prisms. Ice is in the same predicament, and since in freezing water expands one-seventh of its volume, the first result of the fusion of a part of it is to dissect out the prismatic masses, leaving them standing isolated by reason of their being on a larger scale than the fluid volume from which they were formed. In this process the air bubbles no doubt materially assist by opening channels of escape for the ice-water.

What I have stated may assist in explaining why immense fields of fresh water ice disappear in a single gale of a few hours duration. When the temperature rises above 32° the ice soon loses its cohesion, and the first agitation breaks it up. In popular phrase it sinks, and is thus lost sight of suddenly; but in truth it is dissolved by the warmer water acting upon the fragments in the shape of little columns and pyramids such as Col. Totten saw strewed along the shore of Lake Champlain.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Platinum and the metals which accompany it.*—H. St. Claire Deville and Debray have published a very interesting and valuable memoir on the platinum metals, considering the subject rather from a metallurgical than from a purely chemical point of view. For the details of the processes employed we must refer to the original memoir, which rarely admits of abridgment, and which is in the highest degree instructive. The authors employ exclusively the *dry* method of investigation and operate at temperatures much higher than any which have hitherto been obtained upon a working scale. By a new arrangement of the oxy-hydrogen blowpipe most of the members of the platinum group may be fused—platinum even in larger quantities than was accomplished by Dr. Hare. By the same apparatus properly employed, the authors purify the metals and their alloys from more volatile elements with which they may be mixed. Osmium has a density of 21·3 to 21·4, and when dissolved in tin exhibits traces of a crystalline structure. It is bluish white, has no odor, and gives off vapors of osmic acid only above the temperature of melting zinc. At the temperature of melting ruthenium, osmium is sensibly volatilized but it does not fuse, and hence resembles arsenic in having its boiling point lower than its point of fusion. Two determinations of the density of the vapor of osmic acid gave 8·88 and 8·89, corresponding to 2 vols. Next to osmium ruthenium is most difficult of fusion, but may yet be obtained in small fused masses when its density is from 11 to 11·4. The authors give analyses of the protoxyd of ruthenium and of the crystallized deutoxyd isomorphous with stannic acid. They also describe a beautiful alloy of ruthenium and tin having the formula $RuSn_2$ and crystallizing in cubes. Palladium fuses even more readily than platinum, and volatilizes at the temperature at which iridium melts. It also absorbs oxygen when fused like silver without becoming oxydized. Its density at 22·5° is 11·4. With tin it forms an alloy crystallizing in small brilliant plates having the formula Sn_2Pd_3. Rhodium fuses less easily than platinum; it has about the color of aluminum and when pure is malleable and ductile; its density is 12·1. It forms crystalline alloys with zinc and tin.

Platinum may (as first shown by Dr. Hare) be fused in large quantities before the oxy-hydrogen blowpipe. Deville and Debray give a detailed description of the apparatus by which this metal may be fused in quantities of not less than 12 to 15 kilograms at an expense of from 0·24 fr. to 0·40 fr. per kilogram. (The late Dr. Hare fused 28 ounces at one operation.) The fusion of platinum is best accomplished in crucibles of lime, which serve to refine the metal by absorbing the impurities. When fused and refined, platinum is as soft as copper; it is whiter than ordinary platinum and free from porosity; its density is 21·15. With tin platinum forms a beautiful crystallized alloy having the formula Pt_2Sn_3. Iridium requires an extremely high temperature for its fusion, but when fused has a pure white color, and is brittle under the hammer like a crystalline metal; its density is the same as that of platinum, namely 21·15.

(Dr. Hare, who long ago succeeded in fusing iridium, found its density 21·83.) Iridium forms with tin a beautiful alloy crystallizing in cubes having the formula $IrSn_2$.

The authors remark that the alloys of platinum with iridium and rhodium are much more valuable in the arts than pure platinum, many of them resisting the action of aqua-regia, and possessing a considerable degree of hardness and rigidity. The memoir contains in addition numerous elaborate analyses of different specimens of platinum ore and of osmiridium, as well as new processes for the treatment of platinum ores upon the larger scale, the preparation of pure platinum for industrial purposes, and of alloys of platinum with its associate metals possessing useful properties. For these we must refer to the original.—*Ann. de Chimie et de Physique*, lvi, 385, Aug. 1857.

[*Note.*—A memoir read before the Am. Association for the Advancement of Science at its meeting in August 1859, and shortly to appear in the 12th volume of the Transactions of the Smithsonian Institution, contains entirely new processes for the separation of all the platinum metals in a state of absolute purity. These processes are in the wet way; they are very simple and easy of execution, and they not only apply to the separation but to the qualitative analysis of mixtures of the different metals of this group in almost any proportions. The methods in question involve the preparation and properties of a new and remarkable series of salts, and will I hope be found to remove completely the difficulties which have hitherto surrounded the subject.—W. G.]

2. *Blowpipe experiments.*—Bunsen has contributed some very interesting additions to our knowledge of the use of the blowpipe in quantitative as well as qualitative analysis. The author employs the peculiar form of gas burner, first introduced by him, and now used in all laboratories, instead of the blowpipe. The lower part of the flame is surrounded by a conical sheet iron chimney, 30^{mm}. in diameter above, and 55^{mm}. below, so that the burner tube is in the axis of the chimney and 45^{mm}. below the upper base of the cone. The cock is to be regulated so that the apex of the inner non-luminous cone of gas within the flame exactly reaches the level of the upper base of the chimney. In this manner we obtain a flame of perfectly constant dimensions which is immovable, sharply defined in all its parts, and which may always be obtained of uniform character. The outer cone of flame has a very faint sky-blue color, which is invisible even by feeble daylight. The inner cone of flame is less intensely blue than the outer. The object to be submitted to the action of the flame must never be larger than the half or the third of a grain of millet seed. It is to be introduced into the flame by means of a little loop on the end of a platinum wire which is attached to a holder by which it may be moved gently and steadily, so that the object may be introduced into any part of the flame. The loop is to be moistened with water, when a grain or a little of the powdered substance will readily adhere to it.

The author remarks that the temperature which the flame is capable of producing depends principally upon the constitution of the gas consumed. The temperatures corresponding to gas analyzed in the Heidelberg laboratory on four different occasions were 2369 C., 2352 C., 2391

C., 2386 C., or as a mean, 2350 C., so that the temperature of the flame where the quantity of air is exactly sufficient for the combustion of the gas, may be assumed in round numbers as 2300 C. It is easy to see, however, that the temperature will vary in different parts of the flame. The author gives a simple and elegant method of determining the point of maximum temperature by introducing a platinum wire into the flame, and determining at what point the light emitted by this is most intense. In this manner it is found that the zone of maximum temperature lies in the external cone of flame, a few millimetres above and below the apex of the internal non-luminous cone, which is on a level with the upper base of the chimney. The author employs this zone to investigate the action of a temperature of 2300 C. upon different substances, and terms it the *melting space.* The outer border of this *melting space* acts as an oxydizing flame, the inner as a reducing flame, the reduction being most powerful immediately above the apex of the innermost cone. The great constancy which the flame exhibits in all its parts allows us to observe and estimate the volatility of substances at the very high temperature of 2300 C. For this purpose a mass of matter having a measured diameter of 1 millimeter is introduced into the flame, and the time required for complete evaporation determined by means of a seconds pendulum or a metronome. The size of the mass introduced upon the platinum wire is easily regulated by adding new substance or by evaporation in the flame, and may be measured under the microscope. The author takes the volatility of carbonate of soda as unity, and gives a table of the comparative volatility of different substances in terms of this unit: thus the volatility of chlorid of potassium is 15·33; of chlorid of sodium 6·57; of phosphoric acid 23·00. Other substances are more or less completely decomposed at the temperature of 2300 C.

Besides its use in experiments on volatility the flame may also be employed for a series of other very valuable blowpipe reactions, among which the author cites the quantitative determination of soda in the presence of potash and lithia.

For the simple recognition of soda in its volatile salts, it is sufficient to introduce a small bead of the substance into the melting space and then to illuminate a crystal of bicromate of potash with the light produced. The salt appears perfectly colorless, transparent, and with a diamond lustre so long as the rays of the soda flame fall upon it, and this even by ordinary lamplight or daylight. A still more delicate reaction is obtained by using paper, covered with iodid of mercury, a square centimeter of which may be attached to the chimney in front of the flame by a movable arm. If we introduce the smallest quantity of a soda compound into the melting space, the red paper appears white, with a faint tinge of tawny yellow. When the soda salt is in solution the loop of the fine platinum wire may be flattened under the hammer to a little ring. This ring introduced into the liquid will take up a drop which must be gently evaporated to dryness and then tested as before. In this manner $\frac{4}{10,000}$ of a milligram of common salt may be easily detected.

Volatile potash compounds, as is well known, communicate a bluish violet tint which is completely concealed by small quantities of soda. In this case the potash may easily be detected by means of Cartmell's

reaction, that is, by looking at the flame through a deep blue cobalt glass, when a violet or ponceau-red color appears. This potash test is even more delicate than that for soda, $\frac{2}{10,000}$ parts of chlorid of potassium may be detected with perfect distinctness.

The detection of lithia in the presence of potash and soda is best effected by looking at the flame through a hollow prism filled with a solution of sulphate of indigo. The carmine-red color of the lithia flame disappears when a certain thickness of solution is reached, and if a mark be put upon the prism, all the layers of liquid above the mark will allow only the red potash-rays to pass through. Lime and soda have no influence on this reaction. When potash and lithia are both present the flame from the salt to be tested should be compared with that of pure potash. The flame containing lithia and potash appears through thin layers of liquid redder than the potash flame alone; through thicker layers the potash flame appears scarcely weakened. In this manner some thousandths of lithia may the discovered in potash salts.

To render these processes available for silicates Bunsen mixes the mineral with pure gypsum and heats in the melting space, when silicate of lime and volatile sulphate of potash are formed. By comparing the intensity of the color produced by a mineral to be tested with that of a series of silicates whose percentages of alkali are known, it is easy even with very small fragments to determine the relative quantities of potash, soda and lithia with tolerable approximation.

The author in the first place determines whether the mineral to be examined contains lithia or not by the method already explained. The minerals of the first group are those containing no lithia, and the author arranges a scale of minerals for comparison according to the content of soda in each. These are—

	NaO	KO
1. Lazulite,	9·09	—
2. Nepheline,	15·44	4·94
3. Albite,	10·06	—
4. Orthoclase,	7·08	7·03
5. Sanidin,	4·0	8·0
6. Labradorite,	2·55	1·06
7. Anorthite,	1·13	0·62
8. Leucite,	—	2·2

These are to be ignited, pulverized and preserved with their numbers as blowpipe reagents. One of these and the mineral to be tested, with or without gypsum, are to be introduced into the flame together so that small equal ends of wire are ignited; the iodid of mercury paper then appears more or less bleached. Remove the test from the flame, if now the paper shows a reddish tint, the test contains more soda than the mineral used for comparison. If the paper however becomes paler the contrary is the case. In this manner it is easy to determine between what two minerals the test lies, so that the percentage of soda may be estimated within a few per cent. The substances to be compared must be as nearly as possible in equal quantities, the ignited lengths of wire be the same and the soda-flame of the same size and form. The eye must be accustomed to distinguish the different degrees of brightness of the same tint

from actual differences of tint. When the iodid paper is intensely bleached it may also be illuminated by a candle flame so that the soda-flame produces with this foreign light a white tint nearer to red.

The potash color-test is not as accurate quantitatively as that for soda. It is sufficient for all purposes to distinguish *a slight*, *a strong*, and a *very strong* potash reaction, using for comparison in succession the flame of oligoclase, orthoclase and leucite heated with gypsum in the same flame with the test. The indigo prism is to be used and the dimensions, color and duration of the red flames observed.

Lazulite gives a stronger soda reaction than nepheline because it contains sulphuric acid, and it is always necessary to determine beforehand whether the test contains sulphuric acid, chlorine or fluorine. This is best accomplished by the common blowpipe.

For further details we must refer to the original memoir, which must create an entirely new department in blowpipe analysis.—*Ann. der Chemie und Pharm.*, cxi, 257.

[*Note.*—It is easy to see that Bunsen's memoir contains the solution of many chemical and physical problems of great interest. Thus it is easy to produce at will a flame which shall have any required temperature, at least between certain limits. Since according to Bunsen's calculations (Gasometrische Analyse, p. 254) the flame of hydrogen burning freely in the air has a temperature of 3259° C., and that of olefiant gas has a temperature of 5413° C., we may as readily experiment at these temperatures as at 2300° C. Very much higher temperatures are of course produced when these gases burn with pure oxygen—in the case of hydrogen 8061° C.; in that of olefiant gas 9187° C. Hence by burning mixtures of these gases with oxygen and varying the proportion of nitrogen arbitrarily, we can make a scale of flames the temperatures of which shall range from less than 2000° C. up to at least 9000° C. and the melting point as well as degree of volatility of almost all metals and mineral substances may be thus assigned within quite narrow limits. It is easy to calculate the percentage of nitrogen or carbonic acid to be added in each case to the combustible gas in order to produce the required temperature. It appears probable that at high temperatures the radiating power of a body for heat is proportional to its radiating power for light. By directly comparing the intensity of the light radiated from platinum heated in a furnace with the intensity of the light radiated when the platinum is heated to a known temperature in a gas-flame, the temperature of the furnace might be approximately determined. Interesting results could also be obtained as to the exact temperature at which bodies become luminous and as to the relative quantities of light which different substances emit when heated to the same temperature, or the same substance at different temperatures. With respect to Bunsen's scale or series of minerals containing different but known proportions of soda I will suggest that perhaps a series of *glasses* could be made containing perfectly definite quantities of either soda or potash from 2 up to 30 per cent, so that each number would contain 2 per cent more alkali that the next lowest number. These would then serve as universal standards of comparison and give much precision to blowpipe observations.—W. G.] W. G.

II. GEOLOGY.

1. *Review.—On some points in the Geology of the Alps.**—For nearly a century the great problem of the structure of the Alps has engaged the attention of geologists, and although much still remains to be done, we are gradually attaining to a clearer notion of the age and the mode of formation of this great mountain region. Among the most remarkable conclusions deducible from modern investigations is that of its very recent upheaval, and the age, geologically modern, of a large portion of its rocks. The great mass of nummulitic strata, with the overlying series of slates, limestones, marls, and fucoidal sandstones (once regarded as of transition age), which constitute the *flysch*, and belong to the eocene division of the tertiary rocks, has been upheaved vertically more than two miles, and penetrated by intrusive granites. Then took place the great movements of folding, inversion, and lateral and vertical displacement, the slow sinking of the northern side of the chain permitting the accumulation of the conglomerate of the *molasse* or the *nagelflue* (a formation partly freshwater and partly marine), which belongs to the miocene, and in part perhaps to the pliocene, and attains in some parts a thickness of from 6000 to 8000 feet. After the subsidence which allowed this vast accumulation, we find it raised by a vertical displacement of many thousand feet, and dipping towards the older rocks, so that the upper beds of the *nagelflue* seem to pass beneath the nummulitic limestones. (Murchison, *Quar. Jour. Geol. Soc.*, v, 241.) These great foldings and dislocations have often given rise to lateral movements by which the older strata have actually been forced over and made to rest unconformably upon younger rocks.

The Messrs. Rogers have shown that these great movements of the Alps present a close resemblance with those observed by them in the Alleghanies, and we are persuaded that a farther study of our older mountain chains, rendered more easy of investigation from the smaller number of formations involved, and from the regions being less elevated and more accessible, will throw still farther light upon the structure of the Alps and of mountain chains in general.

The vast thickness of the sedimentary deposits of secondary and tertiary age over the area now occupied by the Alps, when compared with their volume in other parts of Europe, serves to exemplify the relation which Prof. James Hall has so well pointed out, of the apparent connection of mountain elevations with original deposition (*Geology of Iowa*, i, 41). At the same time the weight of evidence now tends to show that the crystalline nucleus of the Alps, so far from being an extruded mass of so-called primitive rock, is really an altered sedimentary deposit more recent than many of the fossiliferous strata upon their flanks, so that the Alps as a whole have a general synclinal structure.

In the memoir before us the learned professor of geology in the Academy of Geneva, already well known by his researches in Alpine geology, has devoted himself to the study of a number of sections which exhibit

* *Memoire sur les terrains liassique et keuperien de la Savoie, par* Alphonse Favre. (Extrait du tome xv *des Memoires de la Société de physique et d'histoire naturelle de Genève*, 1859. Quarto, pp. 92, with three plates.)

the anthracitic and jurassic formations of Savoy. In these sections which he has given with lists of their organic remains, he includes (1) under the general name of the jurassic system both the oolitic or jurassic proper and the lias. Beneath these he distinguishes (2) a succession of limestones, chiefly dolomitic (*cargneules*), with interstratified beds of gypsum, underlaid by red and green marls and a silicious sandstone or arkose, the whole series, which attains a thickness of 2500 feet, being destitute of fossils, and reposing upon (3) shales and sandstones which enclose beds of anthracite and plants of the carboniferous epoch. Succeeding these comes the conglomerate of Valorsine (4), which, with the preceding group, he refers to the carboniferous system, while beneath all these are (5) reddish crystalline schists and gneiss, passing into a rose-colored protogine or talcose granite.

The third division is regarded by Favre as representing the *keuper* or triassic system, which, although known in the Tyrol and in Lombardy, has not hitherto been recognized in Savoy. Its identification is regarded by our author as highly important, inasmuch as it establishes a well defined geological horizon throughout this region. To the triassic system he refers the gypsums which are found encircling Mt. Blanc, and the salt deposits of Bex, of the Tarentaise, and elsewhere. Favre however recognizes the existence of more recent gypsums, with *cargneules*, in a fucoidal sandstone overlying the nummulitic limestone. The *cargneule*, which is here a constant associate of the gypsum, is according to the recent analyses of Marignac (p. 11) a cellular limestone or tufa containing about nine per cent of carbonate of magnesia, while the cells are filled with a nearly pure pulverulent dolomite. Massive, compact and crystalline dolomites are however frequently associated with these cargneules, which Haidinger conceives to have been formed by the alteration of dolomites under the influence of a solution of gypsum. We are however inclined to regard the cargneules as analogous to the tufas which we have elsewhere described as often associated with true dolomites. (This Journal, xxviii, 372.) To this subject we propose soon to return, but we take occasion to correct an error into which Mr. Favre has fallen in attributing to Sir William Logan the view that magnesian limestones owe their origin to the action of solutions of carbonate of soda upon sea-water (p. 42). This has arisen from an oversight in quoting from the *Literary Gazette*, where some observations made by us at the meeting of the American Association at Montreal in August, 1857, are reported in immediate connection with a paper of Sir W. E. Logan. We have endeavored to show that carbonate of magnesia has been formed in nature from the decomposition of soluble magnesian salts either by solutions of bicarbonate of soda or bicarbonate of lime, in the latter case sulphate of magnesia being concerned and sulphate of lime being also a product. (*Comptes Rendus de l'Acad.*, xlviii, 1003, and this Journal, xxviii, 170.)

Mr. Favre has not endeavored to identify the Permian system in this region, although according to Pidancet it occurs with its characteristic flora, beneath the variegated sandstones of the trias, in the adjacent department of the Jura. (*Bul. Soc. Geol. de Fr.*, [2], xii, 149.) The trias, according to our author, rests directly upon the shales and sandstones which are regarded by him as belonging to the carboniferous system, and

at other times upon crystalline schists, which however he suggests may be no other than the carboniferous rocks so highly altered as not to be distinguished from the older crystalline schists.

In the view which he takes of the relations of the jurassic and anthracitic rocks of the Alps, Mr. Favre is so completely at variance with most of those who have preceded him that it will be necessary to enter into some details with regard to this so-called anthracitic formation, which has been hitherto regarded as intercalated with and forming a portion of the jurassic system of the Alps. The conglomerate of Valorsine, which Favre classes with the palæozoic series, is according to Sismonda, no other than the *verrucano* of Savi, and the lowest member of the liassic series, beneath which at Jano in Tuscany is an anthracitic formation associated with a strictly palæozoic fauna. (Ibid, p. 635.) In various localities in Savoy, on the contrary, and particularly at Petit-Cœur in the Tarentaise and near Briançon, the anthracitic beds, consisting of sandstones and shales with impressions of ferns and layers of anthracite, appear to be intercalated in the jurassic system. This fact, announced by Elie de Beaumont in 1828, has been confirmed by a great number of subsequent observers, among which may be mentioned Sismonda, Murchison, Gras and Rozet. The fossil plants of this formation were by Brongniart and subsequently by Heer identified with those of the carboniferous system; according to Mr. Bunbury, who has since examined the fossils of Petit-Cœur, which are very much distorted and replaced by a film of talcose matter, their identification is difficult, and although some are undoubtedly carboniferous species, others are doubtful. (*Geol. Journ.*, v, xxix, 130.) Elie de Beaumont, with Murchison and Rozet, conceives that the evidence of the age of a formation furnished by fossil plants, to be far less reliable than that afforded by its fauna, and the latter imagines that during the jurassic period the climate may have permitted the development, upon islets in the midst of the sea, of a flora like that of the coal period. The observations of Elie de Beaumont in 1828 led him to refer the ferns and anthracites of Petit-Cœur as well as those of the neighborhood of Briançon to the summit of the lias. Both Gras and Sismonda distinctly recognize two horizons of anthracitic beds with fossil ferns, the one at the summit and the other at the base of the great anthracitic system; but while Sismonda (grounding himself upon the inferiority of the true coal formation to the conglomerate of Valorsine,) agrees with Elie de Beaumont and Murchison in regarding this system as really of jurassic age, although including a flora like that of the coal, Gras is inclined to look upon it rather as a formation of carboniferous age, with a liassic fauna, which has not however afforded a single palæozoic species.

Notwithstanding the concurrent evidence of so many observers to the fact of the intercalation of the anthracitic beds over a large area, with liassic fossils, there have not been wanting those who have endeavored to explain this association by supposed foldings, which may have involved the liassic beds with others belonging to an underlying carboniferous formation. This explanation, long since suggested by Voltz, was afterwards brought forward by Favre in 1841, since which time it has been discussed by various geologists, although generally with little favor. In

the present memoir our author examines the section of Petit-Cœur, and finds on one side, intercalated between the beds with belemnites and those containing ferns, a mass of cargneule which he considers the representative of the triassic system, separating the carboniferous and liassic series; the cargneule has not however been detected on the other side of the supposed fold, where the belemnite beds are in direct contact with the anthracite series. It might farther be objected that the mass of cargneule, the usual associate of gypsum, is not alone sufficient proof of the existence of the triassic system, since, according to Gras, gypsums accompanied by yellow altered limestones (in one case declared to be dolomite,) occur at two distinct horizons in the anthracitic series.

The observations of Mortillet on the section of Petit-Cœur are important; he not only concludes from his examinations that it is impossible to admit the existence of a fold in the strata at this locality, but asserts that the belemnites which underlie the fossil plants are different from those above. In the lower beds there is found but a single species, the *Belemnites acutus* (Miller), which as well as an associated ammonite, *A. bisulcatus*, belongs to the lower lias. In the beds above the coal fossils, on the contrary, there are several species of belemnites, not readily identified, but differing in form from that just mentioned, and apparently belonging to the middle or upper lias. He farther cites the existence in the same specimen of a belemnite and a fossil fern. (*Bull. Soc. Geol. de France*, [2], x, 18.)*

The testimony of Sir Roderick Murchison, after a minute examination of the section at Petit Cœur, is also most decisive as to the impossibility of admitting any folding or inversion of the strata, and he confirms the opinion of Élie de Beaumont and Sismonda as to the liassic age of the coal flora in question. As regards the vertical range of certain fossil plants he refers to a species of *Calamites* which extends from the carboniferous series through the permian and trias, and an *Equisetum* which is common in the trias of Germany and yet abounds in the oolite of England. (*Geol. Journal*, v, 178.)

In the Alps of Lombardy, Omboni has described a succession of rocks similar to that given by Favre; beneath the lias, according to him, are red and green marls with limestones containing the fossils of the muschelkalk, underlaid by red and green sandstones, the whole being referred to the triassic system. Lower still a series of limestones, dolomites and sandstones underlaid by black shales, the whole without fossils, and resting upon crystalline schists and protogines, is referred to the permian and carboniferous formations. These sandstones he considers as identical with the verrucano, which according to him contains in Tuscany the fossils of the coal period. (*Bull. Soc. Geol. de France*, [2], xii, 517.) We have already seen that Sismonda regards the verrucano as belonging to the base of the lias, and although associated with the anomalous liassic flora, as overlying the true palæozoic coal measures.

* For fuller details see Gaudry's sketch of the various papers on this subject, *Bull. Soc. Geol. de France*, [2], xii, 580 and 637. Sismonda, Ibid., p. 631. Rozet, *Memoire sur les Alpes Françaises*, ibid., 204; Gras, *Sur le terrain anthracifère des Alpes*, *Ann. des Mines*, [5], v, 475, and d'Archiac *Histoire de la Géologie*, vii, 107–157. Also Lyell, *Geol. Journal*, 1849, p. xxxviii, and Portlock, ibid., 1856, p. lxvii.

Connected with the view which supposes the continuation of a palæozoic flora up to the epoch of the lias is a question raised by our author (p. 29) regarding the association (established by comparison of considerable lists of species from various localities), of the fossils of the different stages of the lias in a single bed. Analogous cases are frequently met with at different geological horizons, showing that the causes which have limited the vertical range of certain animals are so far local, that under somewhat modified conditions the duration of a species may be prolonged after its disappearance from the adjacent seas. As our author has well said, it is in the continuation of similar chemical and physical conditions, generally dependant upon local accidents of the surface, that we must look for the cause of these apparent anomalies in the distribution of fossils. The investigations of Prof. Safford in Tennessee and Sir William Logan in Canada have made known a remarkable example of this in the Lower Silurian series of North America. In the State of New York the fauna of the Black River and Trenton limestones are almost totally distinct, so that the division between the two formations constitutes a well-marked palæontological horizon, while in Canada and in Tennessee the fossils of the two formations are so completely intermingled that it is impossible to distinguish between the Black River and Trenton limestones. (Billings, *Report Geol. Survey of Canada*, 1857, p. 152.) The genus *Catenipora* in North America also presents a remarkable instance of anomalous distribution; this coral throughout New York and Western Canada is Upper Silurian and is unknown below the Clinton Group, while at Lake St. John on the Saguenay it is abundant in the Black River limestone, a position which corresponds with its geological horizon in Europe.

The anthracitic system of the Alps has been described by Gras with great minuteness in the memoir cited above. He estimates the total thickness of the system at between 25,000 and 28,000 feet, and divides it into an upper and lower series, which are unconformable. The former with a thickness of about one-tenth of the whole, is referred to four subdivisions, and consists chiefly of argillo-calcareous shales terminated by a considerable mass of limestone, immediately beneath which occurs a bed of spilite, associated with gypsum and a yellow altered dolomite. The inferior division also consists in great part of similar shales, interstratified with gneissoid and talcose rocks to be noticed farther on; gypsum and a yellow altered limestone (dolomite?) also occur in this portion. In both the upper and lower divisions sandstones and shales are found with coal plants and layers of anthracite, the latter sometimes changed into graphite, while the associated calcareous shales contain in various localities, and at different horizons, belemnites and ammonites of liassic age.

Sismonda confirms the accuracy of the observations of Gras, and admits an upper anthracitic series, resting unconformably upon the lower, and of oolitic age, the lower only being referred by him to the lias proper, while Studer concludes the discussion of the subject as follows: "The intercalation of the lower anthracitic slates sometimes with gneissoid and talcose schists, and sometimes with the belemnitic shales, the layer of jurassic limestone which separates the lower from the upper anthracites, the extraordinary thickness of this calcareous layer and the anthracitic

zone which covers it, the presence of the verrucano (or talco-quartzose conglomerate) between these two formations and the quantity of talc disseminated in all these rocks, present us problems which science is not able to solve completely."—(Studer, cited by Laugel, *Bull. Soc. Geol. de France*, [2], xii, 576.)

In the metamorphic rocks of the anthracitic system Gras distinguishes what he calls a protogine or talcose formation, consisting of granite, gneiss and mica slates, generally more or less talcose, containing also schistose diorites, eurites and leptynites. This protogine formation is intimately associated with the argillo-calcareous shales of the lower anthracitic series, which are found interstratified with and even passing into gneiss and other feldspathic schists. Gras concludes that this somewhat irregular interpenetration of the two classes of rocks is due to an irregular metamorphism, portions of the argillo-calcareous sediments having, as he supposes, been profoundly altered by emanations from below, and he hence regards the protogine formation as a portion of the inferior anthracitic series. Similar crystalline schists also occur in some parts of the upper anthracitic series, and in both the upper and lower there are found serpentines, euphotides, porphyries and spilites, all of which the author regards as of sedimentary origin, and as having undergone *in situ* a profound metamorphism which has often effaced the marks of stratification; this view he declares is the only one which appears to agree with the observed facts. This protest against the theory of the igneous origin of serpentines is in accordance with the results obtained by the Geological Survey of Canada; in his report for 1848 Sir William Logan insisted upon the sedimentary origin of the serpentines which occur in the altered palæozoic strata of the Green Mountains.

Rozet, in his investigations of the Alps, has referred to the liassic and jurassic periods the great system of gneiss, with micaceous and talcose schists, which makes up Mts. Cenis and Pelvoux, and a great part of the mountains of the frontier of Piedmont, while, according to Fournet, the jurassic rocks of the Valais have been altered in like manner. (Coquand, *des Roches*, pp. 300–301.)

Our author however supposes that all these altered strata are of carboniferous age, and remarks that the fossils in the limestone of the lower lias show but slight marks of metamorphism, from whence he concludes that "in this region of the Alps there are no highly metamorphosed jurassic rocks," and that the metamorphic action "which took place beneath the sea before the elevation of these mountains" ceased to be powerful after the deposition of the Valorsine conglomerate, (which he regards as of carboniferous age,) the paste of which is often converted into a crystalline talcose schist. At the same, although the liassic and so-called triassic rocks are comparatively unaltered, the jurassic shales are described as more or less talcose and greasy to the touch, but the alterations of these argillaceous rocks our author regards "as rather mechanical than chemical." (p. 76.)

The question before us is then, whether in that series of rocks which embraces liassic and jurassic beds with gypsum, dolomite, anthracite, and a carboniferous flora, and which geologists have generally referred to one great system not older than the lias, we have really in these mountains,

as Favre supposes, the whole succession of formations from the oolite to the carboniferous inclusive, so involved by foldings and inversions that it has hitherto been impossible to determine their real structure.

But even if we admit with Favre the palæozoic age of the protogine formation described by Gras and Rozet, we cannot agree with him in limiting to the rocks of that period the action of the metamorphic process. The development of a talcose character in the jurassic shales we cannot regard as the result of a mechanical process, and we have besides evidences in the Alps of the metamorphism of still higher rocks.

As early as the year 1834 Keferstein had asserted that the granites of Mont Blanc are nothing more than altered strata of *flysch*, (*Naturgeschichte des Erdkörpers*, i, 286–292; *Bul. Geol. Soc. de France*, [1], vii, 198,) and in 1850 Sir Charles Lyell in his address before the Geological Society of London suggested that the protogines of the Alps might be of tertiary age. This is so far true that both Studer and Murchison have shown that portions of the eocene flysch have been converted into crystalline gneiss, mica slate and even granitic beds; Murchison, like Gras, whom we have already cited, remarks that the metamorphism seems irregular, some bands of the rock being apparently much more altered than others. (*Geol. Journal*, v, 164, 210.) The intercalation of wedge-shaped masses of fossiliferous limestone of liassic age among the gneissic strata of the Alps has been well described by Studer (*Bul. Soc. Geol. de France*, [2], iv, 208), and similar phenomena observed in various other metamorphic regions present a problem the right understanding of which is most important in its relations to the theory of metamorphism, and one which we propose to consider at an early day.

In conclusion we hasten to say, that although dissenting from some of the views of Mr. Favre, we are not less grateful for his very suggestive memoir, which with its carefully prepared lists of fossils and its beautiful sections is a valuable contribution to alpine geology.

T. Sterry Hunt.

2. *The Geological Structure of the "Jornada del Muerto," New Mexico, being an abstract from the Geological Report of the Expedition under Capt. John Pope, U. S. Top. Eng., for boring Artesian Wells along the line of the 32d parallel;* by G. G. Shumard, M.D., Geologist of the Expedition, (Trans. Acad. Sci. St. Louis, vol. i, No. 3, p. 341. 1859).—This paper gives a description of the geological structure of a district of country lying immediately east of the Rio Grande, and between the 32d and 34th parallels. The "Jornada del Muerto" is described as a gently sloping plane, of an elliptic form, from twelve to forty miles in breadth, and extending from near the southern extremity of the Doña Ana Mountains eighty or ninety miles in a N.N.W. direction. It is bounded on the east and west by ranges of mountains, varying in their elevation from two or three hundred to one thousand feet above the plane, and seem to be mainly composed of dark colored limestones of Upper Carboniferous age, dipping towards the interior of the intermediate plane; these rocks however, in the western range, were observed at some places, surmounted by shales and sandstones referred to the Cretaceous epoch. Igneous rocks form a belt of low hills along the eastern side of the eastern range, and also occur between the other and the Rio Grande. From the structure of

the surrounding country, the "Jornada" is supposed to be a great synclinal depression, in which water could probably be obtained by artesian borings, through heavy detrital deposits.

The fossils found in the Carboniferous rocks here, and in the Organ Mountains, are all Upper Carboniferous types, many of them being identical with species almost everywhere common in the western Coal Measures. Those mentioned in the Cretaceous beds are *Inoceramus* and *Cardium*. The paper also contains interesting local details in regard to the igneous and metamorphic rocks of the several mountain ranges explored. M.

3. *Notice of Fossils from the Permian strata of Texas and New Mexico, obtained by the United States Expedition under Capt. John Pope, for boring Artesian Wells along the 32d parallel, with descriptions of new Species from these strata and the Coal Measures of that region;* by B. F. SHUMARD, M.D., (Trans. Acad. Sci. St. Louis, vol. i, Part 3d. 1859).—This is an important paper, containing descriptions of many new species, with an enumeration of others identified with forms known in the Kansas rocks, and of a few which are, by the author, supposed to be identical with foreign species; it is also illustrated by an excellent plate of twenty-seven figures by Leopold Gast & Brother of St. Louis.

Most of these fossils are from an extensive deposit of white limestone, and inferior beds of sandstone and darker colored limestone, in the Guadalupe Mountains, referred by Dr. S. to the Permian System. The new species described from these rocks are—

Campophyllum? Texanum, Chonetes Permiana, Spirifer Guadalupensis, Terebratula perinflata, Rhynchonella indentata, R. Texana, Camerophoria Swalloviana, Crania Permiana, Axinus securis, Turbo Guadalupensis, Pleurotomaria Halliana, and *Chemnitzia Swalloviana.* Those from the beds regarded as Carboniferous, are *Turbo Texanus, Straparollus cornudanus, Pleurotomaria Proutiana, P. obtusipira, P. perornata* and *Machrocheilus Texanus.*

All of which appear to be described with the author's well known care and accuracy.

Dr. S. had previously described from the beds he places in the Permian,

Phillipsia perannulata, Fusulina elongata, Productus Mexicanus, P. pileolus, P. Popei, Strophalosia (Aulosteges) Guadalupensis, Spirifer Mexicanus, S. sulciferus, Spirifcrina Billingsii, Terebratula perinflata, Rhynchonella Guadalupæ, Camerophoria bisulcata, Retzia papilata, R. Meekiana, and *Myalina recta,*—

several of which are well illustrated in the plate accompanying the paper now under consideration. He also gives the following list of forms from these rocks, regarded by him, with more or less confidence, as identical with species occurring in the Permian and Upper Carboniferous beds of Kansas, viz:

Acanthocladia Americana, Productus Calhounianus, P. Norwoodii, Spirifer cameratus, Streptorhynchus (Orthisina) Shumardianus, Edmondia suborbiculata, and *Pleuraphorus occidentalis;* while he thinks he recognizes the following foreign species in the same association:—*Chœtetes Mackrothii, Productus semireticulatus,* var. *antiquatus, P. Leplayi?, Terebratula elongata, Camerophoria Schlotheimi?, Myalina squamosa, Monotis Speluncaria,* and *Turbo helicinus?.*

As Dr. S. finds a *Phillipsia* and a *Fusulina*, in these rocks, neither of which genera are known to range up into Permian beds in the old world, and *Spirifer cameratus* is a characteristic Coal-measure species, from Pennsylvania to the Rocky Mountains; while *Productus semireticulatus,*

is regarded by most authors as peculiar to the Carboniferous system—and a large proportion of the other species mentioned as common to the New Mexican and Kansas rocks, are known to occur in the latter territory in beds containing even a majority of well marked Coal-measure species, we may infer that in New Mexico, as in Kansas, there is a considerable blending of Carboniferous and Permian types; so that it becomes a matter of doubt and difficulty to determine at what particular horizon the line of demarkation should be drawn between these two Systems, if indeed there is any such natural break in our upper Palæozoic series of this country. It is to be hoped Dr. S. will continue his investigations of the fossils occurring in these formations, which he will doubtless have an opportunity to do, in connection with the geological survey of Texas, under his charge. M.

4. *Observations on the Geology of the County of Ste. Geneviève, being an extract from the Report made to the Missouri Geological Survey, in* 1859; by B. F. SHUMARD, M.D., (Trans. St. Louis Acad. Sci., vol. i, part 3, page 404, 1859.)—In addition to information respecting the iron and lead mines, building materials, &c., of the county, this extract contains some facts having an important bearing on mooted points in the classification of the Lower Carboniferous Series of the West. Dr. S. found this series to be composed of the following members, in the descending order:

1st. THE UPPER ARCHIMEDES LIMESTONE, characterized by *Pentremites pyriformis, P. sulcatus, Agassizocrinus dactyliformis, Spiriferina spinosa, Spirifer trigonalis*, and species of *Archimedipora.*

2d. THE FERRUGINOUS SANDSTONE, in which no fossils were found,—estimated thickness, 80 to 100 feet.

3d. THE STE. GENEVIEVE LIMESTONE, a second Archimedes bed, in which the following fossils were found:—*Rhynchonella trimela, R. Wortheni, Spirifera hirsuta, Retzia Marcyi, Spiriferina spinosa, Spirifera Leidyi, Productus elegans, P. bisulcatus, Murchisonia vermicula, Pentremites florealis*, and one or more species of *Archimedipora.*

4th. THE SAINT LOUIS LIMESTONE—containing *Lithostrotion mammillaria, Archæocidaris* and *Pentremites conoides*,—thickness 100 feet or more.

5th. THE THIRD ARCHIMEDES LIMESTONE, containing *Pentremites laterniformis, P. conoides, Archimedipora, Dichocrinus simplex, Spirigera hirsuta, Productus Indianensis, Rhynchonella subcuneata*, and *Holopea Prouti*,—thickness from 100 to 150 feet.

6th. THE ENCRINITAL LIMESTONE (= BURLINGTON LIMESTONE), with its usual fossils—being the lowest member of the great Carboniferous series.

Of the Devonian rocks he recognizes the CHEMUNG GROUP, HAMILTON GROUP, and the ORISKANY SANDSTONE. Of the Silurian—1st. LOWER HELDERBERG SERIES, 2d. The NIAGARA GROUP, 3d. The HUDSON RIVER GROUP, 4th. The RECEPTACULITE LIMESTONE, 5th. The TRENTON LIMESTONE, 6th. The BLACKRIVER and BIRDSEYE LIMESTONES. Then comes the five members of the GREAT MAGNESIAN LIMESTONE SERIES, which represents the Calciferous, and possibly portions of the Potsdam and Chazy Limestones of New York; and last of all, eruptive rocks.

Of the Coal measures, only thin outliers cap the hills half a mile above St. Mary's on the Mississippi. The beds are alternations of shale and sandstone, surmounted by a thin bed of hard siliceous limestone. M.

5. *Third Series of Descriptions of Bryozoa, from the Palæozoic Rocks of the Western States and Territories;* by H. A. PROUT, (Trans. Acad. Sci. St. Louis, vol. i, part 3, p. 443, 1859.)—Every geologist who has

worked amongst western rocks, must have regretted that Palæontologists have generally given so little attention to the remains of *Polyzoa* characterizing these formations. These delicate forms of life existed in great profusion during portions of the Palæozoic era, especially during the deposition of some of the lower members of the great Carboniferous series, and are often met with in a good state of preservation, where no other organic remains are to be seen. Consequently when accurately classified, and the species and genera are fully described and illustrated,—due care being taken to determine the exact geological position of each,—they will at once become an important guide in the identification of strata.

The task of classifying, describing, and illustrating these remains occurring in the western rocks, has been undertaken by Dr. Prout of St. Louis, who has produced several valuable papers on this subject, previous to the publication of that now under review. His last paper, mentioned at the head of this notice, contains full descriptions of nine new species, and two new genera, with four beautiful plates illustrating these and some of the species described in his former papers. These plates are engraved on stone by Leopold Gast and Brother, of St. Louis, from drawings by Dr. Prout and Mr. Gast, and bear evidences of skill and accuracy. The new genera described in this paper are *Semicoscinium* and *Septopora*, and the new species, *Semicoscinium rhomboideum*, *Fenestella hemitrypa*, *F. banyana*, *Limaria falcata*, *Flustra spatula*, *F. tuberculata*, *Septopora Cestriensis*, and *Polypora tuberculata*.

Dr. S. also thinks he has identified a Permian species, *Polypora biamica* of Keyserling, in the Upper Archimedes Limestone, a member of the Lower Carboniferous Series,—at any rate no essential differences were observed in the specimens compared. It is probable however, that when better specimens are obtained, showing all the characters of this Lower Carboniferous form, it will prove distinct from the Permian species. M.

III. BOTANY AND ZOOLOGY.

1. *Collections of Cuban Plants.*—Mr. Charles Wright revisited the eastern part of Cuba in the autumn of the year 1858, where he still remains, engaged in botanical explorations in that little-known region. His collections of dried plants, up to last autumn, have already been received; and the Ferns, which form a large and very attractive part of them, have been distributed into sets. A number of these sets, not yet appropriated, are offered for sale. The fullest of these sets contain about 180, the smallest 120 species, which may be increased by further collections to a moderate extent. Sets can be obtained from Professor Gray, Cambridge, at $10 per hundred specimens. Of phænogamous plants, soon to be distributed, a very few sets are still open to subscribers, at the same price. It is expected that the species will be named very soon. The names of the ferns are about to be published by Mr. Eaton of New Haven, our principal Pteridologist. The rich collections in the lower *Cryptogamia*, made by Mr. Wright in his former visit to Cuba, along with those of the present exploration, are now in course of study, the *Musci* and *Hepaticæ* by Mr. Sullivant, the *Lichenes* by Prof. Tuckerman, the *Fungi* by the Rev. Dr. Curtis, preparatory to their distribution in

named sets. Those who desire to secure full sets should make early application to Prof. Gray, who will have them in charge during Mr. Wright's absence. A. G.

2. *Systematic Arrangement of the Species of the Genus Cuscuta, with critical remarks on old species, and descriptions of new ones;* by GEORGE ENGELMANN, M.D. (Extr. from Trans. Acad. Science of St. Louis, vol. i, pp. 453–523; separate issue pp. 73,) 8vo, 1859.—It is well known to botanists that Dr. Engelmann has for many years been making a special study of *Cuscuta*, and that, besides his own and other American collections, those of the principal European herbaria have been rendered to him for examination. His recent visit to Europe enabled him to extend and to revise his study of this genus. The study of the *Cactaceæ* of North America, which so long interrupted the former investigation, having been brought to a conclusion, Dr. Engelmann has at length been able to publish his revision of the whole genus *Cuscuta*. The thirty-eight species more or less known to Choisy when he elaborated this genus for the Prodromus are doubled in the present enumeration, although an equal number, including several of Choisy's, and of his own formerly proposed, are reduced to synonyms, or arranged as varieties. Many species prove to be remarkably polymorphous, and require an array of varieties and subvarieties to express their manifold diversities in systematic form. The work has involved immense labor, and bears the marks throughout of the most patient and conscientious treatment. Sound views prevail in the generalities as well as in the details; the Linnæan genus is preserved intact, but disposed in three primary groups or subgenera,—*Cuscuta* proper, *Grammica*, and *Monogyna*, and these in nine sections. The species of the first group are all indigenous to the Old World; those of the second are mostly American and Eastern Asian; of the third principally Asian, but two species extend into Europe, one is Texan, and one South African. A. G.

3. *On the Distribution of the Forests and Trees of North America, with Notes on its Physical Geography;* by J. G. COOPER, M.D. pp. 40, 8vo. Extr. from the Report of the Smithsonian Institution for 1858.—The tables illustrating the geographical distribution of our trees and larger shrubs bring together and systematize a great amount of valuable information. As a first essay, it appears to be all that could be expected; and the author himself, having taken up the subject with great zeal and good opportunities, will doubtless perfect it in the future reports for which he is collecting materials. Since the catalogue purports to be one of the *trees* of the United States, we should not have included shrubs, except those of the largest class, and a few which belong to genera characteristically arboreous, such as *Quercus*, *Acer*, &c. Such depressed shrubs as *Prunus maritima* and *Cerasus pumila* are surely out of place. The incongruity is all the more serious in the catalogue of the trees of the regions of the Rocky Mountains and westward, which regions being "comparatively poor in trees," the standard is there still farther reduced, "since shrubs become more valuable where trees diminish in number." This confusion of economical with scientific considerations has the effect of representing the western part of the continent to be far richer in trees than it really is. Of the 108 species in this list only a moiety would

seem to make good their claim to be called forest trees, or trees at all. And in this connection we venture to suggest that the *minimum* as well as the *maximum* height which the trees attain should be given. The *average* height is the more important to be known; the maximum is rather a matter of curiosity. "The reason for giving the maximum heights [only] is, that it is thought the cultivation of trees will become some day a matter of national interest, and I wish to show what they are under the best natural circumstances, supposing that, with cultivation, they will at least equal this standard." We do not suppose so. Under the *same circumstances*, the Lambert-pines of future ages might indeed aspire to 300 feet in height, and the giant *Sequoia* to 450 feet; but planted trees, with room to spread as they should, are never drawn up as in primæval forests.

The delineation of *natural provinces* and *regions* in North America, according to the distribution of our arborescent vegetation, opens wide questions upon which we must not here enter. We should incline to broader views and fewer subdivisions, as preferable for exhibiting the general facts of the case, and more likely to be stable.

The Floridian region is said to have "about thirty-two characteristic and thirteen peculiar trees," and accordingly would appear to be far more strongly marked than any other of the nine. The erroneous impression which this may give would be removed by an expansion of the statement that "Florida appears rather to belong to the West Indian province." Nearly all these peculiar or characteristic trees belong to the Keys, and are Bahaman or West Indian species. Probably no other part of North America is really so destitute of peculiar species of plants as southern and eastern Florida.

In conclusion, while we heartily thank Dr. Cooper for this interesting and useful essay, and expect still better results from his continued investigations, we simply enter a protest against the anachronism of appending an early author's name to a species under a genus which he never heard of, and against the bad taste of writing the names of persons without a capital initial,—both innovations from which botanical writings have thus far been nearly free. A. G.

Zoological Notices.—

1. Prof. J. Victor Carus, of Leipsic, writes to the Smithsonian Institution, July 27, 1859, as follows: "During the last two years, I have been collecting materials towards a general catalogue of zoological literature, from 1750 up to the present day, including not only all the separately published works, books and pamphlets, (most of which are to be found in Engelmann's Bibliotheca Historico-Naturalis,) but also, and especially, all the papers, articles and notices contained in periodicals, the number of which is increasing every year. All the titles and references will be arranged systematically, not according to the alphabet of the authors, but within the classes and groups according to the alphabet of the genera; so that at a glance one will find the whole literature of any particular genus." Prof. Carus calls for aid in obtaining access to American scientific papers, many of which are found in periodicals which are difficult to obtain, as he wishes to make his work as nearly complete as possible. Any who wish to further his laudable object would confer a favor by forward-

ing to him synopses of the contents of the more obscure publications, copies of minor papers, etc.

2. *Die Klassen und Ordnungen des Thier-Reichs, wissenschaftlich dargestellt in Wort und Bild.* Von Dr. H. G. Bronn, Prof. an der Univ. Heidelberg. Leipsic and Heidelberg; C. F. Winter, 1859. 8vo. with plates.—Of this excellent work now in the course of publication, five parts have appeared, containing the Amorphozoa and part of the Actinozoa. The subject is treated in a masterly manner, and the work will undoubtedly be far more complete in its character than any general treatise on Zoology yet published. It commences with an introduction, giving a short general account of the nature of the Animal Kingdom, a history of its investigation by zoologists, and a tabular view of the characteristics of the five subkingdoms, (which are named Amorphozoa, Actinozoa, Malacozoa, and Entomozoa and Spondylozoa,) in regard to their radical form, radical number, embryonic development with reference to the "Primitive-Theil" and position of the yolk, organic system, the head, nervous system, structure of skeleton, teeth, circulatory system, and blood.

In the body of the work each class is treated of under the following heads: I. Introduction, an account of the name and literary history of the class; II. Organic structure; III. Chemical composition; IV. Vitality, and Embryology or Development; V. Classification; VI. Geographical and Topographical Distribution; VII. Geological evolution; and VIII. Signification in the Economy of Nature. In the *Introduction* a full bibliography is given for each class. Under the head of *Classification*, a systematic review or synopsis is given of the sub-classes, orders, suborders, families, sub-families, relationships, (sippschaft) and genera, (sippen); more than the usual number of subdivisions being introduced. In this synopsis, such characters only are given as are necessary to discriminate between known genera, but the work is carefully brought up to the present state of the science in a more thorough manner than has been attempted before in any general work. Some useful abbreviations are introduced which will tend to a convenient shortening of descriptions, although it would have been perhaps better to have formed them from Latin rather than German words. (See p. 44, Vol. II.) The plates are well done, and illustrate not only the structure and development, but a considerable number of the generic forms of each class. The wood-cuts are less remarkable for artistic excellence. W. S.

3. *Description of Oceania (Turritopsis) nutricula, n. s. and the embryological history of a singular Medusan Larva found in the cavity of its bell;* by Prof. John McCrady, (Proc. Elliott Soc. Nat. Hist. of Charleston, S. C., vol. i, p. 55–90); and *The Gymnophthalmata of Charleston Harbor;* by the same author. (Ibid., i, p. 103–221. Plates 4–12.)—These articles, particularly the latter, form the most valuable contribution to the history of our American *Hydroidea* that has appeared since Agassiz' papers in the Memoirs of the American Academy. Like the latter, they are written in that easy, interesting style which is so much more agreeable to the reader than the dry description of details which constitutes the bulk of biological writings. The author has put forth some new views, and enlarged upon the suggestions of his predecessors, in a manner well worthy of the attention of scientific zoologists.

Prof. McCrady states that there is no essential difference between the so-called alternation of generations and a regular metamorphosis, which he calls a *homogony*, after Gegenbaur. The hydroid larvæ therefore of the Gymnophthalmata should no longer be separately named and described, but their history should be included in that of the species to which they belong, as is done in the case of Insects. For the group the name *Hydroidea* is retained, following the rule of priority, *Cryptocarpæ* of Eschscholtz being rejected on account of its erroneous significance. In subdividing the group the author seizes upon an excellent character, founded upon the mode of growth in the medusa-buds, which has been overlooked by previous investigators. In *Tubularia*, *Coryne*, etc., the outer covering of the bud becomes the disc of the medusa, and the digestive tube is enclosed from the first. In *Campanularia* and its allies, on the contrary, the digestive tube projects at first freely from the bud, and becomes afterward enclosed by the over-arching disc, which grows outward from its base. Upon this ground the Hydroidea are separated into two sub-orders—*Endostomata* and *Exostomata*. This subdivision is confirmed by characters derived from the full-grown Medusæ:—in the first group these are deeply bell-shaped, sometimes even sub-cylindrical, with no sinuses in the radiating tubes, and a long digestive cylinder pendant from the disc; in the second they are generally broad and shallow, or saucer-shaped, with many sinuses in the radiating tubes, and a short digestive cavity imbedded in the disc. In the former group are included the Corynidæ, Velellidæ, Tubularidæ and Siphonophoræ. Recent investigations have shown that the Velellidæ and Siphonophoræ are true Hydroidea and the free medusa (*Chrysomitra*) of Velella has been observed and described by Huxley, Vozt, Kolliker and others. The development of the medusa-buds in these two families is after the manner of the Endostomata. In the latter group, Exostomata, the author includes the Campanularidæ, Sertularidæ and Æginidæ, with several genera the family connections of which are uncertain.

In the description of the Hydroidea of Charleston Harbor, Prof. McCrady has more than doubled the number of species known to exist on our shores. He describes 32 species belonging to 30 genera, 8 of which are new, as follows;—among the Endostomata, *Turritopsis*, *Corynitis*, and *Dipurena*; among Exostomata, *Encheilota*, *Entima*, *Epenthesis*, *Phortis*, and *Persa*. In the genus *Nemopis* the author has failed to discover the ocelli described by Agassiz as existing in the tips of the upturned tentacles, and says that the darker hue of these tips is occasioned by their greater thickness. The *Globiceps tiarella* of Ayres (*Eucoryne elegans*, Leidy) is found in Charleston Harbor, and is placed in the genus *Pennaria* by Prof. McCrady, who describes its medusa. The parasitic medusa found in the bell of *Turritopsis nutricula*, and described in the first paper as the young of that species, is afterward considered to be the young of a *Cunina*. The paper closes with a discussion of the geographical distribution of the American Hydroidea. W. S.

4. *On the zoological affinities of Graptolites;* by Prof. JOHN MCCRADY. (Proc. Elliott Soc. Nat. Hist., i, p. 229.)—These paradoxical fossils are regarded by Prof. McCrady as similar to the toothed rods of the larvæ of Echinoderms, described by Joh. Muller. The great discrepancy in size

he explains by suggesting that these embryo-like forms were fully developed animals characteristic of the early geological period at which they existed. W. S.

5. *Letters from Alabama, chiefly relating to Natural History;* by PHILIP HENRY GOSSE, F.R.S. London, 1859, pp. 306: small 8vo.—Another popular scientific work of this prolific writer. It consists of a series of letters written many years ago, when the author was teaching school in the interior of Alabama. In his usual interesting style, Mr. Gosse describes the habits and points out the beauties of many of our Southern plants, insects, reptiles and birds, with numerous illustrations on wood. W. S.

6. *Sketch of a revision of the genera of Mithracidæ;* by WM. STIMPSON.—The old genus *Mithrax* was divided into three groups by Milne-Edwards, (Hist. Nat. des Crust., i. 318), *Mithrax triangulaires*, *M. transversaux*, and *M. déprimés*. These groups are now considered of generic value. For the first group De Haan has proposed the name *Dione*, which cannot however be adopted, as it was previously applied to a genus of Lepidoptera, by Hübner. It is also used for a bivalve shell. To the third group White gave the name *Mithraculus*, which was adopted by Dana.

In the Proceedings of the Zoological Society of London, 1847, p. 222, Mr. Adam White gives a wood-cut of a maioid crustacean, to which he applies the name *Schizophrys*, with the following description: "Carapax oval, depressed, somewhat attenuated behind; beak deeply cloven; upper orbit deeply cloven, with a strong tooth in the middle of the cleft; under orbit an elongated appendage on the inside, with two teeth at the end. Tale of male with 7 joints, the sides nearly parallel. Fore-legs shortest. Fingers without teeth." This genus, in the catalogue of the British Museum, is placed between *Othonia* and *Pericera*. But if we consider that by the "elongated appendage on the inside" of the orbit, Mr. White probably means the basal joint of the external antennæ, and observe how closely the figure corresponds to young crabs belonging to De Haan's genus *Dione*, the conclusion is unavoidable that *Schizophrys* is really synonymous with the *Mithrax triangulaires* of Milne-Edwards, and as such we here adopt it. In the following synopsis new characters are introduced; some new genera are described, and a list given of the species known up to the present date.

A. Maxillipedis externi merus margine antico integer.

MITHRACULUS, White. (*Mithrax*, pro parte, De Haan, Fauna Jap. Cr. 82.) Carapax depressus, rostro brevissimo vel nullo.—*M. sculptus*, (M.-Edw.,) *M. nodosus*, (Bell,) *M. denticulatus*, (Bell,) *M. coronatus*, (Herbst.) *M. cinctimanus*, Stm.

B. Maxillipedis externi merus angulo antero-interno excisus, ad palpum incipiendum.

a. Frons angusta. Orbitæ parvæ, profundæ, oculos pæne operientes, MITHRAX, Leach. Carapax plus minusve transversus. Orbita multifissa, margine dentibus vel tuberculis armata.

1. Antennæ externæ articulus basalis spinis tribus armatus.—*M. verrucosus*, M.-Edw., *M. aculeatus*, (Herbst.)

2. Antennæ externæ articulus basalis spinis duabus armatus.—*M. spinosissimus*, (Lamk.), *M. hispidus*, (Herbst,) *M. ursus*, Bell, *M. rostratus*,

Bell, *M. pygmæus*, Bell, *M. cornutus*, De S., *M. armatus*, De S., *M. tuberculatus*, Stm.

TELEOPHRYS, nov. gen. Carapax antice triangularis, postice et lateraliter rotundatus, sulco cervicali sat profundo. Orbita margine supero externoque integra, nec dentata.—*T. cristulipes*, Stm.

b. Frons lata. Oculi majores. Orbitæ grandes, non profundæ, late fissæ, oculos non operientes.

SCHIZOPHRYS, White. (*Dione*, De Haan.) Carapax ovato-triangularis. Rostrum longum, bifidum, cornibus bidentatis.—*S. dichotoma*, (Latr.) *S. affinis*, (De Haan,) *S. aspera*, (M.-Edw.), *S. serrata*, White, *S. spinigera*, White, *S. dama*, (Herbst.)

CYCLOMAIA, nov. gen. Carapax orbiculatus, antrorsum quam retrorsum vix angustior. Rostrum brevissimum, bifidum, cornibus acutis, non dentatis. Oculi grandes, sat breves. Antennæ externæ articulus basalis trispinosus, spinis superne conspicuis. Maxillipidis externi merus apice interno profunde sinuatus. Pedes mediocres.—*C. suborbicularis*, Stm.

CYCLAX, Dana. *Cyclomaiæ* carapacem affinis. Oculi longi; pedes longi, tenues.—*C. Perryi*, Dana.

Mithraculus, *Mithrax*, and *Teleophrys* are American; *Schizophrys*, *Cyclomaia* and *Cyclax*, old-world types.

7. *Archiv für Naturgeschichte*, vol. xxiv, for 1858; Berlin, 1859:—contains the following articles of special interest:—Anatomy and development of Copepoda, with 2 plates; *Claus.*—Deseriptions of new Chilian Vertebrates by *Dr. Philippi.*—Revision of the Gadidæ, Soleinæ, and Plagusinæ; *Kaup.*—(Mr. Kaup considers our *Morrhua pruinosa* and *M. americana* as one species and identical with *M. vulgaris!*)—Geographical and historical remarks on certain mammals; *Martens.*—On the species of Velutina; *Martens.*—On Annelides of the Brazilian Coast, 2 plates; *F. Müller.*—Enthelminthica, 2 plates; *Wagener.*—Anatomy and Histology of some Trematodes; *Wulter.*—New Batrachians, *Günther.*—On the hard-cheeked Acanthopterygians; *Kaup.* (The number of genera very much reduced.)—Critical remarks on Castelnau's Siluroids; *Kner.*

W. S.

8. *The Natural History Review and Quarterly Journal of Science*, a periodical published at London and Dublin, containing reviews of works relating to Natural History, and also the proceedings of the DUBLIN Scientific Associations, as follows:—the Geological Society, the Natural History Society, the University Zoological and Botanical Association, the Royal Irish Academy, and the Royal Society. In giving a synopsis of the more important zoological papers read before these five Associations, we shall, for the sake of convenience, cite the "Quarterly Journal" instead of their own regular publications:—

Vol. v. 1858, contains—p, 134, On new genera and species of Polyzoa, 4 plates; *W. Thompson.*—p. 148, Cambrian fossils, Histioderma, n. g. (annelide); *Kinahan.*—p. 168, Steropis, a new genus of Carboniferous Crustacea, allied to Limulus; *Baily.*—p. 194, On some Oniscoidea, with a plate, *Kinahan.*—p. 202, New forms of Diastylidæ (wood-cuts); *Bate.*—p. 207, On Oldhamia, a Cambrian fossil; *Kinahan.*—p. 276, Ancient and modern races of Oxen in Ireland, (wood-cuts); *Wilde.*—Vol. vi., 1859, p. 108, On the urticating organs of Actinia; *M'Donnell.*—p. 113, Irish

—p. 191, Crimean Fossils; *Bailey.*—p. 199, New Irish Orthocerata; Actiniadæ; *Wright.*—p. 125, Platyarthrus, Brandt, and allied genera; *Kinahan.*—p. 152, Anatomy of the brain in some small Quadrupeds, 2 plates; *Garner.*—p. 161, Subterranean Gammaridæ; *Bate* and *Hogan. Haughton.*—p. 237, Morphology of the Hydrozoa, with reference to the constitution of the sub-kingdom Cœlenterata; *Greene.* (Mr. Greene includes the Lucernaridæ with Hydrozoa, and considers the Medusidæ—"Animal consisting of a polype suspended from the under surface of a natatorial organ"—an order distinct from the Hydridæ and Tubularidæ !)

9. *An essay on Classification*, by Louis Agassiz.—This work, forming the introduction to Prof. Agassiz' Contributions to the Natural History of the United States, has been published in a separate form, a convenient octavo, by the Longmans and Trübner & Co., London.

10. *On the genus Synapta;* by Woodward and Barrett, (Proc. Zool. Soc. of London, xxvi, 360. Plate xiv).—A short historical and anatomical account of the interesting family of Holothurians in whose skin are found the miniature anchors and wheels, which form such elegant objects for the microscope. They give detailed descriptions of *Synapta digitata* and *S. inhærens*, and add one new species, *S. bidentata*, from China. They also give what purports to be a list of the known species of the family, to which, however, the following should be added:—*Synapta oceanica* Less., *S. doreyana* Quoy and Gaimard, *S. punctulata* Q. & G., *S. bachei* Pourt., *S. tennis* Ayres, *S. pellucida* Ayres, *S. dolabrifera* Stm., *Chirodota fusca* Q. & G., *C. rubeola* Q. & G., *C. tenuis*, Q. & G., *C. rufescens* Brandt, *C. rotifera* Pourt., *C. pallida* Ayres, *C. australiana* Stm., *C.* (*Myriotrochus*) *brevis* Huxley. W. S.

Proceedings of the Elliott Society of Natural History of Charleston, S. C.—Vol. I. 1856.—p. 50, On Cicadæ; *J. Lee.*—New and rare Phænogamous Plants found in the State of South Carolina; *H. W. Ravenel.*—p. 55, Description of Oceania (Turritopsis) nutricula, nov. sp., and the embryology of a Medusan Larva found in its bell; with four plates; *J. McCrady.*—1857, p. 91, On the past and present condition of Niagara Falls; *L. R. Gibbes.*—p. 101, Notice of an ore of Argentiferous Galena; *Frampton.*—p. 102, On the fruit of Yucca gloriosa; *L. R. Gibbes.*—p. 103, Gymnophthalmata of Charleston Harbor, with five plates, (noticed in this Volume, p. 130); *J. McCrady.*—p. 222, On Specific Form; *J. McCrady.*—p. 223, On a Bolina found in Charleston Harbor; *J. McCrady.*—p. 225, Description of Ranilia muricata, M. Edw., with a plate; *L. R. Gibbes.*—p. 229, On the zoological affinities of Graptolites (noticed in this volume, p. 131); *J. McCrady.*—p. 237, Gigantic Orthoceras from Minnesota; *L. A. Frampton.*—p. 238, On a Cactus from Eding's Bay, S. C.; *L. R. Gibbes.*—p. 239, Medusæ of Port Royal Harbor, S. C.; *J. McCrady.*—p. 241, Botany of Eding's Bay; *L. R. Gibbes.*—p. 251, Preparation of Metallic Cobalt; *W. Sharswood.*—p. 254, Development of two species of Ctenophora found in Charleston Harbor, with a plate; *J. McCrady.*—1858, p. 272, Cacti of S. Carolina; *L. R. Gibbes.*—p. 275, Instance of incomplete longitudinal fission in Actinia cavernosa; *J. McCrady.*—p. 278, A new locality for Rutile; *W. Sharswood.*—p. 280, Three new Univalves; *E. Ravenel.*—p. 282, New genus of fossil Echini, Ravenelia, allied to Pygorynchus; *J. McCrady.*—p. 287, Antidote for Arsenious Acid; *W. Sharswood.*—p. 288, Phenomena of the Earthquake of Dec. 19, 1857; *L. R. Gibbes.*—p. 291, On a convenient form of Aspirator; *L. R. Gibbes.*

Proceedings Boston Soc. Nat. Hist., 1859.—p. 49, On the priority of discovery of the fossil footmarks of the Connecticut Valley; *T. T. Bouvé.*—p. 54, Trilobite from Newfoundland; *C. T. Jackson.*—p. 58, Japanese plant-wax; *W. B. Rogers* and *C. T. Jackson.*—p. 60, On the Infusorial deposit in the Tertiary of Virginia and Maryland; *W. B. Rogers.*—p. 64, Mineral resources of the Rocky Mountain Chain;

W. P. Blake.—p. 74, On the frozen well of Brandon, Vt.; *C. Stodder.*—p. 75, Paradoxides Harlani, *C. T. Jackson.*—Habits of marine animals observed at West Yarmouth, Mass.; *T. Lyman.*—Diatomaceæ from Milwaukee; *A. M. Edwards.*—p. 81, Report on the frozen well at Brandon, Vt.; *Jackson* and *Blake.*—p. 89, On collecting, preparing and mounting Diatomaceæ; *A M. Edwards.*—p. 102, A list of Birds seen at the Bahamas, from Jan. 20th to May 14th, 1859, with descriptions of new or little known species; *H. Bryant.*

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 162, On fossil teeth and bones collected by Prof. Emmons: Ontocetus Emmonsi, nov. sp., founded on the tooth of a Cetacean; *J. Leidy.*—Notice of Humboldt; *I. Lea.*—p. 164, On a specimen of Hydaticus zonatus; *C. A. Helmuth.*—p. 165, Description of new generic types of Cottoids from the Collection of the North Pacific Expedition; *T. Gill.*—p. 167, Description of a new species of Callianidea; *T. Gill.*—p. 168, Entomacrodus, a new genus of Salarianæ; *T. Gill.*—p. 169, Herpetological Notices; *C. Girard.*—p. 170, Twelve new Uniones from Georgia; *I. Lea.*—Catalogue of Birds collected on the Rivers Camma and Ogobai, W. Africa, by Mr. P. B. Duchaillu, with notes and descriptions of new species; *J. Cassin.*—p. 177, On fresh-water shells, *I. Lea.*—Fossils from the Post-Pliocene of S. C., with Dr. Leidy's paper on the fossil Horse, and Prof. Agassiz' letter; *Holmes.*—p. 187, Four new exotic Unionidæ; *I. Lea.*—p. 188, Notes on American land-shells, No. 5; *W. G. Binney.*—p. 189, Catalogue of birds collected in the vicinity of Fort Tejon, Cal., with description of a new Syrnium; *J. Xantus.*—p. 194, Freyia Americana, a new animalcule from Newport Harbor; *J. Leidy.*—p. 195, Evorthodus, a new genus of Gobioids; *T. Gill.*—p. 196, Pimeletropis, a new genus of Siluroids from South America; *T. Gill.*—p. 197, New genera and species of N. American Tipulidæ with short palpi, with an attempt at a new classification of the tribe, (pp. 59 and three plates); *R. von Osten Sacken.*—p. 255, A spider catches a fish; *E. A. Spring.*—p. 256, Contributions to American Lepidopterology; *B. Clemens.*—p. 262, On a deformed fragmentary Human Skull from Jerusalem; *J. A. Meigs.* Supplement—Catalogue of the Invertebrate Fossils of the Cretaceous Formation of the United States; *W. M. Gabb.*

Journal Philadelphia Acad. Nat. Sci., Vol. VI, Pt. II.—Contains the following papers:—Synopsis of the North American Sphingidæ, by B. Clemens, M.D.; and New Unionidæ of the United States, by I. Lea, LL.D.

Canadian Naturalist and Geologist, Oct. 1859.—A new Gasterosteus, G. gymnetes; *Dawson.*—Glacial Phenomena of Canada and the North-eastern United States during the Drift Period; *Ramsey.*—On Ozone; *Smallwood.*—Fossils of the Calciferous Sandrock, etc.; *Billings.*—New Trilobites; *Billings.*—On the Aurora of 28th of August; *Smallwood.*

Annals and Magazine of Natural History, London, Oct. 1859.—Cellulose in starch-grains; *H. von Mohl.*—New spiders from Madeira; *J. Blackwall.*—Nudibranchiate mollusks of Ceylon; *Kelaart.*—New genera and species of phytophagous insects; *Baly.*—Digestive power in the Actiniæ; *Holdsworth.*—New Entomostraca from Jerusalem; two plates; *Baird.*—N. American Fungi; *Berkeley* and *Curtis.*—A new antelope (Kobus), from Central Africa; *Gray.*—Nov. 1859.—Reproduction of Bark-lice, with a plate; *R. Leuckart.*—New Anthribidæ; *Pascoe.*—Nomenclature of the Foraminifera; *Parker* and *Jones.*—Coleoptera of Old Calabar; *Murray.*—On certain genera of plants; *Miers.*—On Hydroid Zoophytes; *Allman.*—Ceylon insects; *F. Walker.*—A new Catharus from Western Mexico; *Sclater.*

Proceedings of the Zoological Society of London.—The volume for 1858 contains the following papers of more or less interest to American zoologists:—p. 38, Genera of Olivæ; *J. E. Gray.*—p. 90, On Stavelia, a new genus of Mytilidæ, and on some Distorted Bivalves, with a plate; *J. E. Gray.*—p. 92, Nerita and its operculum; *J. E. Gray.*—p. 145, Rearrangement of the genera of British Actiniadæ; *W. Thompson.*—On Sponges, by Dr. *Gray*, pages 113, 114, 229, and 531, with plates.—p. 136, Separation of the Salamandridæ into two families, by the form of the skull, *J. E. Gray.*—p. 225, Description of new Pinnæ; *S. Hanley.* (The South Carolinian Pinnæ called by American Conchologists P. seminuda and P. muricata are P. carolinensis, Hanl. and P. squamosissima, Phil.)—p. 339, Systematic Arrangement of the Tail-less Batrachians; *Gunther.*—p. 360, On the genus Synapta; *Woodward & Barrett.*—p. 373, Geographical Distribution of Reptiles; *Gunther.*—p. 413, Monograph of the Cæcidæ; *P. P. Carpenter.*—Also many papers relating to Central American Ornithology, by *P. L. Sclater.* W. S.

IV. ASTRONOMY AND METEOROLOGY.

1. *Discovery of the 57th planetoid (Mnemosyne).*—Another planet, appearing like a star of the 10th magnitude, supposed to be one of the group between Mars and Jupiter, was discovered by *M. Robert Luther*, Sept. 22, 1859, at the Observatory of Bilk. It is the 57th of the group, and has been named *Mnemosyne.*—(*Comptes Rendus*, Oct. 5, 1859.)

2. *Total Solar Eclipse of July* 18, 1860.—M. FAYE has called the attention of astronomers and of all lovers of astronomy to the rare opportunity for important observations presented by this eclipse, which will traverse the earth from California to the Red Sea. The total darkness will travel across North America about the 60th degree of North latitude, leaving it at Hudson's Straits, and leaping the Atlantic, pass across Spain, strike the Balearic Isles, pass through Algeria, and crossing the Nile north of Dongola, take leave in Ethiopia. He names seven stations as specially favorable for observation, viz., 1. In Oregon between the Pacific ocean and the Rocky Mountains. 2. In Labrador, in lat. 59° N. 3 and 4. In Spain on the Atlantic and on the Mediterranean coasts. 5. At Ivica in the Balearic isles. 6. At Kabylia in Algeria. 7. At Dongola on the Nile.

At the time of the eclipse, Venus, Mercury, Jupiter and Saturn, will be in the vicinity of the Sun, and form a sort of rhomboid about it. Such a spectacle will not be visible again for many ages.

The objects to be secured by these observations may be arranged under four heads. 1. The more exact determination of the errors of the lunar tables. 2. The determination of the longitudes of places too remote from each other to be connected by the electric telegraph. 3. The verification of the present data for the solar and lunar parallax and the flattening of the earth. 4. The solution of certain questions respecting the physical constitution of the sun, and of the space in its vicinity.

M. Faye proposes that at the two principal stations photographic methods should be substituted in place of direct observation. A telescope of large object glass and long focus should be used, and a large number of proofs should be taken between the first and last contact, taking care to keep horizontal the collodionized plate. During the total obscuration, the whole object glass should be uncovered, and the most sensitive plates employed in order to obtain proofs on a large scale of the aureola and solar flames, while observers provided with hand telescopes, with fresh eyes, should deliberately study all particulars which photography can not secure.

As to the meteorological phenomena, M. Faye proposes to add the sympiezometer as more quick to show the rapid fluctuations of the atmosphere; and instead of the common thermometer to use a self-registering Breguet's metallic thermometer carried into the air by a captive balloon. The variations of the magnet should also be observed, for if the earth's magnetism is affected by the spots which periodically obscure part of the sun's disk, may it not be affected by the more rapid obscuration of the same by the moon? Possibly the wires of the electric telegraph, arranged now with and now against the direction of the eclipse may show perturbations too fugitive to be detected by bar magnets.

The station at Ivica seems to combine all the advantages offered by the peak of Teneriffe. Here especial attention should be given to the form and prolongations of the aureola, the nature and intensity of its light, and also to the zodiacal light, which is now made to play so important a part in the solar system. Careful search should also be made for the small planets near the sun, suspected by M. LeVerrier. Perhaps, moreover, it may be possible to notice clearly the motion of the cone of the lunar shadow, the lower base of which should traverse the surface of the sea at the rate of 900 metres per second, while the upper terminus, if visible, will show by its distance from the zenith the height of the upper strata of our atmosphere.

3. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—On the morning of Nov. 15th, about 9½ o'clock, a remarkable meteor was witnessed by a large number of persons in New York and its vicinity. The meteor was so brilliant that although the sun was unclouded and had an elevation of about twenty degrees above the horizon, the flash attracted the attention of well nigh every person who happened at that time to be looking nearly toward that part of the heavens. The apparent diameter of its head was somewhat less than that of the sun, and it had an appendage like the tail of a comet several degrees in length. Its apparent path was nearly vertical, with a slight inclination towards the west; and the length of its visible path was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds. No sound was heard at New York which could reasonably be ascribed to the meteor. By taking the mean of the estimates of several observers, I have determined that the point of the horizon where the meteor vanished was about 21° west of south.

From the newspaper reports we learn that the same meteor was seen at Salem, Boston, and New Bedford, Mass., at Providence, R. I., at New Haven, Middletown, and Waterbury, Conn., at Albany and many other places in New York, at numerous places in New Jersey, at Baltimore, Md., at Washington and Georgetown, D. C., as also at Alexandria and Fredericsburg, Va. At all of those places the meteor appears to have been seen at the same instant of absolute time; and at all the stations north of New York the appearance was almost identical, and the direction of the meteor was somewhat west of south.

From a newspaper notice coming from Prof. Henry of the Smithsonian Institution, we learn that at Washington the apparent path of the meteor was nearly perpendicular to the horizon, and its point of disappearance was estimated to be four degrees north of east. Those lines of direction as observed at New York and Washington intersect at a point a little north of Cape May; and inasmuch as at each of those stations the apparent path was nearly vertical, the actual path must also have been nearly vertical, and the meteor undoubtedly struck the earth at some point not very remote from Cape May.

This conclusion is confirmed by the reports of the meteor from New Jersey. The meteor was generally observed throughout the southern part of that State, and was everywhere succeeded by a very remarkable explosion. At Beeseley's Point, situated on the Atlantic Ocean near lat. 39° 20′, the course of the meteor is said to have been from northeast to

southwest. It was attended by a sudden flash of light, and left behind a curling track of a smoky or light cloudy appearance, which soon vanished. About a minute after the flash, there was heard a series of terrific explosions, which were compared to the discharge of a thousand cannon. These explosions continued for one or two minutes; they were very sharp and distinct, and shook the windows and doors of the houses. Similar noises have been reported from numerous stations in the southeastern part of New Jersey. These noises occasioned considerable alarm, and by some were thought to have been produced by an earthquake.

From the preceding facts it seems almost certain that the meteor must have struck the earth at some point a little north of Cape May; and as it was unquestionably a body of considerable size and of great density, if it struck on dry ground the meteor ought to have been discovered. As we have received no account of such a discovery there is reason to apprehend that the meteor may have descended into water, and probably into Delaware Bay. Analogy would lead us to conclude that this belonged to the class of iron meteors of which we have numerous specimens in our cabinets.

The velocity of this meteor was very extraordinary. It probably struck the earth at a distance of 110 miles from Washington, and is said to have been first seen at an elevation of 45°. This would make the length of its visible path 110 miles, and it is said to have described this path in two seconds, giving a velocity of 55 miles per second. A small portion of this velocity (7 miles per second) may be ascribed to the earth's attraction, and another portion was due to the motion of the earth in its orbit, for the earth was moving obliquely towards the meteor; but there still remains an independent velocity nearly double the velocity of the earth in its orbit. The path of the meteor in space could not therefore have been a circle with the sun for its center, as the above velocity is too great for any ellipse or even parabola; but such conclusions must be received with caution on account of the imperfection of the observations, for if we suppose the time of describing this path was three seconds, the independent velocity of the meteor would not have been much greater than that of the earth in its orbit.

4. *Meteoric Explosion, in West Tennessee, Sept. 1st*, 1859; by Prof. B. W. McDonnold, of Bethel College.—The first of September was made memorable by the great Aurora. Here, that day of the calendar had another *creta nota*—a meteoric explosion. This explosion was heard at Bethel College about 10 o'clock, A. M., and was at first thought to be the firing of cannon in honor of a political election.

The first report was double, like the almost simultaneous explosion of two great rockets, The reverberations were protracted, deep, distant. After the lapse of perhaps a half minute another explosion was heard, louder, deeper than the former, and the reverberations more protracted. The bearing of the sound was N. E.

I find that the report was heard forty miles north of us, where it was supposed to bear South East; it was heard twelve miles south and west, bearing same as here; but farther south and west it was not heard at all.

As yet, I have heard of no fragments of the meteor being found. I feel satisfied myself, however, of the meteoric origin of the explosion.

5. *Catalogue of the Meteorites in the Imperial Austrian Collection at Vienna;* by Prof. W. HAIDINGER.—Haidinger has communicated to the Austrian Academy of Sciences a complete list of the meteorites contained in the Imperial Collection at Vienna. It is an abstract from the complete catalogue made by the late Prof. Partsch and continued by Dr. Hoernes, the present director of the Imperial Cabinet.

In the list—which here follows—only the names of the localities and the time of falling (I) are given, or (II) in case of meteorites the time of falling of which is unknown they are classified according to the date when first described.

The letter *I* following the year indicates the specimens to be meteoric *iron.*

I. *Meteorites, with time of fall.*

	A. D.		
1.	1492,	Nov. 7,	*Ensisheim*, Alsace, Département du Haut-Rhin, France.
2.	1715,	April 11,	*Garz* (Schellin), near Stargard, Prussia.
3.	1751, *I.*	May 26,	*Agram* (Hraschina village), Croatia.
4.	1753,	July 3,	*Tabar* (Plan, Strkow), Bohemia.
5.	1753,	Sept. 7,	*Liponas*, near Pont de Verle and Bourg en Bresse, Dép. del'Ain, France.
6.	1768,	Sept. 13,	*Lucé* en Maine, Dép. de la Sarthe, France.
7.	1768,	Nov. 20,	*Mauerkirchen*, Inn, Lower Austria.
8.	1773,	Nov. 17,	*Sigena* (Sena village), Aragon, Spain.
9.	1785,	Feb. 19,	*Eichstaedt* (Wittens), Franconia, Bavaria.
10.	1787,	Oct. 13,	*Charkow* (Bobrik), Government Charkow, Russia.
11.	1790,	July 24,	*Barbotan* (Roquefort, Créon, Juillac, Mezin, Agen, &c.), Dép. des Landes, Dép. du Gers, Dép. du Lot et Garonne, formerly Gascony, France.
12.	1794,	June 16,	*Sienna*, Tuscany.
13.	1795,	Dec. 13,	*Wold Cottage*, Yorkshire, England.
14.	1798,	March 8–12,	*Salès*, near Villefranche, Dép. du Rhône, France.
15.	1798,	Dec. 13,	*Benares* (Krakhut village), Bengal, E. Indies.
16.	1803,	April 26,	*L'Aigle*, Normandy, Dép. de l'Orne, France.
17.	1803,	Oct. 8,	*Apt* (Saurette), Dép. de Vaucluse, France.
18.	1803.	Dec. 13,	*Maessing* (Dorf St. Nikolas), Eggenfeld, Bavaria.
19.	1804,	April 5,	*Glasgow* (High Possil), Scotland.
20.	1805,	March 25,	*Doroninsk*, Government Irkutsk, Siberia.
21.	1805,	June,	*Constantinople*, Turkey.
22.	1805,	Nov.,	*Asco*, Corsica.
23.	1806,	March 15,	*Alais*, St. Étienne de Solm and Valence. Dép. du Gard, France.
24.	1807,	March 13,	*Timochin*, (Timschino, according to Eichwald), Iuchnow, Gov. Smolensk, Russia.
25.	1807,	Dec. 14,	*Weston*, Connecticut.
26.	1808,	April 19,	*Parma* (Casignano, Borgo S. Donino).

27. 1808, May 22, *Stannern*, Iglau, Moravia.
28. 1808, Sept. 3, *Lissa*, Bunzlau, Bohemia.
29. 1809, ? *Kikina*, Wiasemsk, Gov. Smolensk, Russia.
30. 1810, Aug. *Tipperary* (Mooresfort), Ireland.
31. 1810, Nov. 22, *Charsonville*, near Orleans, Dép. du Loiret, France.
32. 1811, March 12, *Kuleschowka*, Gov. Poltawa, Russia.
33. 1811, July 8, *Berlanguillas*, near Burgos, Spain.
34. 1812, April 10, *Toulouse*, Dép. de la Haute-Garonne, France.
35. 1812, April 15, *Erxleben*, between Magdeburg and Helmstaedt, Prussia.
36. 1812, Aug. 5, *Chantonnay*, between Nantes and La Rochelle, Dép. de la Vendée, France.
37. 1813, Sept. 10, *Limerick* (Adair, Scagh, Brasky, Faha), Limerick County, Ireland.
38. 1813, Dec. 13, *Lontalax* (Lontalaks), Gov. Wiborg, Finland.
39. 1814, Feb. 15, *Bachmut*, Gov. Iekaterinoslaw, Russia.
40. 1814, Sept. 5, *Agen*, Dép. du Lot et Garonne, France.
41. 1815, Oct. 3, *Chassigny*, near Langres, Dép. de Haute-Marne, France.
42. 1818, April 10, *Zaborzika* (Saboryzy, Saboritz on the Slutsch), Volhynia, Russia.
43. 1818, June, *Seres*, Macedonia, Turkey.
44. 1818, Aug. 10, *Slobodka*, Iuchnow, Gov. Smolensk, Russia.
45. 1819, June 13, *Jonzac* (Barbézieux), Dép. de la Charente, France.
46. 1819, Oct. 13, *Politz*, near Gera, Duchy of Reuss.
47. 1820, July 12, *Lixna* (Liksen), Lasdany, Gov. Witebsk, Russia.
48. 1821, June 15, *Juvenas*, near Libonnez, Dép. de l'Ardèche, France.
49. 1822, Dec. 13, *Épinal* (la Baffe), Dép. des Vosges, France.
50. 1823, Aug. 7, *Nobleborough*, Maine.
51. 1824, Jan. 15, *Renazzo*, in Ferrara, Papal States.
52. 1824, Oct. 14, *Zebrak* (Praskoles), near Horzowitz, Beraun, Bohemia.
53. 1825, ? Government *Iekaterinoslaw*, Russia.
54. 1825, Feb. 10, *Nanjemoy*, Maryland.
55. 1825, Sept. 14, *Honolulu*, Sandwich Islands.
56. 1827, May 9, *Nashville* (Drake Creek), Tennessee.
57. 1827, Oct. 5, *Bialystok* (Kuasta or Kuasti village), Russian Poland.
58. 1828, June 4, *Richmond*, Virginia.
59. 1829, May 8, *Forsyth*, Monroe County, Georgia.
60. 1829, Sept. 9, *Krasnoi-Ugol*, Gov. Riesan, Russia.
61. 1831, July 18, *Vouillé*, near Poitiers, Dép. de la Vienne, France.
62. 1831, Sept. 9, *Wessely* (Dorf Znorow), Moravia.
63. 1833, Nov. 25, *Blansko*, Bruenn, Moravia.
64. 1833, Dec. 27, *Okniny* (Okaninah), Kremenetz District, Gov. Volhynien, Russia.

65. 1835, Nov. 13, *Simonod* (Samonot), Belmont, Dép. de l'Ain, France.
66. 1836, Nov. 11, *Macao*, Prov. Rio Grande de Norte, Brazil.
67. 1837, July 24, *Gross-Divina*, near Budetin, Hungary.
68. 1837, Aug. *Esnaude*, Dép. de la Charente, France.
69. 1838, June 6, *Chandakapoor*, Berar, E. Indies.
70. 1838, Oct. 13, *Capeland* (Bokkeveld, 15 miles from Tulpagh), South Africa.
71. 1839, Feb. 13, *Little Piney*, west of Potosi, Missouri, lat. 37° 55′ N., long. 92° 5′ W. from Greenwich.
72. 1840, July 17, *Cereseto*, near Offiglia, Casale, Piedmont.
73. 1841, March 22, *Grueneberg* (Heinrichsau), Prussian Silesia.
74. 1841, June 12, *Château-Renard*, S. E. of Montargis, Dép. du Loiret, France.
75. 1842, April 26, *Milena* (Milyan), Pusinsko Selo, 4½ miles S. of Milena, Croatia.
76. 1842, June 4, *Aumières*, Canton St. George, Dép. de la Lozère, France.
77. 1843, March, *Bishopville*, South Carolina.
78. 1843, June 2, *Utrecht*, Blaauw Kapel, Loewenhutye, Netherlands.
79. 1843, Sept. 16, *Klein-Wenden*, near Nordhausen, Prussia.
80. 1846, May 8, *Macerata*, Monte Milan village, Ancona, Papal States.
81. 1847, Feb. 25, *Iowa*, Linn County, Iowa.
82. 1847, *I.* July 14, *Braunau* (Hauptmannsdorf), Koeniggraetz, Bohemia.
83. 1849, Oct. 31, *Cabarras County*, North Carolina.
84. 1851, April 17, *Guetersloh*, Westphalia.
85. 1852, Sept. 4, *Mezö-Madaras* (and Fekete), Transylvania.
86. 1852, Oct. 13, *Borkut*, Marmaros, Hungary.
87. 1853, Feb. 10, *Girgenti*, Sicily.
88. 1855, May 13, *Bremervoerde*, Landdrostei Stade, Hanover.
89. 1857, Oct. 10, *Ohaba*, E. of Karlsburg, Transylvania.
90. 1857, Apr. 15, *Kaba*, S. W. of Debreczin, Nordbihar, Hungary.
91. 1858, May 19, *Kakova*, N. W. of Oravitza, Temesvar Banat.

II. *Meteorites, with time of discovery.*

92. 1751, *I.* *Steinbach*, between Eibenstock and Johann-Georgenstadt, Saxony (sometimes given as coming from Norway, Tabor, Senegal, &c.).
93. 1763, *I.* *Senegal*, Siratik in Bambuk, Africa.
94. 1776, *I.* *Krasnojarsk*, Gov. Ieniseisk, Siberia.
95. 1784, *I.* *Toluca*, Mexico.
96. 1788, *I.* *Tucuman* (Otumpa), Argentine Republic, S. America.
97. 1792, *I.* *Zacatecas*, Mexico.
98. 1801, *I.* *Cape of Good Hope*, Africa.
99. 1811, *I.* *Elbogen*, Bohemia.

100. 1811, *I.* *Durango*, Mexico.
101. 1814, *I.* *Bitburg*, Lower Rhine, Prussia.
102. 1814, *I.* *Texas* (Red River).
103. 1815, *I.* *Lénarto*, Scharosch, Hungary.
104. 1816, *I.* *Bahia* (Bemdego), Brazil.
105. 1819, *I.* *Baffins Bay*, Greenland.
106. 1822, *I.* *Brahin*, Gov. Minsk, Russia.
107. 1823, *I.* *Rasgata*, New Granada, S. America.
108. 1827, *I.* *Atacama*, Bolivia.
109. 1828, *I.* *Caille* (Grasse), Dép. du Var, France.
110. 1829, *I.* *Bohumilitz*, Prachin, Bohemia.
111. 1830, *I.* *Guilford*, North Carolina.
112. 1838, *I.* *Claiborne*, Alabama.
113. 1839, *I.* *Asherville*, Buncombe Co., North Carolina.
114. 1840, *I.* *Smith County*, Coney Fork, Tennessee.
115. 1840, *I.* *Cocke County*, Cosby-Creek, (also called Sevier iron), Tennessee.
116. 1841, *I.* *Petropaulowsk*, Gov. Tomsk, Siberia.
117. 1843, *I.* *Oaxaca*, Mexico.
118. 1844, *I.* *Burlington*, Otsego Co., New York.
119. 1844, *I.* *Arva* (Szlanicza), Hungary.
120. 1845, *I.* *Lockport*, New York.
121. 1845, *I.* *Green County* (Babbs-Mills), Greenville, Tennessee.
122. 1845, *I.* *Government Simbirsk*, Russia.
123. 1845, *I.* *Government Kursk*, Russia.
124. 1845, *I.* *Government Poltawa* (according to Eichwald in the district of Kamensk), Russia.
125. 1847, *I.* *Seeläsgen*, Neumark, Brandenburg, Prussia.
126. 1849, *I.* *Chesterville*, South Carolina.
127. 1850, *I.* *Schwetz*, Province of Prussia.
128. 1850, *I.* *Ruff's Mountain*, Newberry, South Carolina.
129. 1850, *I.* *Salt River*, Kentucky.
130. 1851, *I.* *Seneca Falls*, Cayuga Co., New York.
131. 1852, *Mayence*, Duchy of Hesse.
132. 1853, *I.* *Union County*, Georgia.
133. 1853, *I.* *Lion River*, Namaqua Land, South Africa.
134. 1854, *I.* *Tazewell*, Claiborne Co., Tennessee.
135. 1854, *I.* *Putnam County*, Georgia.
136. 1854, *I.* *Canada*, Madoc, Canada West.
137. 1856, *Hainholz*, S. W. of Paderborn, Minden, Westphalia.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Inquiries into the Phenomena of Respiration;* by EDWARD SMITH, M.D., (L. E. and D. Phil. Mag., xvii, 439).—The author gives in this communication the result of numerous inquiries into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration,—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day, and from season to season; and 3rd, the influence of some kinds of exertion.

After a description of the apparatus employed by previous observers, he describes his own apparatus and method. This consists of a spirometer to measure the air inspired, capable of registering any number of cubic inches; and an analytical apparatus to abstract the carbonic acid and vapor from the expired air. The former is a small dry gas-meter, of improved manufacture, and the latter consists of—1st, a desiccator of sulphuric acid to absorb the vapor; 2nd, a gutta-percha box, with chambers and cells, containing caustic potash, and offering a superfices of 700 inches, over which the expired air is passed, and by which the carbonic acid is abstracted; and 3rd, a second desiccator to retain the vapor which the expired air had carried off from the potash box. A small mask is worn, so as to prevent any air entering the lungs without first passing through the spirometer, and the increase in the weight of this with the connecting tube and the first desiccator gives the amount of vapor exhaled, whilst the addition to the weight of the potash box and the second dessiccator gives the weight of the carbonic acid expired. The balances employed weigh to the $\frac{1}{100}$ of a grain, with 7 lbs. in the pan. By this apparatus the whole of the carbonic acid was abstracted during the act of expiration, and the experiment could be repeated every few minutes, or continued for any number of hours, and be made whilst sleeping and with certain kinds of exertion.

The amount of carbonic acid expired in the twenty-four hours was determined by several sets of experiments. Four of these, consisting of eight experiments, were made upon four gentlemen, on the author, Professor Frankland, F.R.S., Dr. Murie, and Mr. Moul, during the eighteen hours of the working day. In two of them, the whole of the carbonic acid was collected, and in two others the experiment was made during ten minutes at the commencement of each hour, and of each hour after the meals. The quantity of carbonic acid varied from an average of 24·274 oz. in the author to 16·43 oz. in Professor Frankland. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and scarcely awake 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute; and the whole evolved in the six hours of the night at 1950 grains. Hence the total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 oz. The effect of walking at various speeds is then given, with an estimate of the amount of exertion made by different classes of the community, and of the carbon which would be evolved with that exertion.

The author then states the quantity of air inspired in the working day, which varied from 583 cub. in. per minute in himself to 365 cub. in. per minute in Professor Frankland; the rate of respiration, which varied in different seasons as well as in different persons; the depth of inspiration, from 30 cub. in. to 39·5 cub. in.; and the rate of pulsation. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. One-half of the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 gr. to 54·7 cub. in. to as 1 gr. to 58 cub. in. The variations in the carbonic acid evolved in the working day gave an average

maximum of 10·43, and minimum of 6·74 grains per minute. The quantity increased after a meal and decreased from each meal, so that the minima were nearly the same, and the maxima were the greatest after breakfast and tea.

The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food; to render the quantity nearly uniform throughout the day, with a little increase at the hours when food had usually been taken, and to cause the secretions to become alkaline.*

The variations from day to day were shown to be connected with the relation of waste and supply on the previous day and night, so that with good health, good night's rest, and sufficient food, the amount of respiration was considerable on the following morning, whilst the reverse occurred with the contrary conditions. Hence the quantities were usually large on the Monday. Temperature was an ever-acting cause of variation and caused a dimunition in the carbonic acid as the temperature rose.

The effect of season was to cause a diminution of all the respiratory phenomena as the hot season advanced. The maximum state was in spring, and the minimum at the end of summer, with periods of decrease in June and of increase in October. The diminution in the author was 30 per cent. in the quantity of air, 32 per cent. in the rate of respiration, and 17 per cent. in the carbonic acid. The influence of temperature was considered in relation to season, and it was shown that whilst sudden changes of temperature cause immediate variation in the quantity of carbonic acid, a medium degree of temperature, as of 60°, is accompanied by all the variations in the quantity of carbonic acid, and that there is no relation between any given temperature and quantity of carbonic acid at different seasons. Whatever was the degree of temperature, the quantity of carbonic acid, and all other phenomena of respiration, fell from the beginning of June to the beginning of September. The author then described the influence of atmospheric pressure, and stated that neither temperature nor atmospheric pressure accounts for the seasonal changes.

The kinds of exertion which had been investigated were walking and the treadwheel. Walking at two miles per hour induced an exhalation of 18·1 grs. of carbonic acid per minute, and at three miles per hour of 25·83 grs.; whilst the effect of the treadwheel at Coldbath Fields Prison was to increase the quantity to 48 grs. per minute. All these quantities vary with the season, and hence the author recommends the adoption of relative quantities, the comparison being with the state of the system at rest, and apart from the influence of food.

2. *Dr. Newberry's Explorations in New Mexico, Utah and Texas*, during the past season, are rewarded by many new and important discoveries, especially in structual geology and palæontology. His collection of fossils is very large, offering conclusive evidence of the geological structure of a very large area. Of the cretaceous deposits he was fortunate in obtaining a particularly satisfactory analysis. Contrary to all our previous notions, these beds turn out to be much more largely developed—that is, existing in much greater force, stratigraphically, West of

* The quantity of air was reduced 30 per cent, that of vapor in the expired air 50 per cent, the rate of respiration was reduced 7 per cent, and of pulsation 6 per cent.

the Rocky Mountains, than East of them. In Southern Utah, (just where Marcou claims there are no cretaceous rocks) he found beautiful exposures of 4000 feet thickness of strata of that age, with abundant fossils, both animal and vegetable. The bones of a huge Saurian are among Dr. Newberry's novelties.

We hope in our next Number to be able to give a more exact statement of Dr. Newberry's important discoveries.

Rumors reach us of other and even more startling geological discoveries in the extreme West and North, which we are not at liberty to name at present, but which ere long will be announced, from the proper authority.

3. *Discovery of Devonian rocks and fossils in Wisconsin.* (Private communication to the editors.)—At a late meeting of the Milwaukee Geological Club or Association, Mr. I. A. Lapham announced the discovery of rocks near Milwaukee, equivalent to the Devonian (Old Red Sandstone,) containing remains, which he exhibited, of characteristic fishes. These remains consist of fragments of bone, teeth, a paddle with portions of the tuberculated skin or osseous covering. The bed containing these remains overlies the Niagara group, and is the uppermost of the geological series yet observed in Wisconsin.

4. *Cretaceous Strata at Gay Head, Mass.*—Wm. Stimpson, Esq., accompanied by Messrs. Slack and Ordway, during an excursion in August to Martha's Vineyard, obtained at Gay Head many new fossils in addition to those mentioned by Hitchcock, an examination of which appears to authorize the conclusion that these well known beds are Cretaceous rather than Eocene. Among the fossils obtained are cretaceous bones, vertebræ and teeth of shark, (fragments of some teeth indicating a length of seven inches!) some brachyurous crustacea in a good state of preservation, twelve species of bivalve mollusca, and one univalve; also leaves, fragments and seeds of dicotyledonous plants, &c.

5. *The New Museum of Comparative Zoology, at Cambridge*, is making rapid progress. One wing is nearly ready to receive collections. During his summer trip in Europe, Agassiz made large and important acquisitions for the Museum, in addition to the vast stores already awaiting an occasion for display. Besides a superb suite of fossil Crustacea, Agassiz was so fortunate as to purchase at Heidelburg the collection of fossils from which Bronn's *Lethaea geognostica* was composed. This collection contains the original specimens of the first and most important writers on Palæontology.

Another important addition to the new museum has been made by a sea Captain, who has just brought from Penang and Singapore some three thousand specimens of fish, crustacea, and a most beautiful and choice collection of zoophytes.

6. William P. Blake, Esq., the geologist, has assumed the editorship of the Mining Magazine, a monthly heretofore published in New York. Under his direction this Journal will undoubtedly become a reliable exponent of the important interests it represents.

7. Prof. Wm. S. Chauvenet, lately of the U. S. Naval Academy, at Annapolis, has accepted the Chair of Mathematics in the University of Missouri, at St. Louis.

8. Professor DANA was, by our last dates (Nov. 26th) at Florence, on his way to Rome—designing to divide the winter and spring between Rome, Naples and Sicily, if the state of the country permits his visiting that Island. His health was improving.

NEW BOOKS.—

1. *Archaia; or Studies of the Cosmogony and Natural History of the Hebrew Scriptures;* by J. W. DAWSON, LL.D., F.G.S., author of "*Acadian Geology*," Principal of McGill College. Montreal: B. Dawson & Son. London: Sampson, Low, Son & Co. 1860. 12mo, pp. 400.—The author of this interesting volume brings to his task the union of a varied scientific, literary and biblical acquirement with a hearty Christian faith. Like all devout and earnest men of science he has an unwavering confidence in the divine unity of truth, and does not for a moment doubt that the genesis of the rocks will confirm the genesis of Moses. He seizes boldly and with candor the real difficulties which every writer has found standing in his way when treating this interesting problem. After an eloquent introduction and a discussion of the object, character and authority of the Hebrew Cosmogony, of the general views of nature contained in the Hebrew Scriptures, &c., he thus sums up his chapter on the Days of Creation—the events of the first day. "At the beginning of the period, the earth, covered with a universal ocean and misty atmospheric mantle, was involved in perfect darkness. A luminous ether was called into existence, which spread a diffused light throughout the whole solar system. This luminous matter being gradually concentrated toward the centre of the system at length produced, in connection with the earth's rotation, the alternation of day and night. These changes were the work of a long period—an æon or day of the Creator."

Undoubtedly the most difficult points in the whole Mosaic Cosmogony, to explain in a rational manner consistent with the views of science, are the creation of plants before the appearance of the '*luminaries*,' and the separation of the two organic kingdoms by the introduction of this middle term. These difficult points are treated with much acuteness and learning, and with a full recognition of the various opinions put forth to meet them by various authors. If we cannot fully agree with our Author in his conclusions, we can truly say that no one has higher claims to a respectful hearing, and if the conclusions at which he arrives leave yet something to be desired, the want rests more in the imperfection of our knowledge than in Prof. Dawson's enunciation of it.

Prof. Dawson does not shrink from a fearless review of the much vexed question of the unity of the human race in a long and interesting chapter on the "Unity and Antiquity of Man." It is hardly necessary to add that he adopts the Mosaic view. We can do this volume little justice, in these few lines to which our last pages restrict us, but we can earnestly commend its spirit, and hope for its wide perusal by all who follow the course of the deep questions it involves.

2. *On the Origin of Species by means of Natural Selection: or, the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN. (Murray.)—[Waiting the arrival of our copy of this new volume from Mr. Darwin's pen, which at this present writing (Dec. 16) has not reached the United States, we copy some passages from a rather

timid and superficial notice of the work in the London Athenæum of Nov. 19th. It is no doubt destined to produce a great discussion on what may properly be called the most fundamental truth of natural history.—Eds.]

"Naturalists of the highest eminence are thoroughly satisfied that each species of animal—all that flies, and walks, and creeps, and wades—has been independently created; and the majority of naturalists have agreed with Linnæus in supposing that all the individuals propagated from one stock have certain distinguishing characters in common, which will never vary, and which have remained the same since the creation of each species. Mr. Darwin, on the contrary, believes that 'the innumerable species, genera, and families of organic beings with which this world is peopled, have all descended, each within its own class or group, from common parents, and have all been *modified in the course of descent*.' To his mind, 'it accords better with what we know of the laws impressed on matter by the Creator that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual.' When he views 'all beings not as special creations, but as the lineal descendents of some few beings which lived long before the first bed of the Silurian system was deposited, they seem to him to become ennobled.' We confess some doubt and some uneasiness here. 'Judging from the past, we may safely infer that not one living species will transmit its unaltered likeness to a distant futurity. And of the species now living very few will transmit progeny of any kind to a far distant futurity; for the manner in which all organic beings are grouped shows that the greater number of species of each genus, and all the species of many genera, have left no descendants, but have become utterly extinct. We can so far take a prophetic glance into futurity as to foretell that it will be the common and widely-spread species, belonging to the larger and dominant groups, which will ultimately prevail and procreate new and dominant species.' We cannot say that this is easy doctrine.

"To support these bold views the volume is devoted. The world of animals is contemplated as engaged in one vast unceasing struggle for existence. All organic beings are exposed to severe competition. The face of Nature, it is true, is bright with gladness, and her garner-houses are stored with an abundance of food. Birds sing, insects hum, beasts prowl about in ease and take no thought for the morrow: but the morrow measured by seasons and years has not always a superabundance of food for them. The struggle for existence does not merely relate to self, but includes success in leaving healthy progeny. The high rate at which all organic beings tend to multiply approaches to the rapidity of geometrical increase. More individuals are produced than can by any possibility be supported. There must, then, in every case, be a severe struggle, either of one individual with another of the same species, or with individuals of distinct species, or with the physical conditions of life."

"Now, how does the struggle for existence operate with respect to Variation? Man can produce varieties in animals by the practice of selection. What he has already done by this means the menagerie, the poultry-yard, the field, and the garden display. Is there anything analo-

gous to this in the course of Nature? The author contends that there is, and he names it *Natural Selection.* This principle, whatever others may think of it, and whether they admit its operations or not, in Mr. Darwin's book plays the prominent part. It may be plainly defined, and appears to be briefly this. Under domestication it may be truly said that the whole animal organization becomes in some degree plastic. As variations useful to man have undoubtedly occurred, is it not to be expected that other variations, useful in some way to each being in the great and complex battle of life, should sometimes occur in the course of thousands of generations? If such do occur, then, remembering the struggle for existence, individuals possessing any advantage over others would have the best chance of surviving and of procreating their kind, while injurious variations would be rigidly destroyed. Such a continual preservation of favorable, and rejection of injurious variations, is the principle of Natural Selection. It is illustrated, amplified and confirmed by abundant examples through many pages."

"Certainly there is something poetical in the conception of a succession of created beings, daily and hourly making the wisest election amidst all variations and divergencies; carefully rejecting what is bad, and preserving and accumulating all that is good; operating silently and insensibly, whenever and wherever opportunity offers, towards the improvement of every organized existence in relation to its organic and inorganic condition of life. There is, too, a certain simplicity in the theory of descent with modification through natural selection from a few vastly remote progenitors. 'I believe,' says Mr. Darwin, 'that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number. Analogy would lead us one step further—namely, to the belief that all animals and plants have descended from some one prototype.' A cabbage may have been the parent plant, a fish the parent animal.

"A man of imaginative power might most attractively depict the grand yet simple and direct issues of such a theory. Here are a vast variety of forms of life, most wonderfully co-adapted, most closely connected, most richly adorned, yet they are all 'the lineal descendants of those which lived before the Silurian epoch; and one may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence, we may look with some confidence to a secure future of equally inappreciable length. And as Natural Selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress toward perfection.' Yes, an unbroken, sure, though slow, living progress towards animal perfectibility is a delightful vision; natural and gradual optimism is a welcome fancy. What need of distinct creation? If a monkey has become a man—what may not a man become?

"Let the past history of organic life speak. From the thirteen miles in thickness of British strata (exclusive of igneous rocks) comes there no testimony? Palæontology is summoned into court, and is closely interrogated by Mr. Darwin. This proves but a hesitating and reluctant witness; yet counsel for the new theory detects and exposes its imperfections where its testimony is not favorable. We might fairly expect to find in

the fossiliferous rocks not a few proofs of the former existence of the numerous intermediate links between distinct specific forms if the proposed theory be true. We do not find them, many will allege, because they never existed. Not so, says our theorist,—but because they were never preserved. Palæontology, however, has not yet revealed any such finely graduated organic scale, and it is not logical to assume that it ever will. When a record is flatly against you, it is quite allowable for you to display its imperfection, but, that being proved, you have only established a negative, and have acquired no confirmation. Grant imperfection, enormous lapse of time, poverty of palæontological collections, and comparative restriction of research, and other such postulates, and then the theory stands just as it stood before, uncorroborated by geology.

"There is positively hostile testimony from the rocks to be confronted. Whole groups of species suddenly and abruptly appear in certain formations, and seem at once to contradict any theory of transmutation of species. Either that fact or the theory must be overturned. Of course, Mr. Darwin accepts the former alternative, and strives to show how liable we are to error in supposing that whole groups of species have been suddenly produced. But another and an allied objection may be started, derived from the manner in which numbers of species of the same group suddenly appear in the lowest known fossiliferous rocks. To meet this and uphold the new theory, it must be sustained by another, viz.,—that before the lowest Silurian stratum was deposited, immensely protracted periods elapsed, at least as long as any subsequent periods, and that during these vast extensions of time the world swarmed with living creatures. Several of the most eminent geologists, including Murchison, will refuse to admit this presumption. Mr. Darwin's geology is more singular than we had thought. 'For instance,' says he, 'I cannot doubt that all Silurian trilobites have descended from some one crustacean which must have lived long before the Silurian age, and which probably differed greatly from any known animal.' Extend and multiply such assumptions, and the theories may take any form you please."

"After all, this book is but an abstract. The larger work is nearly finished, but it will demand two or three more years for completion. Health, labor, and observations are wanting for awhile, but in due season we hope to see the work 'with references and authorities for the several statements.' We should offer remarks on some important topics but that our author says, 'A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of the question; and this cannot possibly be here done.'

"Meanwhile Mr. Darwin anticipates small favor from many of the older and more eminent naturalists; his hopes chiefly rest on the young, and, as he would say, the unshackled. 'A few naturalists,' he observes, 'endowed with much flexibility of mind, who have already begun to doubt on the immutability of species, may be influenced by this volume; but I look with confidence to the future, to young and rising naturalists who will be able to view both sides of the question with impartiality.' It is enough for us to add that neither book, author, nor

subject is of merely ordinary character. The work deserves attention, and will, we have no doubt, meet with it. Scientific naturalists will take up the author upon his own peculiar ground; and there will we imagine be a severe struggle for at least theoretical existence."

3. *Elements of Somatology: A Treatise on the general properties of Matter;* by GEO. M. MACLEAN, M.D., Prof. Chemistry and Nat. Philosophy, in Alleghany City, Pa. New York: J. Wiley, 56 Walker St., 1859. 12mo, pp. 124.—This modest little volume, the author tells us in his preface, is the fruit of many hours of study during a period of ill-health—a sort of "Consolations of a Philosopher."

It is a simple exposition of accepted doctrines on the familiar subjects of extension, impenetrability, figure, divisibility, indestructibility, porosity, compressibility, dilatibility, mobility, inertia, contraction, repulsion, polarity, elasticity, and the constitution of matter. The subject of Attraction he considers under thirty-one subdivisions. His expositions of the phenomena of adhesion, capillarity and osmose are more full and satisfactory than it is usual to find in elementary works. On many points in his discussions, we might join issue with our Author, as when he states cohesive attraction to be only a modification of gravitation—(p. 57)—and when he adduces the phenomena of contraction in a soap-bubble, in illustration of the cause of the meniscus of capillarity (p. 69).

It would have added materially to the value of the Treatise, and its interest to the student and general reader, if the author had appended to passages marked as quotations a reference to the authority from which they are copied. Except a quotation accredited to Cavallo, we do not recall a single reference to any authority in the volume.

A brief statement of the accepted doctrines of physics on the subject of "*Molecular Forces*" would have relieved his chapters on attraction and repulsion of several obscure points.

The work bears marks of haste, or want of careful revision of the press. Among many examples of this we may name the sentence under capillary attraction, commencing "The tube having the form of a syphon," (foot of p. 69) which conveys so confused a notion of what the Author seeks to express, that after several readings we have been unable to comprehend it. Bodies are said to weigh *less* near the poles than at the equator, (p. 55), and numerous typographical blemishes evidence the disadvantage of printing a scientific book at a distance from the press. These minor faults are easily removed in a new edition—which will be very likely to be called for, as the book is one of convenient reference for all teachers. The author will, however, confer a great favor on all such readers in a new edition, by quoting his authorities.

4. *The Telegraphic Manual, a complete history and description of the Semaphoric, Electric and Magnetic Telegraphs of Europe, Asia, Africa, and America, ancient and modern, with 625 illustrations;* by T. P. SCHAFFNER, of Kentucky. N. York, 1859. 8vo, pp. 850.—The title of this volume is an index to its contents. That he may leave nothing behind him for future explorers, the author commences his labors with Adam and Eve, in Eden! Mr. Schaffner has, from his wide and long experience in telegraphic construction and management both in the United States and in Europe, remarkable qualifications for the work he has under-

taken. The result of his labors is satisfactory. His work, in fullness of detail, leaves little to desire, and he appears always solicitous to avoid the charge of partizanship in awarding to rival parties what he judges to be their respective shares of merit, in cases of contested claims. As a literary production, it is to be regretted that the author did not submit his manuscripts to the revision of some judicious literary friend—thus avoiding certain faults of style of too frequent occurrence. But these are minor faults and can be easily pardoned where there is so much to praise.

5. *Bail's Drawing System:* THE HUMAN HEAD, by LOUIS BAIL, (graduate of the Royal Academy of Fine Arts, in Munich). New Haven: Author, 1859. 8vo, 64 plates in outline.—Prof. Bail has here done a great service to both teachers and pupils in the Arts in the United States. The success which has followed the Author's use of his own system in many of our higher seminaries, as well as in public classes, is the best guaranty of the adaptation of its parts to the great ends of instruction, and no doubt will secure its general adoption.

6. *Memoir of John Griscom, LL.D., late Professor of Chemistry and Nat. Philosophy, &c.;* by his son, JOHN H. GRISCOM, M.D. New York: Carter, 1859. 8vo, pp. 427.—Some among the older readers of this Journal will recall with pleasure the selections from foreign scientific literature, which for many years Prof. Griscom prepared for these pages. His active life was well filled with varied duty as an instructor and philanthropist. He was either largely or entirely instrumental in the establishment of the New York High School; the Society for the prevention of Pauperism; the House of Refuge; and other institutions of public charity, which amid all the complaints of profligacy in her public administration, have shed a peculiar honor on the active benevolence of the City of New York.

As early as 1818, he instituted and sustained for many years, independent courses of scientific lectures, in New York city, and in other places—illustrating his courses by numerous experiments, and a costly apparatus procured at his own expense. This was long before the era of *popular lectures*, and Dr. Griscom, with the senior Editor of this Journal, may claim the honor of inaugurating a system which has since become almost universal in the United States.

Dr. Griscom published two volumes of Travels in Europe, in 1818–19, remarkable for the spirit of candor and kindness which is seen on every page, and interesting to this day, for the characteristic personal sketches he gives of the distinguished men of science he met abroad.

Dr. Griscom was an eminently good man; a member of the Society of Friends; a devout Christain believer, and without bigotry. His mild and gentle nature delighted in the most catholic liberality, and many of his warmest friends were members of other Christian sects. We have recently been called upon to commemorate several eminent scientific friends and collaborators—Cleaveland, Hare, Redfield and Olmsted, now numbered with the dead. We now add the name of Griscom—a name cherished long and warmly by intimate social and scientific relations. Our early auxiliaries and friends in science are now few in number, and our duties are soon to pass into younger and we hope better hands—

but the pioneers will be remembered as the pilgrims of science, although its votaries are now a Legion. This Memoir is a fine example of its class, and does credit both to the filial piety and literary ability of its distinguished Author. B. S.

FLEURY: *Des races qui se partagent l'Europe.* 8vo, 132 pages. Hachette & Co.—This author has brought to his study of the races great learning and a deep knowledge of the facts. He considers modern European civilization as springing from the German race.

JAMIN: *Cour de Physique de l'Ecole polytechnique.* T. II. with 3 plates and 191 figures in the text.—This volume contains *Heat* and *Acoustics*. The plates are engraved and the mathematical formulæ are printed with the neatness and accuracy which distinguish at present the productions of the press of Bachelier above all others in France.

G. LAMÉ: *Leçons sur les coordennées, curvilignes et leurs diverses applications*, in 8vo. 1859. Mallet-Bachelier, Paris.—Under this title the distinguished Professor of the Polytechnic School introduces to us a new branch of mathematical science. It is geometry considered from a physico-mathematical point of view.

OBITUARY.—

Professor WILLIAM W. TURNER, one of our most distinguished philologists, died at Washington, Nov. 29, 1859, in the 50th year of his age. Although an excellent linguist, he devoted himself less to the study of words than to that of the structure of languages, their origin and connections, upon which subject his views were eminently philosophic. He was born in London, but was brought in his fifth year to this country, where he has ever since resided. He early developed a taste for the study of oriental languages and was in 1842 appointed instructor in the Hebrew and cognate tongues in the Union Theological Seminary of New York. The last seven years of his life were spent in Washington, where his attainments and upright, amiable deportment secured to him a host of friends. He did a vast amount of work in the way of translation and grammatic compilation, little of which has, however, appeared in his name:—he is chiefly known for his contributions to the "Bibliotheca Sacra" and to the journals of the American Oriental and Ethnological societies. But his published works give no adequate idea of the extent of his labors; his stores of knowledge were always open to his friends and most freely imparted, thus contributing to the advancement of sciences other than his own. He had, during the past ten years, given much attention to the study of the Aborigines of North America and their languages, not only elaborating general principles from the vocabularies collected by travellers, but confirming these and adding new information by communication with the delegates from various tribes that visited Washington upon business with the central government. In this investigation he accumulated a large amount of materials which will, it may be hoped, be some time given to the world. W. S.

Dr. GEORGE WILSON, First Regius Professor of Technology in the University of Edinburgh, and Director of the Industrial Museum of that City, died near the end of November last, at the early age of 41. He was the biographer of Reid and Cavendish, and author of numerous researches, among which are the discovery of fluorine in blood and sea-water. Other of his published works are his "Researches on Color-blindness," an "Elementary Treatise on Chemistry," and "The Five Gate-ways of Knowledge." He is a great loss to his native city and the world.

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[SECOND SERIES.]

ART. XV.—*Review of Darwin's Theory on the Origin of Species by means of Natural Selection.**

FULLY to understand the foregoing Essay of Dr. Hooker,† it should be read in the light of Mr. Darwin's book. The Essay is a trial of the Theory,—an attempt by one inclined in its favor to see how the theory will work, when applied to the flora of a large and most peculiar province of the world.

This book is already exciting much attention. Two American editions are announced, through which it will become familiar to many of our readers, before these pages are issued. An abstract of the argument,—for "the whole volume is one long argument," as the author states,—is unnecessary in such a case; and it would be difficult to give by detached extracts. For the volume itself is an abstract, a prodromus of a detailed work upon which the author has been laboring for twenty years, and which "will take two or three more years to complete." It is exceedingly compact; and although useful summaries are ap-

* *On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN, M.A., Fellow of the Royal, Geological, Linnæan, etc. Societies, Author of "Journal of Researches during H. M. S. Beagle's Voyage round the World." London: John Murray. 1859. pp. 502, post 8vo.

† This article was intended to follow the remaining part of the essay of Dr. Hooker, commenced in our January number; the continuation of which we are obliged to defer, for want of room.—EDS.

pended to the several chapters, and a general recapitulation contains the essence of the whole, yet much of the aroma escapes in the treble distillation, or is so concentrated that the flavor is lost to the general, or even to the scientific reader. The volume itself,—the proof-spirit—is just condensed enough for its purpose. It will be far more widely read, and perhaps will make deeper impression than the elaborate work might have done, with its full details of the facts upon which the author's sweeping conclusions have been grounded. At least it is a more readable book: but all the facts that can be mustered in favor of the theory are still likely to be needed.

Who, upon a single perusal, shall pass judgment upon a work like this, to which twenty of the best years of the life of a most able naturalist have been devoted? And who among those naturalists who hold a position that entitles them to pronounce summarily upon the subject, can be expected to divest himself for the nonce of the influence or received and favorite systems? In fact, the controversy now opened is not likely to be settled in an off-hand way, nor it is desirable that it should be. A spirited conflict among opinions of every grade must ensue, which,—to borrow an illustration from the doctrine of the book before us—may be likened to the conflict in nature among races in the struggle for life, which Mr. Darwin describes; through which the views most favored by facts will be developed and tested by 'Natural Selection,' the weaker ones be destroyed in the process, and the strongest in the long run alone survive.

The duty of reviewing this volume in the American Journal of Science would naturally devolve upon the principal Editor, whose wide observation and profound knowledge of various departments of natural history, as well as of geology, particularly qualify him for the task. But he has been obliged to lay aside his pen, and to seek in distant lands the entire repose from scientific labor so essential to the restoration of his health,—a consummation devoutly to be wished, and confidently to be expected. Interested as Mr. Dana would be in this volume, he could not be expected to accept its doctrine. Views so idealistic as those upon which his "Thoughts upon Species"* are grounded, will not harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin. Though it is just possible that one who regards the kinds of elementary matter, such as oxygen and hydrogen, and the definite compounds of these elementary matters, and their compounds again, in the mineral kingdom, as constituting species, in the same sense, fundamentally, as that of animal and vegetable species, might admit an evolution of one species from another in the latter as well as the former case.

* Article in this Journal, vol. xxiv, p. 305.

Between the doctrines of this volume and those of the other great Naturalist whose name adorns the title-page of this Journal, the widest divergence appears. It is interesting to contrast the two, and, indeed, is necessary to our purpose; for this contrast brings out most prominently, and sets in strongest light and shade the main features of the theory of the origination of species by means of Natural Selection.

The ordinary and generally received view assumes the independent, specific creation of each kind of plant and animal in a primitive stock, which reproduces its like from generation to generation, and so continues the species.* Taking the idea of species from this perennial succession of essentially similar individuals, the chain is logically traceable back to a local origin in a single stock, a single pair, or a single individual, from which all the individuals composing the species have proceeded by natural generation. Although the similarity of progeny to parent is fundamental in the conception of species, yet the likeness is by no means absolute: all species vary more or less, and some vary remarkably—partly from the influence of altered circumstances, and partly (and more really) from unknown constitutional causes which altered conditions favor rather than originate. But these variations are supposed to be mere oscillations from a normal state, and in Nature to be limited if not transitory; so that the primordial differences between species and species at their beginning have not been effaced, nor largely obscured, by blending through variation. Consequently, whenever two reputed species are found to blend in nature through a series of intermediate forms, community of origin is inferred, and all the forms, however diverse, are held to belong to one species. Moreover, since bisexuality is the rule in nature (which is practically carried out, in the long run, far more generally than has been suspected), and the heritable qualities of two distinct individuals are mingled in the offspring, it is supposed that the general sterility of hybrid progeny, interposes an effectual barrier against the blending of the original species by crossing.

From this generally accepted view the well-known theory of Agassiz and the recent one of Darwin diverge in exactly opposite directions.

That of Agassiz differs fundamentally from the ordinary view only in this, that it discards the idea of a common descent as the real bond of union among the individuals of a species, and also the idea of a local origin,—supposing, instead, that each species originated simultaneously, generally speaking over the whole geographical area it now occupies or has occupied, and

* "Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes." —*Linn. Phil. Bot.*, 99, 157.

in perhaps as many individuals as it numbered at any subsequent period.

Mr. Darwin, on the other hand, holds the orthodox view of the descent of all the individuals of a species not only from a local birth-place, but from a single ancestor or pair; and that each species has extended and established itself, through natural agencies, wherever it could; so that the actual geographical distribution of any species is by no means a primordial arrangement, but a natural result. He goes farther, and this volume is a protracted argument intended to prove that the species we recognize have not been independently created, as such, but have descended, like varieties, from other species. Varieties, on this view, are incipient or possible species: species are varieties of a larger growth and a wider and earlier divergence from the parent stock: the difference is one of degree, not of kind.

The ordinary view—rendering unto Cæsar the things that are Cæsar's—looks to natural agencies for the actual distribution and perpetuation of species, to a supernatural for their origin.

The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin, as equally derivative, equally natural.

The theory of Agassiz, referring as it does the phenomena both of origin and distribution directly to the Divine will,—thus removing the latter with the former out of the domain of inductive science (in which efficient cause is not the first, but the last word),—may be said to be theistic to excess. The contrasted theory is not open to this objection. Studying the facts and phenomena in reference to proximate causes, and endeavoring to trace back the series of cause and effect as far as possible, Darwin's aim and processes are strictly scientific, and his endeavor, whether successful or futile, must be regarded as a legitimate attempt to extend the domain of natural or physical science. For though it well may be that "organic forms have no physical or secondary cause," yet this can be proved only indirectly, by the failure of every attempt to refer the phenomena in question to causal laws. But, however originated, and whatever be thought of Mr. Darwin's arduous undertaking in this respect, it is certain that plants and animals are subject from their birth to physical influences, to which they have to accommodate themselves as they can. How literally they are "born to trouble," and how incessant and severe the struggle for life generally is, the present volume graphically describes. Few will deny that such influences must have gravely affected the range and the association of individuals and species on the earth's surface. Mr. Darwin thinks that, acting upon an inherent predisposition to vary, they have sufficed even to modify

the species themselves and produce the present diversity. Mr. Agassiz believes that they have not even affected the geographical range and the actual association of species, still less their forms; but that every adaptation of species to climate and of species to species is as aboriginal, and therefore as inexplicable, as are the organic forms themselves.

Who shall decide between such extreme views so ably maintained on either hand, and say how much of truth there may be in each? The present reviewer has not the presumption to undertake such a task. Having no prepossession in favor of naturalistic theories, but struck with the eminent ability of Mr. Darwin's work, and charmed with its fairness, our humbler duty will be performed if, laying aside prejudice as much as we can, we shall succeed in giving a fair account of its method and argument, offering by the way a few suggestions, such as might occur to any naturalist of an inquiring mind. An editorial character for this article must in justice be disclaimed. The plural pronoun is employed not to give editorial weight, but to avoid even the appearance of egotism, and also the circumlocution which attends a rigorous adherence to the impersonal style.

We have contrasted these two extremely divergent theories, in their broad statements. It must not be inferred that they have no points nor ultimate results in common.

In the first place they practically agree in upsetting, each in its own way, the generally received definition of species, and in sweeping away the ground of their objective existence in Nature. The orthodox conception of species is that of lineal descent: all the descendants of a common parent, and no other, constitute a species; they have a certain identity because of their descent, by which they are supposed to be recognizable. So naturalists had a distinct idea of what they meant by the term species, and a practical rule, which was hardly the less useful because difficult to apply in many cases, and because its application was indirect,—that is, the community of origin had to be inferred from the likeness; that degree of similarity, and that only, being held to be conspecific which could be shown or reasonably inferred to be compatible with a common origin. And the usual concurrence of the whole body of naturalists (having the same data before them) as to what forms are species attests the value of the rule, and also indicates some real foundation for it in nature. But if species were created in numberless individuals over broad spaces of territory, these individuals are connected only in idea, and species differ from varieties on the one hand and from genera, tribes, &c. on the other only in degree; and no obvious natural reason remains for fixing upon this or that degree as specific, at least no natural standard, by which the opinions of different naturalists may be correlated.

Species upon this view are enduring, but subjective and ideal. Any three or more of the human races, for example, are species or not species, according to the bent of the naturalist's mind. Darwin's theory brings us the other way to the same result. In his view, not only all the individuals of a species are descendants of a common parent but of all the related species also. Affinity, relationship, all the terms which naturalists use figuratively to express an underived, unexplained resemblance among species, have a literal meaning upon Darwin's system, which they little suspected, namely, that of inheritance. Varieties are the latest offshoots of the genealogical tree in "an unlineal" order; species, those of an earlier date, but of no definite distinction; genera, more ancient species, and so on. The human races, upon this view likewise may or may not be species according to the notions of each naturalist as to what differences are specific: but, if not species already, those races that last long enough are sure to become so. It is only a question of time.

How well the simile of a genealogical tree illustrates the main ideas of Darwin's theory the following extract from the summary of the fourth chapter shows.

"It is a truly wonderful fact,—the wonder of which we are apt to overlook from familiarity—that all animals and all plants throughout all time and space should be related to each other in group subordinate to group, in the manner which we everywhere behold—namely, varieties of the same species most closely related together, species of the same genus less closely and unequally related together, forming sections and sub-genera, species of distinct genera much less closely related, and genera related in different degrees, forming sub-families, families, orders, sub-classes, and classes. The several subordinate groups in any class cannot be ranked in a single file, but seem rather to be clustered round points, and these round other points, and so on in almost endless cycles. On the view that each species has been independently created, I can see no explanation of this great fact in the classification of all organic beings; but, to the best of my judgment, it is explained through inheritance and the complex action of natural selection, entailing extinction and divergence of character, as we have seen illustrated in the diagram.

"The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during each former year may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have tried to overmaster other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was small, budding twigs; and this connexion of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs which

flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear all the other branches; so with the species which lived during long-past geological periods, very few now have living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these lost branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only from having been found in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favored and is still alive on its summit, so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever branching and beautiful ramifications."

It may also be noted that there is a significant correspondence between the rival theories as to the main facts employed. Apparently every capital fact in the one view is a capital fact in the other. The difference is in the interpretation. To run the parallel ready made to our hands:*

"The simultaneous existence of the most diversified types under identical circumstances, ... the repetition of similar types under the most diversified circumstances, ... the unity of plan in otherwise highly diversified types of animals, ... the correspondence, now generally known as special homologies, in the details of structure otherwise entirely disconnected, down to the most minute peculiarities, ... the various degrees and different kinds of relationship among animals which [apparently] can have no genealogical connection, ... the simultaneous existence in the earliest geological periods....of representatives of all the great types of the animal kingdom, ... the gradation based upon complications of structure which may be traced among animals built upon the same plan; the distribution of some types over the most extensive range of surface of the globe, while others are limited to particular geographical areas, ... the identity of structures of these types, notwithstanding their wide geographical distribution, ... the community of structure in certain respects of animals otherwise entirely different, but living within the same geographical area, ... the connection by series of special structures observed in animals widely scattered over the surface of the globe, ... the definite relations in which animals stand to the surrounding world, ... the relations in which individuals of the same species stand to one another, ... the limitation of the range of changes which animals undergo during their growth, ... the return to a definite norm of animals which multiply in various ways, ... the order of succession of the different types of animals and plants characteristic of the

* Agassiz, Essay on Classification; Contrib. to Nat. Hist., i, p. 132, et seq.

different geological epochs, ... the localization of some types of animals upon the same points of the surface of the globe during several successive geological periods; .... the parallelism between the order of succession of animals and plants in geological times, and the gradation among their living representatives, ... the parallelism between the order of succession of animals in geological times and the changes their living representatives undergo during their embryological growth,* .... *the combination in many extinct types of characters which in later ages appear disconnected in different types*, ... the parallelism between the gradation among animals and the changes they undergo during their growth, ... the relations existing between these different series and the geographical distribution of animals, ... the connection of all the known features of nature into one system,—"

In a word, the whole relations of animals, &c. to surrounding nature and to each other, are regarded under the one view as ultimate facts, or in their ultimate aspect, and interpreted theologically;—under the other as complex facts, to be analyzed and interpreted scientifically. The one naturalist, perhaps too largely assuming the scientifically unexplained to be inexplicable, views the phenomena only in their supposed relation to the Divine mind. The other, naturally expecting many of these phenomena to be resolvable under investigation, views them in their relations to one another, and endeavors to explain them as far as he can (and perhaps farther) through natural causes.

But does the one really exclude the other? Does the investigation of physical causes stand opposed to the theological view and the study of the harmonies between mind and Nature? More than this, is it not most presumable that an intellectual conception realized in nature would be realized through natural agencies? Mr. Agassiz answers these questions affirmatively when he declares that "the task of science is to investigate what has been done, to enquire if possible *how it has been done*, rather than to ask what is possible for the Deity, since *we can know that only by what actually exists*;" and also when he extends the argument for the intervention in nature of a creative mind to its legitimate application in the inorganic world; which, he remarks, "considered in the same light, would not fail also to exhibit unexpected evidence of thought, in the character of the laws regulating the chemical combinations, the action of physi-

* As to this, Darwin remarks that he can only hope to see the law hereafter proved true (p. 449); and p. 338: "Agassiz insists that ancient animals resemble to a certain extent the embryos of recent animals of the same classes; or that the geological succession of extinct forms is in some degree parallel to the embryological development of recent forms. I must follow Pictet and Huxley in thinking that the truth of this doctrine is very far from proved. Yet I fully expect to see it hereafter confirmed, at least in regard to subordinate groups, which have branched off from each other within comparatively recent times. For this doctrine of Agassiz accords well with the theory of natural selection."

cal forces, etc., etc."* Mr. Agassiz, however, pronounces that "the connection between the facts is *only intellectual*;"—an opinion which the analogy of the inorganic world, just referred to, does not confirm, for there a material connection between the facts is justly held to be consistent with an intellectual,—and which the most analogous cases we can think of in the organic world do not favor; for there is a material connection between the grub, the pupa, and the butterfly, between the tadpole and the frog, or, still better, between those distinct animals which succeed each other in alternate and very dissimilar generations. So that mere analogy might rather suggest a natural connection than the contrary; and the contrary cannot be demonstrated until the possibilities of nature under the Deity are fathomed.

But the intellectual connection being undoubted, Mr. Agassiz properly refers the whole to "the agency of Intellect as its first cause." In doing so, however, he is not supposed to be offering a scientific explanation of the phenomena. Evidently he is considering only the ultimate *why*, not the proximate why or *how*.

Now the latter is just what Mr. Darwin is considering. He conceives of a physical connection between allied species: but we suppose he does not deny their intellectual connection, as related to a Supreme Intelligence. Certainly we see no reason why he should, and many reasons why he should not. Indeed, as we contemplate the actual direction of investigation and speculation in the physical and natural sciences, we dimly apprehend a probable synthesis of these divergent theories, and in it the ground for a strong stand against mere naturalism. Even if the doctrine of the origin of species through natural selection should prevail in our day, we shall not despair; being confident that the genius of an Agassiz will be found equal to the work of constructing, upon the mental and material foundations combined, a theory of nature as theistic and as scientific, as that which he has so eloquently expounded.

To conceive the possibility of "the descent of species from species by insensibly fine gradations" during a long course of time, and to demonstrate its compatibility with a strictly theistic view of the universe, is one thing: to substantiate the theory itself or show its likelihood is quite another thing. This brings us to consider what Darwin's theory actually is, and how he supports it.

That the existing kinds of animals and plants, or many of them, may be derived from other and earlier kinds, in the lapse

* Op. cit., p. 131.—One or two Bridgewater Treatises, and most modern works upon Natural Theology should have rendered the evidences of thought in inorganic nature not "unexpected."

of time, is by no means a novel proposition. Not to speak of ancient speculations of the sort, it is the well-known Lamarckian theory. The first difficulty which such theories meet with is that, in the present age, with all its own and its inherited prejudgments, the whole burden of proof is naturally, and indeed properly, laid upon the shoulders of the propounders; and thus far the burden has been more than they could bear. From the very nature of the case, substantive proof of specific creation is not attainable; but that of derivation or transmutation of species may be. He who affirms the latter view is bound to do one or both of two things. Either, 1, to assign real and adequate causes, the natural or necessary result of which must be to produce the present diversity of species and their actual relations; or, 2, to show the general conformity of the whole body of facts to such assumption, and also to adduce instances explicable by it and inexplicable by the received view,—so perhaps winning our assent to the doctrine, through its competency to harmonize all the facts, even though the cause of the assumed variation remain as occult as that of the transformation of tadpoles into frogs, or that of *Coryne* into *Sarzia*.

The first line of proof, successfully carried out, would establish derivation as a true physical theory; the second, as a sufficient hypothesis.

Lamarck mainly undertook the first line, in a theory which has been so assailed by ridicule that it rarely receives the credit for ability to which in its day it was entitled. But he assigned partly unreal, partly insufficient causes; and the attempt to account for a progressive change in species through the direct influence of physical agencies, and through the appetencies and habits of animals reacting upon their structure, thus causing the production and the successive modification of organs, is a conceded and total failure. The shadowy author of the Vestiges of the Natural History of Creation can hardly be said to have undertaken either line, in a scientific way. He would explain the whole progressive evolution of nature by virtue of an inherent tendency to development,—thus giving us an idea or a word in place of a natural cause, a restatement of the proposition instead of an explanation. Mr. Darwin attempts both lines of proof, and in a strictly scientific spirit; but the stress falls mainly upon the first; for, as he does assign real causes, he is bound to prove their adequacy.

It should be kept in mind that, while all direct proof of independent origination is unattainable from the nature of the case, the overthrow of particular schemes of derivation has not established the opposite proposition. The futility of each hypothesis thus far proposed to account for derivation may be made apparent, or unanswerable objections may be urged against

it; and each victory of the kind may render derivation more improbable, and therefore specific creation more probable, without settling the question either way. New facts, or new arguments and a new mode of viewing the question may some day change the whole aspect of the case. It is with the latter that Mr. Darwin now reopens the discussion.

Having conceived the idea that varieties are incipient species, he is led to study variation in the field where it shows itself most strikingly and affords the greatest facilities to investigation. Thoughtful naturalists have had increasing grounds to suspect that a re-examination of the question of species in zoology and botany, commencing with those races which man knows most about, viz. the domesticated and cultivated races, would be likely somewhat to modify the received idea of the entire fixity of species. This field, rich with various but unsystematized stores of knowledge accumulated by cultivators and breeders, has been generally neglected by naturalists, because these races are not in a state of nature; whereas they deserve particular attention on this very account, as experiments, or the materials for experiments, ready to our hand. In domestication we vary some of the natural conditions of a species, and thus learn experimentally what changes are within the reach of varying conditions in nature. We separate and protect a favorite race against its foes or its competitors, and thus learn what it might become if nature ever afforded it equal opportunities. Even when, to subserve human uses, we modify a domesticated race to the detriment of its native vigor, or to the extent of practical monstrosity, although we secure forms which would not be originated and could not be perpetuated in free nature, yet we attain wider and juster views of the possible degree of variation. We perceive that some species are more variable than others, but that no species subjected to the experiment persistently refuses to vary; and that when it has once begun to vary, its varieties are not the less but the more subject to variation. "No case is on record of a variable being ceasing to be variable under cultivation." It is fair to conclude, from the observation of plants and animals in a wild as well as domesticated state, that the tendency to vary is general, and even universal. Mr. Darwin does "not believe that variability is an inherent and necessary contingency, under all circumstances, with all organic beings, as some authors have thought." No one supposes variation could occur under all circumstances; but the facts on the whole imply an universal tendency, ready to be manifested under favorable circumstances. In reply to the assumption that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates, it is asked:

"How could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass or guinea-fowl, or the small power of endurance of warmth by the rein-deer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would vary on an average as largely as the parent species of our existing domesticated productions have varied."

As to amount of variation, there is the common remark of naturalists that the varieties of domesticated plants or animals often differ more widely than do the individuals of distinct species in a wild state: and even in nature the individuals of some species are known to vary to a degree sensibly wider than that which separates related species. In his instructive section on the breeds of the domestic pigeon, our author remarks that:—"at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in the same genus; more especially as in each of these breeds several truly inherited sub-breeds, or species as he might have called them, could be shown him." That this is not a case like that of dogs, in which probably the blood of more than one species is mingled, Mr. Darwin proceeds to show, adducing cogent reasons for the common opinion that all have descended from the wild rock-pigeon. Then follow some suggestive remarks:—

"I have discussed the probable origin of domestic pigeons at some, yet quite insufficient, length; because when I first kept pigeons and watched the several kinds, knowing well how true they bred, I felt fully as much difficulty in believing that they could ever have descended from a common parent, as any naturalist could in coming to a similar conclusion in regard to many species of finches, or other large groups of birds, in nature. One circumstance has struck me much; namely, that all the breeders of the various domestic animals and the cultivators of plants, with whom I have ever conversed, or whose treatises I have read, are firmly convinced that the several breeds to which each has attended, are descended from so many aboriginally distinct species. Ask, as I have asked, a celebrated raiser of Hereford cattle, whether his cattle might not have descended from long-horns, and he will laugh you to scorn. I have never met a pigeon, or poultry, or duck, or rabbit fancier, who was not fully convinced that each main breed was descended from a distinct species. Van Mons, in his treatise on pears and apples, shows how utterly he disbelieves that the several sorts, for instance a Ribston-pippin

or Codlin-apple, could ever have proceeded from the seeds of the same tree. Innumerable other examples could be given. The explanation, I think, is simple: from long-continued study they are strongly impressed with the differences between the several races; and though they well know that each race varies slightly, for they win their prizes by selecting such slight differences, yet they ignore all general arguments, and refuse to sum up in their minds slight differences accumulated during many successive generations. May not those naturalists who, knowing far less of the laws of inheritance than does the breeder, and knowing no more than he does of the intermediate links in the long lines of descent, yet admit that many of our domestic races have descended from the same parents—may they not learn a lesson of caution, when they deride the idea of species in a state of nature being lineal descendants of other species?"

The actual causes of variation are unknown. Mr. Darwin favors the opinion of the late Mr. Knight, the great philosopher of horticulture, that variability under domestication is somehow connected with excess of food. He also regards the unknown cause as acting chiefly upon the reproductive system of the parents, which system, judging from the effect of confinement or cultivation upon its functions, he concludes to be more susceptible than any other to the action of changed conditions of life. The tendency to vary certainly appears to be much stronger under domestication than in free nature. But we are not sure that the greater variableness of cultivated races is not mainly owing to the far greater opportunities for manifestation and accumulation—a view seemingly all the more favorable to Mr. Darwin's theory. The actual amount of certain changes, such as size or abundance of fruit, size of udder, stands of course in obvious relation to supply of food.

Really, we no more know the reason why the progeny occasionally deviates from the parent than we do why it usually resembles it. Though the laws and conditions governing variation are known to a certain extent, while those governing inheritance are apparently inscrutable. "Perhaps," Darwin remarks, "the correct way of viewing the whole subject would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly." This, from general and obvious considerations, we have long been accustomed to do. Now, as exceptional instances are expected to be capable of explanation, while ultimate laws are not, it is quite possible that variation may be accounted for, while the great primary law of inheritance remains a mysterious fact.

The common proposition is, that *species reproduce their like;* this is a sort of general inference, only a degree closer to fact than the statement that genera reproduce their like. The true proposition, the fact incapable of further analysis is, that *individ-*

uals reproduce their like,—that characteristics are inheritable. So varieties, or deviations once originated, are perpetuable, like species. Not so likely to be perpetuated, at the outset; for the new form tends to resemble a grand-parent and a long line of similar ancestors, as well as to resemble its immmediate progenitors. Two forces which coincide in the ordinary case, where the offspring resembles its parent, act in different directions when it does not, and it is uncertain which will prevail. If the remoter, but very potent ancestral influence predominates, the variation disappears with the life of the individual. If that of the immediate parent—feebler no doubt, but closer—the variety survives in the offspring; whose progeny now has a redoubled tendency to produce its own like; whose progeny again is almost sure to produce its like, since it is much the same whether it takes after its mother or its grandmother.

In this way races arise, which under favorable conditions may be as hereditary as species. In following these indications, watching opportunities, and breeding only from those individuals which vary most in a desirable direction, man leads the course of variation as he leads a streamlet,—apparently at will, but never against the force of gravitation,—to a long distance from its source, and makes it more subservient to his use or fancy. He unconsciously strengthens those variations which he prizes when he plants the seed of a favorite fruit, preserves a favorite domestic animal, drowns the uglier kittens of a litter, and allows only the handsomest or the best mousers to propagate. Still more, by methodical selection, in recent times almost marvellous results have been produced in new breeds of cattle, sheep, and poultry, and new varieties of fruit of greater and greater size or excellence.

It is said that all domestic varieties if left to run wild, would revert to their aboriginal stocks. Probably they would wherever various races of one species were left to commingle. At least the abnormal or exaggerated characteristics induced by high feeding, or high cultivation, and prolonged close breeding would promptly disappear, and the surviving stock would soon blend into a homogeneous result (in a way presently explained), which would naturally be taken for the original form; but we could seldom know if it were so. It is by no means certain that the result would be the same if the races ran wild each in a separate region. Dr. Hooker doubts if there is a true reversion in the case of plants. Mr. Darwin's observations rather favor it in the animal kingdom. With mingled races reversion seems well made out in the case of pigeons. The common opinion upon this subject therefore probably has some foundation. But even if we regard varieties as oscillations around a primitive centre or

type, still it appears from the readiness with which such varieties originate, that a certain amount of disturbance would carry them beyond the influence of the primordial attraction, where they may become new centres of variation.

Some suppose that races cannot be perpetuated indefinitely even by keeping up the conditions under which they were fixed: but the high antiquity of several, and the actual fixity of many of them, negative this assumption. "To assert that we could not breed our cart and race horses, long and short-horned cattle, and poultry of various breeds, for almost an infinite number of generations would be opposed to all experience."

Why varieties develope so readily and deviate so widely under domestication, while they are apparently so rare or so transient in free nature, may easily be shown. In nature, even with hermaphrodite plants, there is a vast amount of cross fertilization among various individuals of the same species. The inevitable result of this (as was long ago explained in this Journal*) is to repress variation, to keep the mass of a species comparatively homogeneous over any area in which it abounds in individuals. Starting from a suggestion of the late Mr. Knight, now so familiar, that close interbreeding diminishes vigor and fertility†; and perceiving that bisexuality is ever aimed at in nature,—being attained physiologically in numerous cases where it is not structurally,—Mr. Darwin has worked out the subject in detail, and shown how general is the concurrence, either habitual or occasional, of two hermaphrodite individuals in the reproduction of their kind; and has drawn the philosophical inference that probably no organic being self-fertilizes indefinitely; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable. We refer the reader to the section on the intercrossing of individuals (p. 96–101), and also to an article in the Gardeners' Chronicle a year and a half ago, for the details of a very interesting contribution to science, irrespective of theory.

In domestication, this intercrossing may be prevented; and in this prevention lies the art of producing varieties. But "the art itself is Nature," since the whole art consists in allowing the most universal of all natural tendencies in organic things (inheritance) to operate uncontrolled by other and obviously incidental tendencies. No new power, no artificial force is brought into play either by separating the stock of a desirable variety so as to prevent mixture, or by selecting for breeders those indi-

* Vol. xvii, [2], 1854, p. 13.

† We suspect that this is not an ultimate fact, but a natural consequence of inheritance,—the inheritance of disease or of tendency to disease, which close interbreeding perpetuates and accumulates, but wide breeding may neutralize or eliminate.

viduals which most largely partake of the peculiarities for which the breed is valued.*

We see everywhere around us the remarkable results which Nature may be said to have brought about under artificial selection and separation. Could she accomplish similar results when left to herself? Variations might begin, we know they do begin, in a wild state. But would any of them be preserved and carried to an equal degree of deviation? Is there anything in nature which in the long run may answer to artificial selection? Mr. Darwin thinks that there is; and *Natural Selection* is the key-note of his discourse.

As a preliminary, he has a short chapter to show that there is variation in nature, and therefore something for natural selection to act upon. He readily shows that such mere variations as may be directly referred to physical conditions (like the depauperation of plants in a sterile soil, or their dwarfing as they approach an alpine summit, the thicker fur of an animal from far northward, &c.), and also those individual differences which we everywhere recognize but do not pretend to account for, are not separable by any assignable line from more strongly marked varieties; likewise that there is no clear demarcation between the latter and subspecies, or varieties of the highest grade (distinguished from species not by any known inconstancy, but by the supposed lower importance of their characteristics); nor between these and recognized species. "These differences blend into each other in an insensible series, and the series impresses the mind with an idea of an actual passage."

This gradation from species downward is well made out. To carry it one step farther upwards, our author presents in a strong light the differences which prevail among naturalists as to what forms should be admitted to the rank of species. Some genera (and these in some countries) give rise to far more discrepancy than others; and it is concluded that the large or dominant genera are usually the most variable. In a flora so small as the British, 182 plants generally reckoned as varieties, have been ranked by some botanists as species. Selecting the British genera which include the most polymorphous forms, it appears that Babington's Flora gives them 251 species, Bentham's only 112, a difference of 139 doubtful forms. These are nearly the extreme views; but they are the views of two most capable and most experienced judges, in respect to one of the best known floras of the world. The fact is suggestive, that the best known countries furnish the greatest number of such doubtful cases.

* The rules and processes of breeders of animals, and their results, are so familiar that they need not be particularized. Less is popularly known about the production of vegetable races. We refer our readers back to this Journal, xxvii, pp. 440–442 (May, 1859) for an abstract of the papers of M. Vilmorin upon this subject.

Illustrations of this kind may be multiplied to a great extent. They make it plain that, whether species in nature are aboriginal and definite or not, our practical conclusions about them, as embodied in systematic works, are not *facts* but *judgments*, and largely fallible judgments.

How much of the actual coincidence of authorities is owing to imperfect or restricted observation, and to one naturalist's adopting the conclusions of another without independent observation, this is not the place to consider. It is our impression that species of animals are more definitely marked than those of plants; this may arise from our somewhat extended acquaintance with the latter, and our ignorance of the former. But we are constrained by our experience to admit the strong likelihood, in botany, that varieties on the one hand and what are called closely related species on the other do not differ except in degree. Whenever the wider difference separating the latter can be spanned by intermediate forms, as it sometimes is, no botanist long resists the inevitable conclusion. Whenever, therefore, this wider difference can be shown to be compatible with community of origin, and explained through natural selection or in any other way, we are ready to adopt the *probable* conclusion; and we see beforehand how strikingly the actual geographical association of related species favors the broader view. Whether we should continue to regard the forms in question as distinct species, depends upon what meaning we shall finally attach to that term; and that depends upon how far the doctrine of derivation can be carried back and how well it can be supported.

In applying his principle of natural selection to the work in hand, Mr. Darwin assumes, as we have seen: 1, some variability of animals and plants in nature; 2, the absence of any definite distinction between slight variations, and varieties of the highest grade; 3, the fact that naturalists do not practically agree, and do not increasingly tend to agree, as to what forms are species and what are strong varieties, thus rendering it probable that there may be no essential and original difference, or no possibility of ascertaining it, at least in many cases; also, 4, that the most flourishing and dominant species of the larger genera on an average vary most (a proposition which can be substantiated only by extensive comparisons, the details of which are not given);—and, 5, that in large genera the species are apt to be closely but unequally allied together, forming little clusters round certain species,—just such clusters as would be formed if we suppose their members once to have been satellites or varieties of a central or parent species, but to have attained at length a wider divergence and a specific character. The fact of such association is undeniable; and the use which Mr. Darwin makes of it seems fair and natural.

The gist of Mr. Darwin's work is to show that such varieties are gradually diverged into species and genera through *natural selection;* that natural selection is the inevitable result of the *struggle for existence* which all living things are engaged in; and that this struggle is an unavoidable consequence of several natural causes, but mainly of the high rate at which all organic beings tend to increase.

Curiously enough, Mr. Darwin's theory is grounded upon the doctrine of Malthus and the doctrine of Hobbes. The elder DeCandolle had conceived the idea of the struggle for existence, and in a passage which would have delighted the cynical philosopher of Malmesbury, had declared that all nature is at war, one organism with another or with external nature; and Lyell and Herbert had made considerable use of it. But Hobbes in his theory of society and Darwin in his theory of natural history, alone have built their systems upon it. However moralists and political economists may regard these doctrines in their original application to human society and the relation of population to subsistence, their thorough applicability to the great society of the organic world in general is now undeniable. And to Mr. Darwin belongs the credit of making this extended application, and of working out the immensely diversified results with rare sagacity and untiring patience. He has brought to view *real causes* which have been largely operative in the establishment of the actual association and geographical distribution of plants and animals. In this he must be allowed to have made a very important contribution to an interesting department of science, even if his theory fails in the endeavor to explain the origin or diversity of species.

"Nothing is easier," says our author, "than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, I am convinced that the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood. We behold the face of nature bright with gladness, we often see superabundance of food; we do not see, or we forget, that the birds which are idly singing round us mostly live on insects or seeds, and are thus constantly destroying life; or we forget how largely these songsters, or their eggs, or their nestlings, are destroyed by birds and beasts of prey; we do not always bear in mind, that though food may be now superabundant, it is not so at all seasons of each recurring year."—p. 62.

"There is no exception to the rule that every organic being naturally increases at so high a rate, that if not destroyed, the earth would soon be covered by the progeny of a single pair. Even slow-breeding man has doubled in twenty-five years, and at this rate, in a few thousand years, there would literally not be standing room for his progeny. Linnæus has calculated that if an annual plant produced only two seeds—

and there is no plant so unproductive as this—and their seedlings next year produced two, and so on, then in twenty years there would be a million plants. The elephant is reckoned to be the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase: it will be under the mark to assume that it breeds when thirty years old, and goes on breeding till ninety years old, bringing forth three pairs of young in this interval; if this be so, at the end of the fifth century there would be alive fifteen million elephants, descended from the first pair.

"But we have better evidence on this subject than mere theoretical calculations, namely, the numerous recorded cases of the astonishingly rapid increase of various animals in a state of nature, when circumstances have been favorable to them during two or three following seasons. Still more striking is the evidence from our domestic animals of many kinds which have run wild in several parts of the world; if the statements of the rate of increase of slow-breeding cattle and horses in South America, and latterly in Australia, had not been well authenticated, they would have been quite incredible. So it is with plants: cases could be given of introduced plants which have become common throughout whole islands in a period of less than ten years. Several of the plants now most numerous over the wide plains of La Plata, clothing square leagues of surface almost to the exclusion of all other plants, have been introduced from Europe; and there are plants which now range in India, as I hear from Dr. Falconer, from Cape Comorin to the Himalaya, which have been imported from America since its discovery. In such cases, and endless instances could be given, no one supposes that the fertility of these animals or plants has been suddenly and temporarily increased in any sensible degree. The obvious explanation is that the conditions of life have been very favorable, and that there has consequently been less destruction of the old and young, and that nearly all the young have been enabled to breed. In such cases the geometrical ratio of increase, the result of which never fails to be surprising, simply explains the extraordinarily rapid increase and wide diffusion of naturalized productions in their new homes."—pp. 64, 65.

"All plants and animals are tending to increase at a geometrical ratio; all would most rapidly stock any station in which they could anyhow exist; the increase must be checked by destruction at some period of life."—p. 65.

The difference between the most and the least prolific species is of no account.

"The condor lays a couple of eggs, and the ostrich a score; and yet in the same country the condor may be the more numerous of the two. The Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world."—p. 68.

"The amount of food gives the extreme limit to which each species can increase; but very frequently it is not the obtaining of food, but the serving as prey to other animals, which determines the average numbers of a species."—p. 68.

"Climate plays an important part in determining the average numbers of a species, and periodical seasons of extreme cold or drought, I believe

to be the most effective of all checks. I estimated that the winter of 1854–55 destroyed four-fifths of the birds in my own grounds; and this is a tremendous destruction, when we remember that ten per cent is an extraordinarily severe mortality from epidemics with man. The action of climate seems at first sight to be quite independent of the struggle for existence; but in so far as climate chiefly acts in reducing food, it brings on the most severe struggle between the individuals, whether of the same or of distinct species, which subsist on the same kind of food. Even when climate, for instance extreme cold, acts directly, it will be the least vigorous, or those which have got least food through the advancing winter, which will suffer most. When we travel from south to north, or from a damp region to a dry, we invariably see some species gradually getting rarer and rarer, and finally disappearing; and the change of climate being conspicuous, we are tempted to attribute the whole effect to its direct action. But this is a very false view: we forget that each species, even where it most abounds, is constantly suffering enormous destruction at some period of its life, from enemies or from competitors for the same place and food; and if these enemies or competitors be in the least degree favored by any slight change of climate, they will increase in numbers, and, as each area is already stocked with inhabitants, the other species will decrease. When we travel southward and see a species decreasing in numbers, we may feel sure that the cause lies quite as much in other species being favored, as in this one being hurt. So it is when we travel northward, but in a somewhat lesser degree, for the number of species of all kinds, and therefore of competitors, decreases northwards; hence in going northward, or in ascending a mountain, we far oftener meet with stunted forms, due to the *directly* injurious action of climate, than we do in proceeding southwards or in descending a mountain. When we reach the Arctic regions, or snow-capped summits, or absolute deserts, the struggle for life is almost exclusively with the elements.

"That climate acts in main part indirectly by favoring other species, we may clearly see in the prodigious number of plants in our gardens which can perfectly well endure our climate, but which never become naturalized, for they cannot compete with our native plants, nor resist destruction by our native animals."—pp. 68, 69.

After an instructive instance in which "cattle absolutely determine the existence of the Scotch Fir," we are referred to cases in which insects determine the existence of cattle.

"Perhaps Paraguay offers the most curious instance of this; for here neither cattle nor horses nor dogs have ever run wild, though they swarm southward and northward in a feral state; and Azara and Rengger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by birds. Hence, if certain insectivorous birds (whose numbers are probably regulated by hawks or beasts of prey) were to increase in Paraguay, the flies would decrease—then cattle and horses would become feral, and this would certainly

greatly alter (as indeed I have observed in parts of South America) the vegetation: this again would largely affect the insects; and this, as we just have seen in Staffordshire, the insectivorous birds, and so onwards in ever-increasing circles of complexity. We began this series by insectivorous birds, and we had ended with them. Not that in nature the relations can ever be as simple as this. Battle within battle must ever be recurring with varying success; and yet in the long run the forces are so nicely balanced, that the face of nature remains uniform for long periods of time, though assuredly the merest trifle would often give the victory to one organic being over another. Nevertheless so profound is our ignorance, and so high our presumption, that we marvel when we hear of the extinction of an organic being; and as we do not see the cause, we invoke cataclysms to desolate the world, or invent laws on the duration of the forms of life!"—pp. 72, 73.

"When we look at the plants and bushes clothing an entangled bank, we are tempted to attribute their proportional numbers and kinds to what we call chance. But how false a view is this! Every one has heard that when an American forest is cut down, a very different vegetation springs up; but it has been observed that the trees now growing on the ancient Indian mounds, in the Southern United States, display the same beautiful diversity and proportion of kinds as in the surrounding virgin forests. What a struggle between the several kinds of trees must here have gone on during long centuries, each annually scattering its seeds by the thousand; what war between insect and insect—between insects, snails, and other animals with birds and beasts of prey—all striving to increase, and all feeding on each other or on the trees or their seeds and seedlings, or on the other plants which first clothed the ground and thus checked the growth of the trees! Throw up a handful of feathers, and all must fall to the ground according to definite laws; but how simple is this problem compared to the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins!"—pp. 74, 75.

For reasons obvious upon reflection the competition is often, if not generally, most severe between nearly related species when they are in contact, so that one drives the other before it, as the Hanoverian the old English rat, the small Asiatic cockroach in Russia, its greater congener, &c.: and this, when duly considered, explains many curious results;—such, for instance, as the considerable number of different genera of plants and animals which are generally found to inhabit any limited area.

"The truth of the principle, that the greatest amount of life can be supported by great diversification of structure, is seen under many natural circumstances. In an extremely small area, especially if freely open to immigration, and where the contest between individual and individual must be severe, we always find great diversity in its inhabitants. For instance, I found that a piece of turf, three feet by four in size, which had been exposed for many years to exactly the same conditions, supported twenty species of plants, and these belonged to eighteen genera and to

eight orders, which showed how much these plants differed from each other. So it is with the plants and insects on small and uniform islets; and so in small ponds of fresh water. Farmers find that they can raise most food by a rotation of plants belonging to the most different orders; nature follows what may be called a simultaneous rotation. Most of the animals and plants which live close round any small piece of ground, could live on it (supposing it not to be in any way peculiar in its nature), and may be said to be striving to the utmost to live there; but, it is seen, that where they come into the closest competition with each other, the advantages of diversification of structure, with the accompanying differences of habit and constitution, determine that the inhabitants, which thus jostle each other most closely, shall as a general rule, belong to what we call different genera and orders."—p. 114.

The abundance of some forms, the rarity and final extinction of many others, and the consequent divergence of character or increase of difference among the surviving representatives are other consequences. As favored forms increase, the less favored must diminish in number, for there is not room for all; and the slightest advantage, at first probably inappreciable to human observation, must decide which shall prevail and which must perish, or be driven to another and for it more favorable locality.

We cannot do justice to the interesting chapter upon natural selection by separated extracts. The following must serve to show how the principle is supposed to work.

"If during the long course of ages and under varying conditions of life, organic beings vary at all in the several parts of their organization, and I think this cannot be disputed; if there be, owing to the high geometrical powers of increase of each species, at some age, season, or year, a severe struggle for life, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of existence, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, I think it would be a most extraordinary fact if no variation ever had occurred useful to each being's own welfare, in the same way as so many variations have occurred useful to man. But if variations useful to any organic being do occur, assuredly individuals thus characterized will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance they will tend to produce offspring similarly characterized. This principle of preservation, I have called, for the sake of brevity, Natural Selection."—pp. 126, 127.

"In order to make it clear how, as I believe, natural selection acts, I must beg permission to give one or two imaginary illustrations. Let us take the case of a wolf, which preys on various animals, securing some by craft, some by strength, and some by fleetness; and let us suppose that the fleetest prey, a deer for instance, had from any change in the country increased in numbers, or that other prey had decreased in numbers, during that season of the year when the wolf is hardest pressed for food. I can under such circumstances see no reason to doubt that the swiftest and slimmest wolves would have the best chance of surviving,

and so be preserved or selected,—provided always that they retained strength to master their prey at this or at some other period of the year, when they might be compelled to prey on other animals. I can see no more reason to doubt this, than that man can improve the fleetness of his greyhounds by careful and methodical selection, or by that unconscious selection which results from each man trying to keep the best dogs without any thought of modifying the breed.

"Even without any change in the proportional numbers of the animals on which our wolf preyed, a cub might be born with an innate tendency to pursue certain kinds of prey. Nor can this be thought very improbable; for we often observe great differences in the natural tendencies of our domestic animals; one cat, for instance, taking to catch rats, another mice; one cat, according to Mr. St.John, bringing home winged game, another hares or rabbits, and another hunting on marshy ground and almost nightly catching woodcocks or snipes. The tendency to catch rats rather than mice is known to be inherited. Now, if any slight innate change of habit or of structure benefited an individual wolf, it would have the best chance of surviving and of leaving offspring. Some of its young would probably inherit the same habits or structure, and by the repetition of this process, a new variety might be formed which would either supplant or coexist with the parent-form of wolf. Or, again, the wolves inhabiting a mountainous district, and those frequenting the lowlands, would naturally be forced to hunt different prey; and from the continued preservation of the individuals best fitted for the two sites, two varieties might slowly be formed. These varieties would cross and blend where they met; but to this subject of intercrossing we shall soon have to return. I may add, that, according to Mr. Pierce, there are two varieties of the wolf inhabiting the Catskill Mountains in the United States, one with a light greyhound-like form, which pursues deer, and the other more bulky, with shorter legs, which more frequently attacks the shepherd's flocks."—pp. 90, 91.

We eke out the illustration here with a counterpart instance, viz., the remark of Dr. Bachman that "The deer that reside permanently in the swamps of Carolina are taller and longer-legged than those in the higher grounds."*

The limits allotted to this article are nearly reached, yet only four of the fourteen chapters of the volume have been touched. These, however, contain the fundamental principles of the theory and most of those applications of it which are capable of something like verification, relating as they do to phenomena now occurring. Some of our extracts also show how these principles are thought to have operated through the long lapse of the ages. The chapters from the sixth to the ninth inclusive are designed to obviate difficulties and objections, "some of them so grave that to this day," the author frankly says, he "can never reflect on them without being staggered." We do not wonder at it. After drawing what comfort he can from "the imperfection of

* Quadrupeds of America, ii, p. 239.

the geological record" (chap. 9), which we suspect is scarcely exaggerated, the author considers the geological succession of organic beings (chap. 10), to see whether they better accord with the common view of the immutability of species, or with that of their slow and gradual modification. Geologists must settle that question. Then follow two most interesting and able chapters on the geographical distribution of plants and animals, the summary of which we should be glad to cite; then a fitting chapter upon classification, morphology, embryology, &c., as viewed in the light of this theory, closes the argument; the fourteenth chapter being a recapitulation.

The interest for the general reader heightens as the author advances on his perilous way and grapples manfully with the most formidable difficulties.

To account, upon these principles, for the gradual elimination and segregation of nearly allied forms,—such as varieties, subspecies, and closely related or representative species,—also in a general way for their geographical association and present range, is comparatively easy, is apparently within the bounds of possibility, and even of probability. Could we stop here we should be fairly contented. But, to complete the system, to carry out the principles to their ultimate conclusion, and to explain by them many facts in geographical distribution which would still remain anomalous, Mr. Darwin is equally bound to account for the formation of genera, families, orders, and even classes, by natural selection. He does "not doubt that the theory of descent with modification embraces all the members of the same class," and he concedes that analogy would press the conclusion still farther; while he admits that "the more distinct the forms are, the more the arguments fall away in force." To command assent we naturally require decreasing probability to be overbalanced by an increased weight of evidence. An opponent might plausibly, and perhaps quite fairly, urge that the links in the chain of argument are weakest just where the greatest stress falls upon them.

To which Mr. Darwin's answer is, that the best parts of the testimony have been lost. He is confident that intermediate forms must have existed; that in the olden times when the genera, the families and the orders diverged from their parent stocks, gradations existed as fine as those which now connect closely related species with varieties. But they have passed and left no sign. The geological record, even if all displayed to view, is a book from which not only many pages, but even whole alternate chapters have been lost out, or rather which were never printed from the autographs of nature. The record was actually made in fossil lithography only at certain times and under certain conditions (i. e., at periods of slow subsidence and places of

abundant sediment); and of these records all but the last volume is out of print; and of its pages only local glimpses have been obtained. Geologists, except Lyell, will object to this,—some of them moderately, others with vehemence. Mr. Darwin himself admits, with a candor rarely displayed on such occasions, that he should have expected more geological evidence of transition than he finds, and that all the most eminent palæontologists maintain the immutability of species.

The general fact, however, that the fossil fauna of each period as a whole is nearly intermediate in character between the preceding and the succeeding faunas, is much relied on. We are brought one step nearer to the desired inference by the similar "fact, insisted on by all palæontologists, that fossils from two consecutive formations are far more closely related to each other, than are the fossils of two remote formations. Pictet gives a well-known instance,—the general resemblance of the organic remains from the several stages of the chalk formation, though the species are distinct at each stage. This fact alone, from its generality seems to have shaken Professor Pictet in his firm belief in the immutability of species." (p. 335.) What Mr. Darwin now particularly wants to complete his inferential evidence is a proof that the same gradation may be traced in later periods, say in the tertiary, and between that period and the present; also that the later gradations are finer, so as to leave it doubtful whether the succession is one of species,—believed on the one theory to be independent, on the other, derivative,—or of varieties, which are confessedly derivative. The proof of the finer gradation appears to be forthcoming. Des Hayes and Lyell have concluded that many of the middle tertiary, and a large proportion of the later tertiary mollusca are specifically identical with living species; and this is still the almost universally prevalent view. But Mr. Agassiz states that, "in every instance where he had sufficient materials, he had found that the species of the two epochs supposed to be identical by Des Hayes and Lyell were in reality distinct, although closely allied species."* Moreover he is now satisfied, as we understand, that the same gradation is traceable not merely in each great division of the tertiary, but in particular deposits or successive beds, each answering to a great number of years; where what have passed unquestioned as members of one species, upon closer examination of numerous specimens exhibit differences which in his opinion entitle them to be distinguished into two, three, or more species. It is plain, therefore, that whatever conclusions can be fairly drawn from the present animal and vegetable kingdoms in favor of a gradation of varieties into species, or into what

* Proceedings of the American Academy of Arts and Sciences, iv, p. 178.

may be regarded as such, the same may be extended to the tertiary period. In both cases, what some call species others call varieties; and in the later tertiary shells this difference in judgment affects almost half of the species!

We pass to a second difficulty in the way of Mr. Darwin's theory; to a case where we are perhaps entitled to demand of him evidence of gradation like that which connects the present with the tertiary mollusca. Wide, very wide is the gap, anatomically and physiologically (we do not speak of the intellectual) between the highest quadrumana and man; and comparatively recent, if ever, must the line have bifurcated. But where is there the slightest evidence of a common progenitor? Perhaps Mr. Darwin would reply by another question: where are the fossil remains of the men who made the flint knives and arrow-heads of the Somme valley?

We have a third objection, one, fortunately, which has nothing to do with geology. We can only state it here, in brief terms. The chapter on hybridism is most ingenious, able, and instructive. If sterility of crosses is a special, original arrangement to prevent the confusion of species by mingling, as is generally assumed, then, since varieties cross readily and their offspring is fertile *inter se*, there is a fundamental distinction between varieties and species. Mr. Darwin therefore labors to show that it is not a special endowment, but an incidental acquirement. He does show that the sterility of crosses is of all degrees;—upon which we have only to say, *Natura non facit saltum*, here any more than elsewhere. But, upon his theory he is bound to show how sterility might be acquired, through natural selection or through something else. And the difficulty is, that, whereas individuals of the very same blood tend to be sterile, and somewhat remoter unions diminish this tendency, and when they have diverged into two varieties the cross-breeds between the two are more fertile than either pure stock,—yet when they have diverged only one degree more the whole tendency is reversed, and the mongrel is sterile, either absolutely or relatively. He who explains the genesis of species through purely natural agencies should assign a natural cause for this remarkable result; and this Mr. Darwin has not done. Whether original or derived, however, this arrangement to keep apart those forms which have, or have acquired (as the case may be) a certain moderate amount of difference, looks to us as much designed for the purpose, as does a ratchet to prevent reverse motion in a wheel. If species have originated by divergence, this keeps them apart.

Here let us suggest a possibly attainable test of the theory of derivation, a kind of instance which Mr. Darwin may be fairly asked to produce,—viz., an instance of two varieties, or what may

be assumed as such, which have diverged enough to reverse the movement, to bring out some sterility in the crosses. The best marked human races might offer the most likely case. If mulattoes are sterile or tend to sterility, as some naturalists confidently assert, they afford Mr. Darwin a case in point. If, as others think, no such tendency is made out, the required evidence is wanting.

A fourth and the most formidable difficulty is that of the production and specialization of organs.

It is well said that all organic beings have been formed on two great laws; Unity of type, and Adaptation to the conditions of existence.* The special teleologists, such as Paley, occupy themselves with the latter only; they refer particular facts to special design, but leave an overwhelming array of the widest facts inexplicable. The morphologists build on unity of type, or that fundamental agreement in the structure of each great class of beings, which is quite independent of their habits or conditions of life; which requires each individual "to go through a certain formality," and to accept, at least for a time, certain organs, whether they are of any use to him or not. Philosophical minds form various conceptions for harmonizing the two views theoretically. Mr. Darwin harmonizes and explains them naturally. Adaptation to the conditions of existence is the result of Natural Selection; Unity of type, of unity of descent. Accordingly, as he puts his theory, he is bound to account for the origination of new organs, and for their diversity in each great type, for their specialization, and every adaptation of organ to function and of structure to condition, through natural agencies. Whenever he attempts this he reminds us of Lamarck, and shows us how little light the science of a century devoted to structural investigation has thrown upon the mystery of organization. Here purely natural explanations fail. The organs being given, natural selection may account for some improvement; if given of a variety of sorts or grades, natural selection might determine which should survive and where it should prevail.

On all this ground the only line for the theory to take is to make the most of gradation and adherence to type as suggestive of derivation, and unaccountable upon any other scientific view, —deferring all attempts to explain *how* such a metamorphosis was effected, until naturalists have explained *how* the tadpole is metamorphosed into a frog, or one sort of polyp into another. As to *why* it is so, the philosophy of efficient cause, and even the whole argument from design, would stand, upon the admission of such a theory of derivation, precisely where they stand without it. At least there is, or need be, no ground of differ-

* Owen adds a third, viz:—Vegetative Repetition; but this, in the vegetable kingdom is simply Unity of Type.

ence here between Darwin and Agassiz. The latter will admit, with Owen and every morphologist, that hopeless is the attempt to explain the similarity of pattern in members of the same class by utility or the doctrine of final causes. "On the ordinary view of the independent creation of each being, we can only say that so it is, that it has so pleased the Creator to construct each animal and plant." Mr. Darwin, in proposing a theory which suggests a *how* that harmonizes these facts into a system, we trust implies that all was done wisely, in the largest sense designedly, and by an Intelligent First Cause. The contemplation of the subject on the intellectual side, the amplest exposition of the Unity of Plan in Creation, considered irrespective of natural agencies, leads to no other conclusion.

We are thus, at last, brought to the question; what would happen if the derivation of species were to be substantiated, either as a true physical theory, or as a sufficient hypothesis? What would come of it? The enquiry is a pertinent one, just now. For, of those who agree with us in thinking that Darwin has not established his theory of derivation, many will admit with us that he has rendered a theory of derivation much less improbable than before; that such a theory chimes in with the established doctrines of physical science, and is not unlikely to be largely accepted long before it can be proved. Moreover, the various notions that prevail,—equally among the most and the least religious,—as to the relations between natural agencies or phenomena and Efficient Cause, are seemingly more crude, obscure, and discordant than they need be.

It is not surprising that the doctrine of the book should be denounced as atheistical. What does surprise and concern us is, that it should be so denounced by a scientific man, on the broad assumption that a material connection between the members of a series of organized beings is inconsistent with the idea of their being intellectually connected with one another through the Deity, i. e., as products of one mind, as indicating and realizing a preconceived plan. An assumption the rebound of which is somewhat fearful to contemplate, but fortunately one which every natural birth protests against.

It would be more correct to say, that the theory in itself is perfectly compatible with an atheistic view of the universe. That is true; but it is equally true of physical theories generally. Indeed, it is more true of the theory of gravitation, and of the nebular hypothesis, than of the hypothesis in question. The latter merely takes up *a particular, proximate cause*, or set of such causes, from which, it is argued, the present diversity of species has or may have *contingently* resulted. The author does not say *necessarily* resulted; that the actual results in mode and measure, and none other must have taken place. On the other

hand the theory of gravitation, and its extension in the nebular hypothesis, assume a *universal and ultimate* physical cause, from which the effects in nature must *necessarily* have resulted. Now it is not thought, at least at the present day, that the establishment of the Newtonian theory was a step towards atheism or pantheism. Yet the great achievement of Newton consisted in proving that certain forces, (blind forces, so far as the theory is concerned,) acting upon matter in certain directions, must *necessarily* produce planetary orbits of the exact measure and form in which observation shows them to exist;—a view which is just as consistent with eternal necessity, either in the atheistic or the pantheistic form, as it is with theism.

Nor is the theory of derivation particularly exposed to the charge of the atheism of fortuity; since it undertakes to assign real causes for harmonious and systematic results. But of this, a word at the close.

The value of such objections to the theory of derivation may be tested by one or two analogous cases. The common scientific as well as popular belief is that of the original, independent creation of oxygen and hydrogen, iron, gold, and the like. Is the speculative opinion, now increasingly held, that some or all of the supposed elementary bodies are derivative or compound, developed from some preceding forms of matter, irreligious? Were the old alchemists atheists as well as dreamers in their attempts to transmute earth into gold? Or, to take an instance from force (power),—which stands one step nearer to efficient cause than form—was the attempt to prove that heat, light, electricity, magnetism, and even mechanical power are variations or transmutations of one force, atheistical in its tendency? The supposed establishment of this view is reckoned as one of the greatest scientific triumphs of this century.

Perhaps, however, the objection is brought, not so much against the speculation itself, as against the attempt to show how derivation might have been brought about. Then the same objection applies to a recent ingenious hypothesis made to account for the genesis of the chemical elements out of the etherial medium, and to explain their several atomic weights and some other characteristics by their successive complexity,—hydrogen consisting of so many atoms of etherial substance united in a particular order, and so on. The speculation interested the philosophers of the British Association, and was thought innocent, but unsupported by facts. Surely Mr. Darwin's theory is none the worse, morally, for having some foundation in fact.

In our opinion, then, it is far easier to vindicate a theistic character for the derivative theory, than to establish the theory itself upon adequate scientific evidence. Perhaps scarcely any philosophical objection can be urged against the former to which

the nebular hypothesis is not equally exposed. Yet the nebular hypothesis finds general scientific acceptance, and is adopted as the basis of an extended and recondite illustration in Mr. Agassiz's great work.*

How the author of this book harmonizes his scientific theory with his philosophy and theology, he has not informed us. Paley, in his celebrated analogy with the watch, insists that if the time-piece were so constructed as to produce other similar watches, after the manner of generation in animals, the argument from design would be all the stronger. What is to hinder Mr. Darwin from giving Paley's argument a further *a-fortiori* extension to the supposed case of a watch which sometimes produces better watches, and contrivances adapted to successive conditions, and so at length turns out a chronometer, a town-clock, or a series of organisms of the same type? From certain incidental expressions at the close of the volume, taken in connection with the motto adopted from Whewell, we judge it probable that our author regards the whole system of nature as one which had received at its first formation the impress of the will of its Author, foreseeing the varied yet necessary laws of its action throughout the whole of its existence, ordaining when and how each particular of the stupendous plan should be realized in effect, and—with Him to whom to will is to do—in ordaining doing it. Whether profoundly philosophical or not, a view maintained by eminent philosophical physicists and theologians, such as Babbage on the one hand and Jowett on the other, will hardly be denounced as atheism. Perhaps Mr. Darwin would prefer to express his idea in a more general way, by adopting the thoughtful words of one of the most eminent naturalists of this or any age, substituting the word *action* for 'thought,' since it is the former (from which alone the latter can be inferred) that he has been considering. "Taking nature as exhibiting thought for my guide, it appears to me that while human thought is consecutive, Divine thought is simultaneous, embracing at the same time and forever, in the past, the present and the future, the most diversified relations among hundreds of thousands of organized beings, each of which may present complications again, which, to study and understand even imperfectly,—as for instance man himself—mankind has already spent thousands of years."† In thus conceiving of the Divine Power in act as cöetaneous with Divine Thought, and of both as far as may be apart from the human element of time, our author may regard the intervention of the Creator either as, humanly speaking, *done from all time*, or else as *doing through all time*. In the ultimate analysis we suppose that every philosophical theist must adopt one or the other conception.

* Contrib. Nat. Hist. Amer., i, p. 127-131. † Op. cit., p. 130.

A perversion of the first view leads towards atheism, the notion of an eternal sequence of cause and effect, for which there is no first cause,—a view which few sane persons can long rest in. The danger which may threaten the second view is pantheism. We feel safe from either error, in our profound conviction that there is order in the universe; that order presupposes mind; design, will; and mind or will, personality. Thus guarded, we much prefer the second of the two conceptions of causation, as the more philosophical as well as Christian view,—a view which leaves us with the same difficulties and the same mysteries in Nature as in Providence, and no other. Natural law, upon this view, is the human conception of continued and orderly Divine action.

We do not suppose that less power, or other power, is required to sustain the universe and carry on its operations, than to bring it into being. So, while conceiving no improbability of "interventions of Creative mind in nature," if by such is meant the bringing to pass of new and fitting events at fitting times, we leave it for profounder minds to establish, if they can, a rational distinction in kind between His working in nature carrying on operations, and in initiating those operations.

We wished under the light of such views, to examine more critically the doctrine of this book, especially of some questionable parts;—for instance, its explanation of the natural development of organs, and its implication of a "necessary acquirement of mental power" in the ascending scale of gradation. But there is room only for the general declaration that we cannot think the Cosmos a series which began with chaos and ends with mind, or of which mind is a result: that if by the successive origination of species and organs through natural agencies, the author means a series of events which succeed each other irrespective of a continued directing intelligence,—events which mind does not order and shape to destined ends,—then he has not established that doctrine, nor advanced towards its establishment, but has accumulated improbabilities beyond all belief. Take the formation and the origination of the successive degrees of complexity of eyes as a specimen. The treatment of this subject (pp. 188, 189), upon one interpretation is open to all the objections referred to; but if, on the other hand, we may rightly compare the eye "to a telescope, perfected by the long continued efforts of the highest human intellects," we could carry out the analogy, and draw satisfactory illustrations and inferences from it. The essential, the directly intellectual thing is the making of the improvements in the telescope or the steam-engine. Whether the successive improvements, being small at each step, and consistent with the general type of the instrument, are applied to some of the individual machines, or entire new ma-

chines are constructed for each, is a minor matter. Though if machines could engender, the adaptive method would be most economical; and economy is said to be a paramount law in nature. The origination of the improvements, and the successive adaptations to meet new conditions or subserve other ends, are what answer to the supernatural, and therefore remain inexplicable. As to bringing them into use, though wisdom foresees the result, the circumstances and the natural competition will take care of that, in the long run. The old ones will go out of use fast enough, except where an old and simple machine remains still best adapted to a particular purpose or condition,—as, for instance, the old Newcomen engine for pumping out coal-pits. If there's a Divinity that shapes these ends, the whole is intelligible and reasonable; otherwise, not.

We regret that the necessity of discussing philosophical questions has prevented a fuller examination of the theory itself, and of the interesting scientific points which are brought to bear in its favor. One of its neatest points, certainly a very strong one for the local origination of species, and their gradual diffusion under natural agencies, we must reserve for some other convenient opportunity.

The work is a scientific one, rigidly restricted to its direct object; and by its science it must stand or fall. Its aim is, probably not to deny creative intervention in nature,—for the admission of the independent origination of certain types does away with all antecedent improbability of as much intervention as may be required,—but to maintain that Natural Selection in explaining the facts, explains also many classes of facts which thousand-fold repeated independent acts of creation do not explain, but leave more mysterious than ever. How far the author has succeeded, the scientific world will in due time be able to pronounce.

As these sheets are passing through the press a copy of the second edition has reached us. We notice with pleasure the insertion of an additional motto on the reverse of the title-page, directly claiming the theistic view which we have vindicated for the doctrine. Indeed these pertinent words of the eminently wise Bishop Butler, comprise, in their simplest expression, the whole substance of our latter pages:—

"The only distinct meaning of the word 'natural' is *stated, fixed*, or *settled;* since what is natural as much requires and presupposes an intelligent mind to render it so, i. e., to effect it continually or at stated times, as what is supernatural or miraculous does to effect it for once." A. G.

ART. XVI.—*Forces;* by THEODORE LYMAN.

THE first article in this Journal for November last* brings to mind the singular part which "force" now plays in science. The theory set forth in that article may be stated as follows: the world, and everything on it, may be considered as *matter;* this matter is not the same throughout, but consists of a certain number of ultimate species called *elements;* these elements are not always isolated, but are found joined to form, 1st, simple compounds, known sometimes as *minerals;* 2d, compounds of a nature higher, more complicated, and differently characterized, known as *vegetables;* 3d, compounds still higher and more complicated, and again differently characterized, known as *animals.* As the elements do not remain isolated, so also their compounds continually change their mutual relations; and the result of these changes is that continual falling down and building up which may be seen in the material world. To move these elements and their compounds there is a fund of *force,* constant in quantity and in quality; if ever it seems to be less in quantity, some of it is latent; if ever it seems different in quality, it is but changed in appearance, from being connected with some peculiar compound. Here is Cosmos at a glance!—there is the force, the mover, a; and these are the elements, the things moved, b, c, d, e, &c.—a may be a' (mechanical force), or a'' (chemical force), or a''' (vegetable force), &c., but still it remains a. a may act on b, c, d, e, and there may result such compounds as be, ceb, dec, &c. When a joins b to c, a part of a becomes *latent,* and the result may be called $bc+a$; but, when this compound is decomposed by a *different form* of a (e. g. a'''' or light) then a latent is *set free,* and immediately takes b and joins it to d, e, making the *higher compound* bde, while c is set free as an element. To give an instance, if b is carbon, c hydrogen, and d oxygen, and a''' is vegetable force, then b, c, and d, joined by the action of a''', would be the compound bcd, and might be a turnip. This theory looks simple, but its very roundness is suspicious.

The human mind, craving something more than mere fact, has tried to get at the *reason.* The fact is the law, the reason is the cause. It is in the search for the latter that scientific men have fallen on that unfortunate word, that shadow of a shadow, that last resort of ignorance—*Force!* It is safe to say that no

* The instructive and ingenious essay, by Prof. Joseph LeConte, is quoted in no invidious spirit, but simply as a fair sample of a philosophy now very common. Prof. LeConte may contend that he uses the word "force" only as a convenient supposition, on which to build a theory; if this be the case, it should be remembered that a form of expression which is sure to mislead ninety-nine readers in every hundred, should not be used in scientific writing.

word in the English language has created so much ambiguity, so much meaningless discussion, and so much wrong-headed philosophy, as this ill begotten monosyllable. It is a gag for inquisitive people. What keeps the world from flying off in space? The force of attraction. What keeps it from rushing towards the sun? The centrifugal force. What makes oxyd of lead join acetic acid? The chemical force. What makes oxyd of lead *leave* acetic acid and go to sulphuric acid? The chemical force again. What makes a fish with a ventral fin? The vital force. What makes a fish *without* a ventral fin? The vital force again. Mark this! These forces are blind and they are the same always, yet they make different things, on the one hand, while on the other, they repeat the same thing over and over again. How is this? Can a round auger bore a square hole? Or, can it bore any hole at all without *intellect* to guide it? If several forces are not enough to account for the phenomena of the world, what can we expect of *one* force? Yet to this Prof. LeConte would reduce us. His "correlation of forces" is one force; and his "conservation of forces" is the amount of this force. The *amount of force*, acting on the *sum* of the *material elements*, produces all motion and all being. The force is spoken of as "latent," "set free," "liberated," and "developed," and as being "furnished" and "supplied," by decomposition.

Force is either something or nothing, if it be nothing, it is not worthy a place in science; if it be something it may be considered like any other fact of philosophy. What, then, is force and whence comes our idea of it? To answer these questions it is necessary to define, 1st, true causation; 2d, variable sequences; 3d, invariable sequences. The operations of my own will are the only instances of true causation of which I am conscious. The will, itself immaterial, works either on the material body, or on the immaterial mind. When the body is free and healthy the will has direct control over the voluntary muscles, but none over the involuntary. On the other hand, when the mind is free and healthy, the will has direct control over the whole of it. Whenever I will to do an act, mental or physical, I am *conscious* of using power. *Causation, then, is the act of x, using power to produce y.*

We may, in the outer world, see one thing happen immediately after another; yet, in a little while, we may see the first thing again, but this time *not* followed by the second. In such a case we say that the fact, that one thing followed another, was an accident. For instance, twelve mines of powder may explode, one after another, but probably this will not happen so again. *A variable sequence, then, is the fact that y has followed x, but will not, of necessity, do so again.*

We may, in the outer world, see a particular thing, which, as far as our experience goes, is always followed by another par-

ticular thing; for example, the twelve mines above mentioned are fired by electricity, and, as far as we know, a certain amount of electric heat in contact with gunpowder is followed by an explosion. *An invariable sequence, then, is the fact, that y always follows x.* To sum up; a man, by conscious exercise of will, discharged the battery (*efficient causation*); the electric spark was followed by an explosion of gunpowder (*invariable sequence*); certain other mines happened to explode, after this one, in quick succession (*variable sequence*).

The admission of universal, invariable sequence (i. e., every x is invariably followed by its peculiar y) is the ground-principle of the theory of necessity, which leads directly to pantheistic atheism, with its idealistic and materialistic branches. The sole object of science thus becomes, to find out all the xs in the universe and all the ys, and to give to each x its proper y. All these, when tabulated so as to show their fixed relations to each other, make up the course of nature, and human knowledge has then done its utmost and has nothing left to work on. This course of nature, if admitted as entirely true, may still be looked at in two ways: 1st, it may be said that this course is such as it is by reason of an *essence* which exists in matter; or, 2d, it may be stated, that the course is such as it is, and that no reason therefor can be given, or ought to be given. Such views as these might be allowable if all our knowledge were got from observations on the *outer* world, and without reference to our own consciousness of the structure of our minds; for we could not prove the existence of *power*, in the phenomena of the outer world; and, if we suspected it, we might still set it down as an inherent property of matter. But the moment *consciousness of self* is brought into the question, a new element is introduced, namely, *intelligence.* Consciousness is fundamental, and cannot be denied; consciousness of thought, that is intelligence, cannot be denied; consciousness of *power* has been denied, and with what truth? I am fully conscious, that I turn my mind to a given thought, and I sharply distinguish this act of *causation* from any *sequence*; here then is the immaterial will exercising *power* over the immaterial mind. If there is no such thing as power, whence comes the word, and whence the general idea of it? Again, I will to move some part of my body, and do move it, and I compare this power with that used in mental action, and I find them the same. There is this difference between the cases: in the first instance I move what is strictly myself, the unit, the immaterial; in the second instance I move what is only a frame, over which I have limited control; the compound, the material. And observe this; there is a difference between merely *wishing* and the act of *exerting power consciously.* I may wish that my legs would, all at once, walk off with my body;

but, if this should take place, I should be conscious that I was not exerting the power. Again, if, at my mere wish, a great rock should come to me, I should be conscious that something else moved it, and not myself. And, if my power of will was so extended, that I could thereby move rocks, I should be just as conscious of that power as I now am of the power to move my own body. A wish, followed by the thing wished for, is a sequence; a command, followed by the thing commanded, is power or causation. It is therefore wrong to say, that such sequences, if observed many times, would give me the idea that *I* exercised the power. When ignorant people attribute true causation to material bodies, (e. g., in the case of electricity,) they do so because they have the consciousness of power in themselves, and they attribute the same property to a body, which has in itself no power, but which shows signs of being acted upon by a power. If now it be said that matter has a certain essence, property, or what not, which makes it to do certain things at certain times, what is this essence or property but *power?* From our very idea of the word, anything that produces motion in another thing, either has or uses power. Still, though there may be power, is it *intelligent?* or do there exist a number of *un*intelligent powers, which so limit the scope of each other's action as to make a balance in the course of nature? (e. g., the power of centrifugal force balanced by the power of gravitation; the power of oxygen, produced by plants, balanced by the carbon, produced by animals; the power of insects, balanced by the birds that feed on them; the power of fruits, balanced by the parasitic fungi that grow on them.) To such a query, science is able to make this answer: "The phenomena observed in the outer world (*non-ego*) correspond to the phenomena of self (*ego*), which are produced by *intellect.*"* This is a sound induction; if it be not good, then no induction is good; and, if induction cannot stand, science must fall. To sum up, if it be admitted that there is an essence, inherent in matter, which necessitates its actions, then, 1st, this essence is *power;* and 2d, this power is *intelligent;* for, to deny that the essence was power would be to stultify our understanding, and, to deny this power to be intelligent, would be to ignore induction and to destroy science. It may be well also to take notice of the fact, that most theories, savoring of materialism, speak of some ultimate essence (force) which is at the bottom of all motion and action, as if such essence were fundamental and satisfactory; but this, after all, does not help the theory, for, if this essence is true matter, it moves, first *itself*, and then other matter, and, if it is a property, then this property moves first *itself* and then matter.

* For a full consideration of intellect, as shown in nature, see Essay on Classification, by Louis Agassiz.

There still remains the second way of looking at the course of nature: it is such as it is, and no reason therefor can be given. What is the exact meaning of this statement? Simply that the upholder of the doctrine resolves, so far as concerns the gaining of truth, to put trust in the testimony of his eyes, ears, touch, and other physical senses, and in nothing else. He is a strict positivist; he says: "I see x, feel y, and hear z; to me, then, x, y, and z exist as things seen, felt and heard, respectively. To me there is no causation, for I do not directly perceive it. I believe in memory, it tells me what I have formerly seen; I believe in the axiom, *cogito ergo sum*, because the very denial of thought implies the action of thought. My duty is to clearly understand and to tabulate all the observations I have made; but there it stops; I may make many inductions and may use them for convenience, but am not sure of them. One induction tells me the sun will rise to-morrow: for my convenience I say it will; but of this I am not sure; perhaps it will *not*:—it is a thing of the future and the future I cannot see. I attempt to explain nothing:—*you* attempt to explain, and you come to no certain results:—I am more modest. By my individual observation I know certain things, and all other things are to me only as what may be. The earth may, or may not, turn again on its axis; causation may, or may not, exist; there may, or may not, be a God."

The holder of this theory, when compared with the believers in kindred opinions, is, in a certain sense, consistent; because he only makes statements, which are true as far as they go; and does not attempt explanations, which must in the end prove unsatisfactory. But his theory is incomplete and therefore one-sided and untrue. On the one hand, he acknowledges consciousness, and through consciousness believes in all the phenomena of the *outer* world (*non-ego*) as isolated facts; while, on the other, he ignores the properties of *self* (*ego*) which are also given by consciousness, and are as reliable as any of its data. Thus, he acknowledges the motions of his voluntary muscles, of which he is conscious, but refuses to acknowledge his own power to move them, or to leave them at rest, of which he is also conscious. We need go no further than the idea of *intelligent power*, to find the whole trouble in this, and in many other theories.*

What interest does a true conception of the ever-working Creative Intellect give to science! This correspondence of the human with the Divine mind! The astronomer works out, with pencil and paper, the possible answers to a certain problem of

* For able presentations of the doctrines of free-will and of necessity, consult the writings of Prof. Francis Bowen, and of J. S. Mill. The idea of causation has been carried to its last analysis by Sir William Hamilton.

motion; he looks at the heavens, and there sees these answers, illustrated in the orbits of celestial bodies. The zoölogist, marking the changes of the embryo, thinks of these changes as so many different animals; deep in the rocks he finds all stages of this embryo, each represented by a species, perfect in its kind!

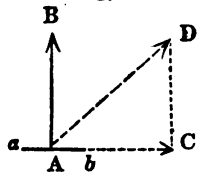
On the other hand, how dead the science, that puts "force" as its first cause! What is this force that makes the star-fish and the oyster, the medusa and the cuttle-fish, the crab and the whale, the tufted sea worm and the shark, each in its kind, and each telling its own story of manifold relations with animal creation, that is, that has been, and that is to come? Nature is no such simple thing that she should be dictated to by light, or heat, or electricity. These are her servants, not her masters!

Boston, Nov., 1859.

ART. XVII.—*On the causes of deviation in Elongated Projectiles;* by Maj. J. G. BARNARD, Corps of Engineers, U. S. A.

THE various and somewhat conflicting explanations given of the deviation of projectiles, both spherical and elongated, arising from their own rotary motions, leave room for a few additional words on this subject.

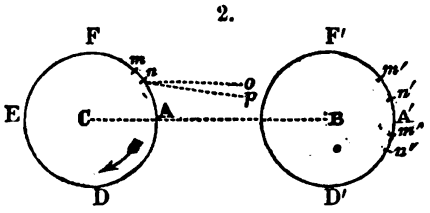
1.



If the plane surface ab moves, in an elastic medium in the direction of its normal, with a velocity A B, that medium will oppose a force to which we apply the term "resistance" and which is measured by a function of the velocity A B.

If, at the same time, the surface has a velocity A C, *in its own plane*, the result will be an actual velocity of *each point of the surface*, represented by the diagonal A D; but the velocity of impact of the surface with the air, is the same in both cases, being due only to the normal velocity A B. The motion A C in its own plane, would displace, in no degree, the atmospheric particles, (except through the agency of that action known as friction—not now considered,) and would therefore generate no component of "resistance."

2.



If the sphere, whose great circle is A D E F, move through the air in the direction C B, with a velocity V, a resistance will be opposed to its motion which will, in magnitude, be a function of the diameter, and of the velocity V. If we leave out of consideration the force of *friction*, the character and intensity of the impact of the sphere with the

air will be identically the same, whether it possesses or not rotary motion: for in either case, the surface, considered *as a whole*, advances in identically the same manner—the displacement of atmospheric particles is the same, and the resulting resistance, the same.

Let the rotation be supposed about a horizontal axis, perpendicular to the line of flight, and in the direction A D. The velocity of the *individual points*, m, n—or if you choose—*elementary surface*, $m\,n$, will be the resultant of the rotary and translative velocities, and the little surface $m\,n$, instead of moving (at the instant) in the direction $n\,o$, will move in an oblique direction $n\,p$. But the *rotary* component of velocity lies in the plane of this elementary surface, and has, (as in the case of the *lateral* velocity A C of the plane, Fig. 1) no agency whatever in displacing the air, or in affecting the intensity or character of its impact.

These considerations will, perhaps, be rendered more clear by reflecting that the resistance of a fluid, is due and *due only* to the *displacement of its particles*—that when the centre of the sphere has advanced from C to B, the anterior surface has advanced from F A D to F′ A′ D′, and displaced the air in identically the same manner, whether the sphere revolves or not.

These considerations are so obvious that it seems superfluous to insist on them; yet few of the writers on this subject have exhibited a clear understanding of them; or rather it may be said that they exhibit the reverse.

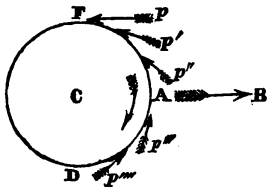
Thiroux, rejecting friction entirely, or rather considering its effects inappreciable, bases his reasoning on the higher velocity with which the points of the surface on the side A F *impinge on the air*, over that belonging to points on the side A D; an idea, as just shown, entirely fallacious. Capt. Neumann (Prussian artillery), in a theory as pretentious as it is unmeaning (Delobel's *Révue de Technologie Militaire*, vol. i.), carries this absurdity to the extreme of considering each elementary surface $m\,n$, *separately*, with its combined motion of translation and rotation, and, applying to each the ordinary expression for resistance of a plane surface impinging obliquely upon an elastic medium, *integrates* through each half of the anterior surface, to obtain the total action on each side.

Not only are the conventional expressions for the resistance of isolated oblique plane surfaces found most inaccurate in practice, but they lose *all* applicability when they cease to be *isolated*, and form part of another larger surface (not plane); and this problem, of which the knot is so expertly cut by Capt. Neumann, who proceeds to apply his results to the criticism or *test* of Magnus' and other theories, is the very "pièce de résistance" which has defied the analysis of d'Alembert, Poisson and Poncelet—per-

haps I might add of Newton and Laplace; one of those problems of mechanics to which the term *difficult* would be misapplied, for analysis has never yet been able to grasp it at all.

I have said that without the consideration of *friction*, the action upon the air of a rotating and non-rotating ball are identically the same. But *friction* materially alters the character of this action. Whatever may be the immediate cause of this force —whether simply a collision of the inequalities of the surface with the particles of the fluid—or whether it is due to *adhesion*, the effect is that the moving surface puts in motion *with it*, the adjacent fluid particles, and in so doing, developes forces tangential and opposed to its own motion.

3.

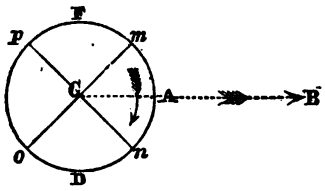


Thus the anterior surface of the sphere C, revolving from F to D, and advancing from A to B, creates, at each point, forces, p, p', p'' &c., tangential and opposed to its rotary motion, the resultant of which is a force acting from D towards F and tending to deflect the flight of the ball in that direction. This is the point of view, *and the sole one,* in which Poisson has considered the effects of friction.

But there is *another* effect which proves to be very powerful. Force cannot be applied to an elastic fluid, neither can motion be imparted or destroyed, without effecting, at the same time, its density and *pressure*. To retard a flowing current is to increase its pressure; to accelerate it is to diminish the same.

Applying this to the ball, the air, displaced and compressed in front, escapes along the surfaces A F and A D. Near its surface, the action of friction is to retard the escaping currents on the side A F, and to accelerate them on the side A D, and in consequence, an increase of pressure ensues on the side A F, and a diminution on the side A D; and therefore, a resulting pressure tending to deflect the ball from F towards D. If we divide the great circle A D F into four quadrants by the lines $m o$ and $n p$, drawn at angles of 45° with the direction of translation A B, we may better analyze the effects of friction, in the two forms in which I have presented them.

4.



The posterior quadrant $o p$ is in air so highly rarified that its action is insensible or nearly so.* On the side quadrant $m p$ the resultant of the forces of friction (the forces p, p', &c., of Fig. 3) are parallel (or nearly so) and *opposed* to the motion of translation. They have no effect (or but trifling) to *deflect* the ball from its course, but acting upon the air, in direct opposition to

* The *high velocities* of translation of military projectiles is supposed.

the escaping currents, their whole force is expended in destroying velocity and generating *pressure*. On the anterior quadrant *m n* the resultant of the forces *p*, *p'*, &c., is from *n* towards *m*, and tends, almost entirely, to deflect the ball in that direction.

On the quadrant *n o* the resultant of the forces *p*, *p'*, is parallel to the motion of translation, and co-incident in direction with the escaping current whose motion it accelerates and whose *pressure* it diminishes. Thus, taking the four quadrants, in *one*, *o p*, the forces of friction are absent; in *two*, *m p* and *n o*, they are expended in producing an *inequality of pressure* on the two sides of the ball, tending to deflect the ball towards the side D (right); while in the anterior quadrant *m n*, they act to deflect the ball in the opposite direction F (left).

It would have been difficult to decide *a priori*, which of these forces would prevail, though, while the force of friction is nugatory in *one* quadrant, in two (*m p* and *o n*) it expends itself in developing forces tending to deflect to the right, and in only *one*, *m n*, does its direct action tend to deflect to the left; yet it must be remarked that in this quadrant the air is most dense, friction the greatest, and that it acts *directly* upon the projectile.

In the two lateral quadrants the air is less dense, and it is only through pressures *developed in the air* that it produces its effect; a loss of effect ensuing in the medium through which it acts.

Experience has shown, however, that the forces developed in the two lateral quadrants prevail, and the projectile is deflected to the right; and the experiments of Dr. Magnus give the same result when, instead of a projectile moving through the air, a *current* of air is directed upon a revolving cylinder.*

The deviation of elongated projectiles, having rotary motion about their axis of figure, though many authors, Thiroux, Panôt, Tamissier, &c., have attempted to refer it to the same causes which produce the deviation in spherical balls, is evidently governed by other causes.

Not only do such writers have to make, as to the direction which the axis maintains, assumptions which conflict with each

* Of course the division into quadrants which I have made is arbitrary, and only used as a simple means of illustrating how the conflicting effects are produced from one and the same *cause*. I comprehend under the term *friction*, all the forces by which a solid *surface* acts upon a fluid flowing along it, whether by adhesion or simple mechanical collision of particles; and this is the usual meaning of the word in this connection. Poisson has considered friction only in its direct action, i. e., as the resultant of the forces *p*, *p'*, &c., of Fig. 3, and, deducing therefrom a deviation to the left, he arrives at the conclusion that the magnitude of the force was not sufficient to account for the *amount* of observed deviation (irrespective, I presume, of *direction*). If this is true, it is difficult to conceive that the force (overlooked by him) arising from developed *pressures* (and which I show to have its origin in friction), is sufficient to annihilate the *direct* effects of friction itself, and to produce the deviations beside. Nevertheless it is all we can account for, and the experiments of Magnus seem to indicate its adequacy.

other,* but the causes they assign are inadequate, and, moreover, as the deviation of an elongated ball depends more on the direction in which its axis happens to be deflected than upon the direction of the disturbing forces, no such uniformity in the deviations as is observed, would be produced, were friction on the inequalities of pressure the governing causes.†

There is another cause which *necessarily* operates in this case; a cause which long before I knew of the experiments of Dr. Magnus, I conceived to be the predominating one.

The gyroscope shows, to the apprehension of every one, that a rotating solid of revolution supported by a point in its axis, about which it is free to move, solicited by a force tending to turn it in any direction, turns, *not* in the direction of the force which solicits it, but, with uniform and slow motion, normally to it; that the axis of figure will describe a *cone* about a line passing through the point of support, parallel to the direction of the soliciting force.

The elongated projectile is discharged from the piece with its axis coinciding with its trajectory, but, through the action of gravity, the trajectory deflects from its original direction and from that of the axis.‡ In consequence of this, the resistance of the air acts obliquely to the axis, and, with the ordinary forms of elongated projectiles, without grooves, its resultant passes in front and *above* the centre of inertia, tending to *raise* the point,

* Thiroux's theory requires that in the descending branch of the trajectory, the point of the ball shall be depressed *below* the trajectory; Panot's, that it shall continue parallel to its original direction, and hence elevated *above* the trajectory. Dr. Magnus supposes the axis to keep pretty nearly coincident with the trajectory, and says that experiments made with balls fired at low velocities confirm this assumption.

† To illustrate my meaning, take the friction theory of Panôt which supposes the

5.



axis elevated above the trajectory and that the friction on the under side (where the air is *densest*) carriés it to the *right;* yet were this ball a hollow one (as nearly all are) the tail of the ball would be so much lighter than the point that the forces of friction (equal all along the cylindrical surface) would affect the tail more than the point and thus deflect the axis to the *left*, and produce a deviation in that direction.

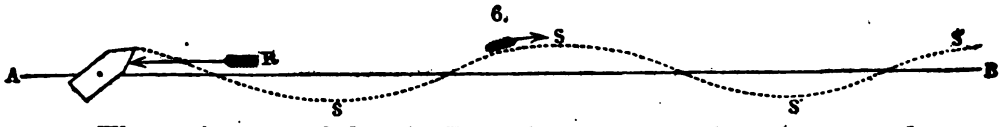
‡ *Which preserves its original direction through the stability it derives from its axial rotation*, is the unmeaning and false expression (or something equivalent) commonly used in this connection. Properly *there is no such thing* as stability of the axis. Take away the possibility of this *conical* motion (in precession) just described, *confine the axis to one plane*, and it becomes as movable as if no rotation existed. This is clearly established in the Analysis of the Gyroscope (this Journal, [2], xxiv, 49), where the following language is used, "the popular idea that a rotating body offers any direct resistance to a change of plane, is unfounded. It requires as little exertion of force (in the direction of motion) to move it from one plane to another, as if no rotation existed." The "surprising phenomena" of "complete mobility" when one of the rings of the gyroscope is so held that the axis is confined to one plane of motion, which Dr. Magnus arrives at *experimentally* (and all the modifications of his experiments) flow directly from my analysis. *If, then*, stability of axial direction is attributed to axial rotation in projectiles, that which is the *inseparable accompaniment* of this *apparent* stability (i. e. the conical motion described in the text), and without which there is nothing even *resembling* stability—must be accepted with it, and the *deviation* of the projectile, which will *certainly result therefrom*.

and from this results the conical motion of the axis to the *right*, if the rotation is to the right—to the *left* in the contrary case.*

Before attempting to apply the foregoing to the real case, it will help us to consider a more simple one.

Suppose the rotating projectile with its axis inclined to its trajectory to be propelled through the air, and that the centre of inertia is *confined* to a rectilinear path (as if it slid along an extended wire, A B, for example), and that the force of gravity does not act.

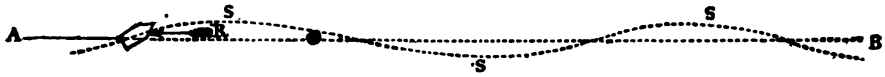
6.



The resistance of the air, R, acting *immediately* to increase the angle of inclination, produces *instead* the resulting conical motion to the right, which, combined with the motion of translation, would cause the *point* to describe a helix *s s s* about the rectilinear path of the centre of inertia.

Let now all the circumstances be as above *except* the confinement of the centre of inertia.

7.

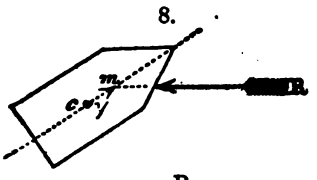


The resistance R, acting obliquely to the axis of figure, will (as in the familiar case of the sailing of a vessel *on* the wind) give the projectile a component of motion in the direction of its axis, and the centre of inertia cannot remain on its rectilineal trajectory A B. But, owing to the conical motion of that axis about a line parallel to the resistance R, (a direction, itself, always changing) the result will be that the projectile *itself* will describe a helix, *s s s*, about the line of original direction A B.†

* These results have been exhibited *experimentally* by Prof. Magnus; they flow directly from my analysis made *without* experiment of any kind.

† The *period* of these conical, or rather helical revolutions, could be computed if we knew the exact intensity of the force R; the distance C *m*, (or γ) from the centre of inertia, at which it cuts the axis of figure; the value *k*, of its radius of gyration about its axis of figure; and the velocity, *n*, of rotation.

8.



The period, T, (as deduced from my treatise on the Gyroscope) would be expressed in seconds thus—

$$T = 2\pi \frac{k^2 n}{\gamma r}$$

In which $r = \frac{R}{M}$ = *total* resistance of the air ÷ mass of the projectile. The value of *r* for the ordinary musket ball would be about 100. Assuming it at 30 for the elongated ball with more mass and *less* resistance—and assuming γ but $\frac{1}{10}$ inch, putting $n = 2\pi \times 150$, and $k^2 = .15$ inch, we should get $T = \frac{3}{4}''$ or three-fourths of a second.

If we suppose there to be an actual centre of *resistance*, *m*. (a supposition not true probably, but like that of the meta-centre, in hydraulics, admissible in narrow limits) this period would be *the same*, whatever be the inclination of the projectile. The period would increase as the resistance of the air diminished by loss of initial velocity.

In the foregoing case there is no *permanent* departure, or *deviation* from the original direction of projection; nor would there be much *apparent* departure in the range of the modern rifled arms since the periods of these helical revolutions would probably be but a fraction of a second, while the time of flight is from 6 to 8 seconds.

The matter is much more complicated when, instead of the imaginary case above presented, the force of gravity is introduced and the actual flight of a projectile is considered. The ball is projected, having its axis coincident with the trajectory, but from the very outset of the flight a severance commences in the directions of the axis and trajectory, owing to the action of gravity. The resistance of the air becomes gradually more and more oblique to the axis, pressing (as in Fig. 7) the point upwards, and producing a *precession* (or conical motion) to the right.

The entire character of the motion becomes altered by the constantly shifting direction in which this resistance is acting, owing to the constant *fall* produced by gravity.

It is very difficult, *a priori*, to describe the exact character of the motion. It depends upon unknown facts, and doubtless varies much with the form of the projectile—its velocity of translation and rotation.

It would be going into reasoning probably too difficult to express intelligibly, to attempt to explain why, in all cases, the period of these helical revolutions would be very greatly prolonged, and why I think that in *most* cases (if not all) *no complete* revolution takes place at all, but that the whole motion is confined to the first quadrant. In the latter case the flight of the projectile would exhibit one continuous and constantly increasing deviation to the right. In the case of a considerable number of helical revolutions actually performed, there would still be, with each revolution, an increased deviation to the right, owing to the descending branch of the vibration being so much larger than the ascending* one.

It would, perhaps, be unprofitable—in the absence of any sufficient data—in the absence even of experiments made with any knowledge of the *real* causes of deviation, and therefore nearly useless as reference for data, to pursue this subject any further.

That the *precessory* motion is an inseparable attendant to that (so called) *stability* of axis which is the *very object* of giving rotation, I have shown; that it is an *adequate* cause of deviation I think will be admitted. Hence, while *other* causes *may* contribute to the effect, or may oppose it (according to the shape of the ball), this peculiar effect must be looked upon as the controlling cause of deviation of elongated balls discharged from rifled arms.

* In this case the helical path would probably be so *prononcé* as to be easily detected by firing through screens.

I will but add a few very general conclusions I have arrived at from the foregoing.

First, the elongated ball possesses, from *its very shape*, a tendency to pursue the *direction* of *its axis*. This tendency should be made available, so far as practicable, to sustaining the flight, and flattening the trajectory.

Second. In the modern improvements in the art of throwing projectiles from rifled fire-arms, a decided step has been made *backwards*, in losing that most essential element to range and accuracy, *initial velocity*. It is desirable, and, I should think, not impracticable to restore it.

The initial velocity of the old rifle ball (weighing but about 180 grains) was 1750 feet per second. To the modern projectile, weighing about 500 grains, is given but from 900 to 1000 feet initial velocity.

It would doubtless be difficult to *give* the high velocity to so heavy a ball, and if given, the recoil would be inadmissible. But why throw so heavy a ball?—and why adhere to such calibres? The long range of such balls is not due to their weight, but to their *model* and to the *low ratio of their cross section to that weight*.

Both these advantages can be attained with a light ball as well as with a heavy one.

An increase of weight over that of the old musket ball is, *in itself*, objectionable, by increasing the soldier's load.

I can see no reason why, to a ball of the weight of the old musket ball (340 grains), may not be given all the properties of the heavier ones, with the additional great advantage of a *high* initial velocity, approximating to that of the old rifle balls.

To accomplish this, of course, the calibre must be greatly reduced—the ball elongated, and a *comparatively* large charge used (less, however, than that used in the old musket—viz. 110 grains).

This highly elongated ball, *balanced* (if I may use the term) by grooves around the after end, so arranged that the resultant of the air's resistance shall pass as nearly as practicable through the centre of inertia,* will fulfill likewise the *first* condition I

* According to my theory of the matter, these grooves, instead of acting like an arrow's feathers, to keep the axis coincident with the tangent to the trajectory, *have just the contrary object*, to keep it (if properly adjusted) parallel to that original direction, by so balancing the forces of resistance as to cause their resultant to pass through the centre of inertia. The theory of "arrows' feathers" supposes, on the contrary, that, by the grooves, this resultant is thrown *behind* that centre, and tends to *tilt* the axis; an operation which must inevitably, *likewise*, produce *deviation*. Prof. Magnus' observation on balls with low initial velocities (so as to be visible to the eye) confirm my position, though I attach little value to those observations.

Experience seems to show that this balancing of the forces of resistance may be produced *without* grooves (vide Wilcox, p. 160), for the Enfield ball and the Whitworth projectile seem to be without them, as likewise Lancaster's. I have seen no statements, however, as to the *deviation* of these projectiles.

have laid down. For the highly elongated ball possesses in a proportional degree the tendency to pursue the direction in which its axis points: and the causes of deflection of the axis' direction being eliminated by a proper balancing about the centre of inertia of the forces of resistance, the tendency is to pursue the *original* line of direction, in opposition to the downward curvature due to gravity, and thus to *flatten* the trajectory and increase the range.

These conclusions are those of theory alone, but, if I mistake not, all the most recent advances in rifled arms have been in the direction which they indicate. The Swiss (Federal) rifle, one of the most perfect in Europe (see Lieut. Wilcox's "Rifles and Rifle Practice," p. 187) has a calibre of but 0·41 (inch), and its ball, 2½ calibres in length, weighs but 257 grains, thus combining with the small calibre, the highly elongated form, and even then weighing *less* than our old spherical musket ball. Though its initial velocity is not given, yet as the greatest *proportional* charge of powder is used with it, doubtless it also receives the *highest initial velocity*,* of any of its class of projectiles now known in any service.

The *hexagonal projectile* of Whitworth is another instance in confirmation of the principles I advance.

Of small calibre and highly elongated,† he throws this projectile with such accuracy as to hit with certainty, at 500 yards, a disk *not more than two inches in diameter;* and "asserts that he will not rest satisfied till he has fired a ball from one of his guns into the barrel of another, at a distance of 500 yards." (Edinburgh Review, Ap. 1859.)

The "Armstrong" projectile is another characteristic instance. So decidedly is the *elongation of the ball* characteristic of the most recent and successful efforts in obtaining range and accuracy, that the English writer just quoted applies the term "bolts" to the Whitworth and Armstrong projectiles.

In fact, the *two* springs from which have risen the modern improvements in projectile weapons are 1st, the application of the *rifled principle* to *all arms;* 2d, the *elongation* of the projectile. *Either one alone* may produce, *to a certain extent*, the results desired; it is only by the best possible combination of the two that the best results can be educed. The *increase of range* is due

* Initial velocity is the *very first* element in procuring range and accuracy. The greater it is, other things being equal, the *flatter* will be the trajectory (one of the principal elements of accuracy), and the flatter the trajectory the less the lateral deviation laterally. The extreme curvature of trajectory (though it may have its advantages in some peculiar circumstances), is the great difficulty as to accuracy *of fire*, in modern rifled weapons.

A *necessary evil* with the heavy balls used; the pretence that it is, *in itself*, an advantage, is too absurd for controversy.

† According to Wilcox the mean calibre is about 0·46, and its length 2½ calibres.

almost entirely to the latter principle, and it is only by applying it to the utmost *practicable* extent (as in the case of the Whitworth and Armstrong projectiles), that the greatest range and most perfect accuracy can be obtained.

The *calibres* in use have been a positive *bar* to the successful use of this principle in small arms. Borrowed from the old smooth-bored weapons, the adherence to them has caused an unnecessary *increase* of weight,* and made a loss of *initial velocity* inevitable, with all the attendant evils of a highly curved trajectory, and large deviation.

"Notwithstanding" (says Wilcox) "the long time that has elapsed since the discovery of the rifle, its principle is not yet so well understood as to have led to the general adoption of any particular form of this arm as the best."

The above conclusion of the author of "Rifles and Rifle Practice," will justify me, I hope, in venturing to make the foregoing suggestions on the subject.

New York, Dec. 15, 1859.

ART. XVIII.—*Gulf Stream Explorations—Third Memoir. Distribution of Temperature in the Water of the Florida Channel and Straits;* by A. D. BACHE, Sup't. U. S. Coast Survey.—With Diagrams.

(Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.)

The results of the explorations of the Gulf Stream in the survey of the Coast, have been communicated to the Association from time to time, as phenomena of peculiar interest have been developed.

The original plan of these explorations having been carefully studied, and having proved successful, has steadily been adhered to. The more recent observations have been directed to that part of the stream between Havana and Cape Florida, known as the channel and strait of Florida.

I have now to present four sections showing the depth and temperature in this most important region of the Gulf Stream. These results are from the observations of Commander B. F. Sands and Lieut. Commanding T. A. Craven, U. S. Navy, Assistants in the Coast Survey, whose names have already been mentioned before the Association in connection with explora-

* The old spherical musket ball of ·69 calibre weighed 340 grains. The new "rifle musket" ball of ·58 calibre weighs 500 grains, while still heavier balls are in use in Europe. Thus we see in use balls surpassing in weight the musket ball, and having *twice*, and even *three times* the weight of the Swiss ball described in the text, admitted to be, practically, the best in Europe.

tions of the Gulf Stream, and furnish a sufficient guaranty that the results have all the reliability which care, experience and zealous labor could give them.

Section No. 1, from Cape Florida to Bemini was run by Lieut. Commanding Craven, in May, 1855; Section No. 4, by Commander Sands, in May, 1858; and Sections 2 and 3 by Lieut. Commanding Craven, in April and May of the present year, (1859). Sections 2, 3 and 4 are perpendicular to the direction of the Stream at distances of about fifty, one hundred, and two hundred miles from Cape Florida. The Florida strait is funnel-shaped, being about ninety miles wide at Havana and about forty-five miles wide at Cape Florida, the narrowest part.

Form of bottom.—The area of the water way and the form of the bottom are represented on diagrams 7, 8, 9 and 10. The Arabic numerals at the top represent distances from the Florida coast (the Keys) in miles, and the Roman numerals, the positions at which observations are made. The numbers at the left hand represent the depth in fathoms.

Commencing at the Cape Florida section, it will be seen that there is a rapid descent of the bottom to the Havana section, from three hundred and fifty fathoms to eight hundred fathoms, or twenty-seven hundred feet in a distance of two hundred miles. The most shallow as well as the narrowest part of the Stream is therefore at Cape Florida. The deepest water follows the coast of Cuba and the Grand Banks, the depth being eight hundred fathoms at a distance of only five miles from Havana, nearly four hundred fathoms within five miles of Salt Key Bank, and three hundred fathoms close to the island of Bemini. The descent from the Florida side is for the most part gradual, but from the opposite side abrupt. This effect seems to have been produced by the action of the sub-current in wearing a deeper channel upon the concave side of the Stream. At Havana there is an abrupt descent of nearly a mile within five miles of the shore, while on the side of the Tortugas and Key West the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf Stream makes the circuit of the Gulf of Mexico, since, if it impinged directly upon the island of Key west and the Tortugas we should find its effects in the wearing of a deeper channel on that side.

TEMPERATURES.

Change of temperature with depth.—In a former communication the law of change of temperature with depth was discussed, and types of the curves representing the law were given for different parts of the Stream. The curves were all merely modifications of a more general form. Thus, the cold water between the Gulf Stream and the Coast gave one form; the axis of the Stream

another; and the water beyond the axis a third form, while in the strait of Florida a fourth was developed. It would be natural to expect in the course of many years' explorations by different individuals with different instruments not even of the same class, that general phenomena of this character should present some contradictions and some inexplicable results. Experience however has confirmed the first conclusions and the constancy of the phenomena. It is not difficult, having the curve representing the temperatures at any position from the surface to the depth of several hundred fathoms to determine from the temperatures alone, in what part of the Stream they were taken.

Temperature in a direction perpendicular to the Stream.—Diagrams 2, 3, 4 and 5, show the changes of temperature for the same depth in each of the sections, and diagrams 7, 8, 9, and 10, the depth for the same temperature.

Bands of warm and cool water.—In the section from Cape Florida to Bemini, the division of the Stream into bands is plainly exhibited, though more faintly than in the northern sections, and the form of the bottom in this section shows the elevations and depressions corresponding to the divisions. In the sections south of Cape Florida, all traces of the bands seem to disappear as well as the ridges of the bottom. The bands therefore seem to have their origin near Cape Florida, and the conclusion stated some years ago, as the probable one, is strengthened, that they are caused by the ridges and valleys of the bottom parallel to the general course of the Stream, and along which the Stream and counter stream have their course.

The Cold Wall.—The cold wall, as an exception to the remark made above in reference to the bands, is traced as far as the Tortugas, and is plainly shown in all the sections with more or less distinctness. In the Sombrero Key section (No. 3) it is strongly marked at depths ranging from seventy to a hundred fathoms, while in all the sections the warm water at the surface overflows the cold wall and reaches quite to the shore.

Diagram No. 6 represents the comparative curves of the cold wall in different sections of the Gulf Stream, including those in the Straits of Florida. The figures at the top show the distances of the cold wall from the shore in the different sections, and the numbers on the left the degrees of temperature. The curves are drawn for different depths in the several sections, as shown in the notes at the bottom of the diagram. The curves *g*, *h*, *i*, *k*, represent the cold wall in the four sections under consideration.

Longitudinal Sections.—It has been found very difficult to deduce any satisfactory law for the decrease of surface temperature along the axis of the Stream owing to the variability of the temperature of the water of the regions from whence the Gulf Stream is supplied. Two modes of investigating the subject

have been pursued, one by following the stream from the Gulf of Mexico, and making hourly observations of the temperature of the water, and the other by comparing the mean temperatures of the various sections with each other, and with the temperature of the Gulf of Mexico. In the first method, the vessel must be allowed to drift with the current of the stream, a difficult condition except in the best weather, even for a day, and to float along thus, for hundreds of miles would rarely be practicable. Any motion communicated by sails or by steam must carry the vessel beyond the water in which she commenced her voyage, and the lateral overflow carries the water constantly from the axis toward the edges of the Stream.

In the comparison of mean temperatures of the different sections, the fact has been established, that the temperature of the water of the Stream at any point may be higher than at a point nearer the source, and hence vessels in running along the Stream may, and generally do, pass through water not of a constantly diminishing temperature, but from cool to warm, and the reverse. This is to be explained mainly though not entirely, by the variability of temperature at the source.

By taking the mean temperature of any one section, and going back to the date of the departure of the waters from the Gulf of Mexico, as determined by the velocity of the stream, and comparing the temperatures observed with the temperature of the Gulf waters, it was supposed that a solution of the question might be obtained. The temperatures were taken from the most authentic meteorological records of the Gulf for a series of years, and those periods sought which corresponded to the dates desired. The uncertainty of the temperatures of the waters of the Gulf of Mexico, as obtained from air temperatures taken here and there along its shores rendered the results unsatisfactory. Enough seems to have been determined, however, to show that the surface temperature of the Gulf Stream along its course is variable; that a vessel sailing along the axis at a more rapid rate than the motion of the stream, will pass through water of higher and lower temperature, depending generally upon two conditions, viz: the distance from the Gulf of Mexico, and the temperature of the Gulf at the time the water entered the straits of Florida; and further, that the latter cause is the predominating one in the parts of the Gulf Stream adjacent to the Atlantic coast where the current is rapid.

The influence of the form of the bottom in forcing the cold counter current of the bottom upward, has been adverted to, and the fact appears to be well established in the cross sections where the ridges and valleys parallel to the direction of the stream separate it into bands of warmer and cooler water, and this conclusion, as has just been stated, is strengthened by the fact that the

bands and ridges simultaneously disappear south of Cape Florida. The phenomenon is moreover strikingly exhibited in the longitudinal section of the bottom, in connection with the lower temperatures.

The shallowness of the Stream in the strait of Florida, connected with the fact that the bottom falls off rapidly to the north and south afforded an excellent opportunity for testing the question. If the cold water of the under polar current follows the bottom, it should appear in the shallow part of the strait, and here the warm water of the surface, and the cold water of the bottom, would approach each other. Diagram No. 1 shows the curves of 40°, 45°, and 50° (bottom temperatures) along the deepest part of the stream, commencing at Sandy Hook, and running as far as the Tortugas. All these curves rise with the bottom and pass over the ridge which divides the bed of the Atlantic from that of the Gulf of Mexico, and again fall with the slope of the bottom towards the Gulf. In the narrowest part of the strait where the depth is three hundred and fifty fathoms, the temperature from the surface to the bottom, ranges between 80° and 40°.

On the effects of pressure on Saxton's deep sea thermometer.—In the exploration of the Gulf Stream, the temperatures below one hundred fathoms have mostly been determined by Saxton's metallic thermometer, and although the results have been consistent amongst themselves, and have agreed well with the indications of other thermometers, yet it was thought advisable to determine the effect of pressure by direct experiment.

Saxton's thermometer consists essentially of a compound ribbon of silver and platinum fused and pressed together by rollers. This ribbon is wound in a spiral form, one end of the spiral being firmly fastened to an interior solid axis and the other left free. Upon the free end is placed an index arm which moves over a circular graduated scale carrying with it a friction hand or indicator which is left at the extreme point of the arc reached by the true index. The instrument is enclosed in a case to which the water is freely admitted. A variation of temperature is immediately noticed, as the effect is to give a rotary motion to the index.

The experiments to determine the effect of pressure were made at my request by Mr. J. M. Batchelder with means devised by Mr. Thomas Davison at the Novelty Iron Works. The following description of the apparatus employed, is given by the last named gentleman.

"The gauge consists of a brass cylinder H, about eight inches long into which a steel plunger is fitted, the upper part of the plunger at A being ·70 of an inch in diameter, and the lower at B about ·786, so that the difference in area of the ends is equal

to one tenth of a square inch. The cylinder is bored out a little larger than the plunger except for about a fourth of an inch near each end at C and D where both are accurately fitted. To the branch E a pipe connects, communicating with the hydraulic cylinder and leading the water into the centre of the gauge which it reaches after passing through the chamber F filled with sponge to prevent any impurities in the water from reaching the plunger. The upper end of the plunger connects by a wire W, to a spring as shown in the sketch at G, so constructed as to indicate pressure from 0 to 450 lbs., the spring being so strong that 450 lbs. produce a movement of the plunger equal to three-eighths of an inch. It is evident that as the difference in area of the ends of the plunger is one-tenth of an inch, one hundred pounds pressure from the water on this surface, as indicated by the balance, would equal a pressure of water of 1000 lbs. per inch, or a pressure ten times as great as that indicated by the balance throughout its scale. The only difficulty in the use of the gauge is that of fitting the plunger to the cylinder so that while it is perfectly free to move it is also perfectly water tight. This difficulty however has been overcome, and much advantage was also derived from Mr. Batchelder's suggestion for supplying the wear of the plunger and cylinder by depositing brass on the plunger through the galvanic process."

Connected with this gauge by a pipe is a strong wrought iron cylinder, sixteen inches long by four inches in diameter, in which the thermometer was placed, the opening being firmly closed by a screw plug. This second cylinder was immersed in a tub of water for the purpose of regulating the temperature. The thermometer once placed in the cylinder, is not again removed, the index being read by means of a mirror until the observations are completed. By the use of this apparatus, the effect of pressure up to 4000 lbs. per square inch was observed upon two thermometers, and the results are given below. The observations were made to indicate the effects of 500, 1000, 1500, 2000, 2500 lbs. pressure, etc. Seven series of experiments were made with thermometer No. 5, and five series with No. 10. The mean results show that a pressure of 1000 lbs. per square inch has no effect upon the thermometer; at 1500 lbs. the effect is less than one degree; and from 1500 to 4000 lbs. per square inch, the effect is to diminish the readings, the maximum effect being seven degrees.

The diagram exhibits the law of diminution by increase of pressure, and the depth corresponding to different pressures. The correction to be applied varies with the depth. For thermometer No. 5 it is only four tenths of a degree Fahrenheit at the depth of 600 fathoms. For thermometer No. 10, it is one degree at the same depth.

At 1500 fathoms the corrections are respectively five and a half and seven degrees.

Nearly all the temperatures observed in the Gulf Stream have been taken at depths less than six hundred fathoms.

Table showing differences of readings of Saxton's Thermometer under pressure and free from pressure.

Thermometer No. 5.							Thermometer No. 10.						
No. of Series.	Pressure in Pounds.						No. of Series.	Pressure in Pounds.					
	1500.	2000.	2500.	3000.	3500.	4000.		1500.	2000.	2500.	3000.	3500.	4000.
	°	°	°	°	°	°		°	°	°	°	°	°
1	1·6	0·	3·75	0·	0·	0·	1	0·	2·0	3·25	4·5	6 25	8·25
2	0·	1·00	2·	2·8	4·5	5·5	2	2·0	1·0	3·5	4·5	6·0	7·25
3	0·	1·0	2·25	3·75	4·75	5·7	3	0·75	2·0	3·0	3·25	5·5	6·5
4	0·	0 5	0 50	2 0	3·6	5·5	4	1·75	2·0	3·5	4·75	5·5	7·25
5	0·	1·75	2 25	3·5	5·0	6·5	5	·75	1·75	1·75	3·75	5·0	6·75
6	0·	1·25	2 25	3·75	6 0	6 5	Means	1·00	1·75	3·0	4·25	5·6	7·25
7	0·5				3·5	4·25							
Means	0·3	1·1	2·1	3·2	4 5	5·6							

Art. XIX.—*On the Chemical Composition of Pectolite;* by J. D. Whitney.

A few years since I made some examination of specimens of a radiated fibrous mineral from Isle Royale, Lake Superior, which proved on analysis to be pectolite. A mineral, closely resembling pectolite, from Bergen Hill, New Jersey, which had been analyzed by L. C. Beck, and considered by him as identical with the stellite of Thomson, was examined at the same time and found to agree in composition with pectolite, as had been previously suggested by J. D. Dana. Both the stellite and Wollastonite of Thomson were referred by me, at that time, to pectolite,* a reference the correctness of which has since been shown by Messrs. Heddle and Greg, in a paper on the composition of the English varieties of this mineral.†

Notwithstanding so many analyses of pectolite have been made by different chemists, there has not been a sufficient accordance in the results obtained to justify a positive decision as to the real formula of the mineral, although that of Von Kobell has been generally adopted. It will be sufficient to refer to the various published analyses, to see that there is but an unsatisfactory degree of uniformity in their results, whether of specimens from American or European localities. Thus, for instance, in

* Journal of Boston Nat. History Soc., vi, 40.

† Philos. Mag., [4], ix, 238; also in Erdmann and Marchand's Journal, lxvi, 144.

Von Kobell's analysis of the Monte Baldo pectolite, the silica is given at 51·3 per cent, while other analyses of Scotch and American varieties give as much, in some instances as 54 and 55 per cent of that substance. In the like manner, the amount of lime, as stated by different analysts, varies from 29·8 to 35·2 per cent, while there is even less agreement in the water, which is given at from 0·41 to 3·39 per cent.

The difficulty of procuring, in a perfectly pure state, a mineral which only occurs in a finely-fibrous condition is undoubtedly one of the principal causes of these discrepancies in the analyses; but it is also possible that the unusual care required for the correct determination of the silica in the very soluble class of minerals to which pectolite belongs may not, in all cases, have been appreciated. The great abundance and purity of the specimens of this mineral which have been obtained from the tunnel of the Erie railroad, recently excavated through Bergen Hill, seemed likely to obviate the first difficulty mentioned above. The results of three analyses indicated that this material was really of almost absolute purity, while no pains were spared to effect a complete and accurate separation of the various ingredients, and especially of the silica.

The pectolite dissolves more or less completely in chlorohydric acid, according to the strength and quantity of the latter. By using a considerable excess of rather dilute acid, all, or nearly all, the pulverized material may be dissolved into a clear liquid. As the attack is usually performed, a portion of the silica remains in solution and the remainder separates as a flocky precipitate.

The following experiments show the difficulty of estimating the silica correctly in this class of highly soluble silicates, and the necessity of unusual precautions in its determination.

On digesting the *ignited* mineral with acid until a perfect attack seemed to have taken place, the solution gelatinized on evaporation, and there was no perceptible gritty feeling when it was stirred with a glass rod or the spatula; on separating the silica, however, after evaporating to dryness, moistening with acid, and adding water, in the usual way, its amount was found to be equal to 62·10 per cent of the substance taken.

Another portion of the *unignited* mineral was attacked by acid, and the silica separated without evaporating to entire dryness; its amount equalled only 35·6 per cent. To procure the whole amount of this substance present in the mineral, and uncontaminated by any traces of the bases, it was found necessary to use the unignited substance for the attack with acid, carefully to evaporate to entire dryness over the water-bath, then to moisten the dried mass with strong acid and allow it to stand for some time before adding water and filtering. These precau-

tions will give a perfectly pure silica, but not all of it, as two or three per cent will still remain in the solution, a part of which will go down with the ammonia precipitate, and the remainder be found after driving off the ammoniacal salts, (the lime having been previously separated,) and igniting the residuum.

The following are the results of three analyses of as many different specimens of the pectolite from the Bergen Hill tunnel.

	I.	II.	III.
Silica,	54·32	54·76	54·27
Lime,	33·12	32·88	32·83
Protoxyd of manganese,	·66	1·16	1·24
Protoxyd of iron,	·26		
Soda,	8·78	9·17	8·94
Water, by loss,	2·36	2·03	2·72
	100·00	100·00	100·00

The direct determination of the water on the substance dried at 80° C. gave, for II, 3·03, and for III, 2·75 per cent. Specimen III, from the Wheatley Collection, Union College, was apparently the purest; it was a fragment of a mass, the fibres of which were several inches in length, slightly divergent from a common centre, and being nearly transparent and evidently quite free from any admixture with quartz or any other foreign substance.

In analysis III the oxygen is as follows:

Silica,	28·942
Lime,	9·379
Soda,	2·306
Protoxyd of manganese,	·290
Water,	2·44

This gives as the ratio:*

	Ḣ	:	Ṅa	:	Ċa	:	S̈i
	1	:	1·05	:	3·83	:	11·84
or nearly	1	:	1	:	4	:	12

If we attempt to express this ratio by a formula, we have

$$\dot{N}a^2\,\ddot{S}i^4 + 4\dot{C}a^3\,\ddot{S}i^3 + 3\dot{H}.$$

The percentage demanded by this formula is given below, by the side of that required by Von Kobell's.

	Whitney.	Von Kobell.
Silica,	54·22	52·05
Lime,	33·73	35·33
Soda,	9·33	9·77
Water,	2·74	2·83

It is evident, from a comparison of the figures given above, that the formula now suggested agrees more nearly with the re-

* The atomic weights used are as follows:

Na=23 : Ca=20 : Si=21, H being =1.

sults of the analyses of the Bergen Hill pectolite than any yet proposed. As written here, its relations to that of spodumene are to be noticed, as also to those of Wollastonite and pyroxene. The latter connection will be made plainer by writing the formula of pectolite thus:

$$\dot{R}^3 \ddot{S}i^2,$$

$\dot{R}$ being $(\dot{C}a\frac{4}{6} + \dot{N}a\frac{1}{6} + \dot{H}\frac{1}{6})$.

Northampton, Mass., Feb. 1859.

ART. XX.—*Notes on the Ancient Vegetation of North America;* by Dr. J. S. NEWBERRY. In a letter to Prof. DANA, dated Santa Fe, New Mexico, Oct. 15th, 1859.

Dear Sir:—I have just returned to Santa Fe after an absence of three months, spent in an examination of the geological structure of the country bordering the San Juan and Upper Colorado rivers, in Utah and New Mexico, connected with the War Department topographical survey under Capt. Macomb, Topog. Engrs.

The region visited proved interesting in many respects—beautifully picturesque and unexpectedly productive—covered with ruins, and once densely populated by a race that has now entirely abandoned it.

I would cheerfully give you a sketch of its remarkable physical and geological structure, but the results of the expedition will doubtless be published in detail by the War Department, and it is not proper that any part of them should now be given to the public. I will say however, in general, that our field of exploration includes an immense labyrinth of great cañons, scarcely less abysmal than those of the Lower Colorado in which we were last year involved—some of which are over a mile in depth—and even more varied and wonderful in character. The sections exposed in their walls permitted me to measure and examine all the strata between the base of the Carboniferous and the summit of the Cretaceous series; the latter formation attaining a thickness of 4000 feet, and occupying an immense area west of the main divide of the Rocky Mountains.

Our work this season connected on the south with that of the party with which I was associated last year under Lieut. Ives, T. E.—and combining the results of both expeditions, I have now a complete and detailed section of all the rocks composing the great central plateau of the continent, from the base of the palæozoic series to the summit of the Cretaceous. These strata are conformable throughout, and over 10,000 feet in thickness.

The collection of fossils made, both animal and vegetable, is quite large, and, with considerable new matter, includes what is much better, many well known species of which the geographical range will be seen to be much greater than has been heretofore supposed.

I was agreeably surprised to find here on my arrival, the May, July and Sept. numbers of the Journal, published during my absence, by which I have been in a degree placed *au courant des affaires scientifiques.* In the articles in the July number from the pen of my friend Mr. Lesquereux, I have been much interested, as they refer to matters which have engaged much of my attention for some years. In the letter of Prof. Heer there is however a passage which seems to me to require notice; although, situated as I am, without specimens or books for reference, I am scarcely prepared to take up the discussion of the questions involved in it. This is the less necessary now, as what I have to say in reference to them will be found *in extenso* in the reports on the geology of the country west of the Mississippi which I have made or have in preparation for the general government.

The paragraph to which I allude is as follows:—

"Your views of the gradation of the flora of North America agree perfectly with what we find in Europe. This led me to believe that the plants of Nebraska belong to the tertiary and not to the cretaceous formation. It is true that I have seen only some drawings which were sent to me by Messrs. Hayden and Meek; but they are all tertiary types. The supposed *Credneria* is very like *Populus Leuce*, Ung., of the lower Miocene, and the *Ettinghausiana* seems hardly rightly determined. Besides it is a genus badly founded, and which has as yet no value. All the other plants mentioned by Dr. Newberry belong to genera that are represented in the Tertiary and not in the Cretaceous. And it is very improbable that in America the cretaceous flora has had the characteristic plants of the tertiary; and this would be the case if these plants did belong to the Cretaceous."

It will be seen that Prof. Heer in this paragraph makes several distinct statements, which for the sake of brevity, I will notice in the order in which they occur. They are,

1st. That the fossil plant I supposed to be a *Credneria* is very like *Populus leuce*, Ung.

2d. The *Ettingshausenia* (called erroneously *Ettinghausiana*) is wrongly determined.

3d. That the genus *Ettingshausenia* is badly founded and has no value.

4th. That all the other plants enumerated by me are represented in the Tertiary and not in the Cretaceous.

5th. That it is improbable that in America the Cretaceous flora had the characteristic plants of the Tertiary, as would be

the case if the plants of which outline sketches were sent to Prof. Heer by Mr. Meek, were Cretaceous.

To which I reply,

1st. The plant I considered a *Credneria* is not *Populus luce* Unger, which, according to his descriptions in the *Foss. Flor. v. Sotzka* and *Genera et Species Plant. Foss.* has a toothed margin, while the leaf in question is entire.

I have recently obtained more and better specimens of this fossil, of which I have apparently three species, but as yet have not had opportunity to study them carefully.

They strongly resemble some of the species of that portion of the old genus *Credneria* to which Stiehler left the name (*C. integerrima*, &c.) when he established his genus *Ettingshausenia*. It may prove a new genus. Further study will alone determine this.

2d. That I was wrong in considering some of Dr. Hayden's fossils generically identical with those Stiehler designated by the name *Ettingshausenia*, I am by no means prepared to admit.

Prof. Heer has had but a single outline sketch of the plant, and can hardly speak decisively on the subject. When at Washington I had before me all the figures and descriptions of Stiehler, Zenker, Dunker, Bronn, and Unger, of the genus *Credneria*, and a large number of specimens in good preservation, for comparison. To me, and to Mr. Meek, who examined the subject with me, there seemed to be a marked correspondence in general form, texture and nervation, between our specimens and some lobate *Crednerias* (*Ettingshausenias* Stieh.). With these I regarded our fossils as generically identical, and shall continue so to regard them until the question—if question there be—can be definitely settled by a comparison of specimen with specimen—old world and new.

3d. Whether Stiehler was in error in establishing the genus *Ettingshausenia* upon a group of species of *Credneria*, I will not pretend to say, for I have nothing like the ample material possessed by him when he made the division; indeed this question has nothing to do with that now before us.

It is true that at the Smithsonian Institution I had access to nearly everything that has been published upon the genus *Credneria*, and it seemed to me the most natural thing in the world that Stiehler should give generic value to the cuneate or lobate form, and strongly marked yet finely reticulated nervation, which characterize the group of species of *Credneria* of which *C. cuneifolia* may be taken as a type, while he left the broadly rounded, entire, or merely toothed leaves, with a sparse and rectangular nervation, such as *C. integerrima*—leaves not very unlike those of *Coccoloba*—to which they have been compared—to retain the name of *Credneria*.

The vindication of his accuracy may, doubtless, be safely left to Stiehler. At least it would be nothing short of arrogance for any one who had not before him a suite of the specimens compared by Stiehler, to review his work and pronounce it either erroneous or correct.

4th. The statement that aside from the so-called *Credneria* and *Ettingshausenia*, *all* the genera enumerated in my letter to Messrs. Meek and Hayden, are represented in the Tertiary and not in the Cretaceous, is at least surprising. I am almost inclined to infer from it that Prof. Heer, though confessedly the highest authority in reference to the Tertiary flora of Europe, has neglected to acquaint himself fully with that of the Cretaceous formation. He makes the statement doubtless in good faith, but he can hardly have seen Stiehler's paper on the Cretaceous plants of Blankenburg, and if he has not seen that he is certainly not yet prepared to discuss intelligently the claims of *Ettingshausenia* to be recognized as a good genus; nor indeed the Cretaceous flora in any of its aspects.

Whoever will take the trouble to examine Stiehler's paper (*Palæontographica*, 1857) will see in the enumeration of plants found in the Lower Cretaceous strata (Quader sandstein) *Populus*, *Salix*, *Acer*, and several other genera which Prof. Heer says are represented in the Tertiary but not in the Cretaceous.

The fossil flora of Blankenburg is indeed strikingly like that of our Lower Cretaceous formation, from which the plants that have given rise to this discussion have been derived, except that ours is more varied, and we have as yet found no palms or *Cycadaceæ*.

5th. In regard to the probability or otherwise that the Cretaceous rocks of America should contain a flora similar to that of the Tertiary, it may be said, that it is not now a question of probabilities but of fact, the evidence of the case being now before us, and in abundance.

In what has heretofore been written in reference to these fossil plants two great questions have been raised, 1st, as to their botanical affinities, 2d, as to their geological position.

As to their botanical relations—outline sketches of a few of the plants have been examined by Prof. Heer. By him they were decided to contain representatives of the genera *Liriodendron*, *Populus*, *Laurus*, *Sapotacites*, *Phyllites*, *Leguminosites*, &c., and were pronounced Lower Miocene.

The entire collection was placed in my hands for examination and description before I knew that Prof. Heer had been written to on the subject. I supposed I found among them *Liriodendron*, *Salix*, *Alnus*, *Populus*, *Platanus*, *Pyrus*, &c., with the Cretaceous genera *Credneria* and *Ettingshausenia* and considered them Cretaceous. That you may see on what evidence that opinion was

based, I enclose a copy of my letter to Messrs. Meek and Hayden, which I chance to have with me.

Washington, D. C., Nov, 12th, 1858.

Messrs. MEEK and HAYDEN,

Gents: The fossil plants which you requested me to examine, I have looked over with great pleasure, and, in answer to your question as to the age of the strata from which they were derived, concur with you in the opinion that they belong to the Cretaceous epoch. They include, however, so many highly organized plants, that were there not among them genera exclusively Cretaceous, I should be disposed to refer them to a more recent era.

A single glance is sufficient to satisfy any one that they are not Triassic. Up to the present time no angiosperm dicotyledonous plants have been found in rocks older than the chalk, while of the eighteen species which compose your collection sixteen are of this character.

What was the general aspect of the flora of our Cretaceous continent we can only conjecture, as the specimens of it which we have, represent only its ruder and coarser elements,—the leaves of some of its deciduous trees, which, perhaps by an annual frost, were, as now in autumn, scattered on the surface of stream, lake, or sea, and, sinking, mingled with the sediment accumulating at the bottom.

In such an herbarium we could expect to find little else than the relics of some of the ligneous plants, and a very imperfect picture of the flora of the period.

The evidence furnished by your specimens is, however, good as far as it goes, and we are warranted in inferring from them the existence of a more highly organized flora during the Cretaceous period than has usually been attributed to it.

A flora so highly organized, embracing so many angiosperm dicotyledonous plants, should lead us to expect the discovery of what have not yet been found, plants of this rank in the Jurassic and Triassic rocks. Such a flora as is indicated by your specimens, could hardly have at once burst into being, but was doubtless preceded in the older formations by more or less highly organized plants, the prophetic types of those which followed them.

From the enumeration of the genera represented in your collections it will be seen that the flora of the Cretaceous epoch was not very unlike that of the temperate portions of our continent at the present time. The same thing may be said of the Miocene Tertiary flora of the Upper Missouri so fully illustrated in the collections of Dr. Hayden. In both the tropical and sub-tropical forms so common in the floras of the same period in Europe, are apparently wanting; indicating a greater relative uniformity of climate during the later geological epochs, and carrying the aspects of nature of the present, far back into the past. Thus it may be said of our plants as of our fishes, that many of them are "old-fashioned" types.

An interesting fact in this connection, to which I can only allude, is that the later extinct floras of Europe are more like the existing flora of North America, than is that now growing over the rocks which contain them.*

* Including as they do *Liquidambar*, *Liriodendron*, &c., now exclusively American.

The species of your fossil plants are probably all new, though generally closely allied to the Cretaceous species of the Old World. From the limited study I have given them, I have referred them to the following genera:

Sphenopteris,	Cornus,	Salix,
Abietites,	Liriodendron,	Magnolia?
Acer,	Pyrus?	Credneria,
Fagus,	Alnus,	Ettingshausenia.
Populus,		

Of these the last two are exclusively Cretaceous and highly characteristic of that formation in Europe.

For comparison with the preceding list of genera, I subjoin a catalogue of the Cretaceous genera found at Blankenburg in the duchy of Brunswick, given by Stiehler in the *Palæontographica*, Sept. 1857.

	Credneria,	Pterophyllum,	Comptonites,
Algæ	Chondrites,	Flabellaria,	Populus,
	Halymenites,	Pinites,	Alnites,
	Delessertites,	Geinitzia,	Acer,
	Equisetum,	Araucarites,	Quercites,
	Pecopteris,	Salicites,	Juglandites.

I may say, in confirmation of the assertion that your fossil plants are Cretaceous, that I found near the base of the Yellow Sandstone series in New Mexico—called Jurassic by Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours—associated with *Inoceramus*, *Gryphæa*, and *Ammonites*, of Lower Cretaceous species.

Yours, &c.,

J. S. Newberry.

Since that letter was written, I have added largely to my material illustrative of the American Cretaceous fauna and flora, having been for some months engaged in studying that formation over a large area, and where it exhibits an unequalled development.

Of the geological age of the deposits which contain the fossil leaves of which sketches were sent Prof. Heer, there cannot now be the slightest doubt. I have in my hands over sixty species of dicotyledonous plants obtained from the Cretaceous formation. At least half of these are derived from near the base of that system in New Jersey, Nebraska, Eastern, Middle and Western Kansas, New Mexico and Utah, collected by Prof. Cook, Mr. Meek, Dr. Hayden and myself. Some of the species are common to nearly all the exposures of the Lower Cretaceous sandstones, which I have examined, and everywhere serve for the accurate identification of these strata. Overlying the rocks containing all this flora, in the same continuous section, where the strata are conformable and undisturbed, both Dr. Hayden and myself have, in repeated instances, found many of the most characteristic fossils of the chalk, such as *Gryphæa Pitcheri*, *Inoceramus problem-*

aticus, Ostrea congesta, Baculites ovatus, Ammonites placenta, Scaphites Conradi, Ptychodus Whipplei, &c.

The botanical character of this group of plants is, in all essential respects, just what I represented it to be in my letter to Meek and Hayden. Among them are certainly *Populus, Salix, Alnus, Platanus, Liriodendron, Fagus, Quercus*, &c., the most common genera in our present forests.

The plant regarded by Prof. Heer as identical with Unger's *Laurus primigenia* is not a *Laurus*, but a *Salix*, as Prof. Heer would have seen if the specimen had been sent him, instead of an outline sketch. As I have before said, his *Populus leuce?* is not that species. The plants which he calls *Sapotacites* and *Leguminosites* are of doubtful affinity, but certainly not referable to these genera. The latter has a nervation closely allied to that of some of the *Rhamnaceæ*. *Phyllites* is not, as Prof. Herr is made to say in Marcou's pamphlet on "American Geology," "peculiar to the Lower Miocene," but is a general receptacle for fossil leaves of all ages of which the botanical affinities are doubtful, just as *Carpolithes* is a general name for fossil fruits.

It is greatly to be regretted that Prof. Heer could not have applied his great knowledge to the specimens themselves rather than to outline sketches; or, at least, that he should not have been permitted to exercise his excellent judgment unbiased by erroneous oral testimony.

The remarks of Prof. Heer on the fossil plants from the Pacific coast described by Mr. Lesquereux, are exceedingly interesting as forming a new page in the botanical history of American geology, and yet the quite different flora which has come under my observation from the Miocene strata of another part of the continent proves that what he has predicated of the flora, and hence the climate of the continent, though doubtless true of the region where Dr. Evans' fossils were found, is not of universal application.

The study of the floras of the different geological formations has always seemed to me to promise much toward giving us a just idea of the physical geography of our continent, during the different geological epochs. Acting on this conviction in such parts of the continent as I have visited, the fossil plants found, and the nature of the sediments containing them—generally the direct debris of the ancient land—have been to me objects of special interest and attention.

The general results of these observations on the extinct floras of North America may be very briefly stated as follows:

1st. The flora of the Devonian and Carboniferous epochs in America was, in all its general aspects, similar to that of the Old World, which has been so fully described; most of the genera, and a larger number of species than at any subsequent period

having been common to the two sides of the Atlantic. The relative number of identical species has, however, it seems to me, been somewhat overrated. In many of the species regarded as the same in Europe and America, the American plants present prevalent or constant characters which may serve to distinguish them. These differences, though frequently remarked by writers, have not been thought to have a specific value; yet it is quite certain that they are as tangible and important as those which now separate many American and European species of recent plants and recent or fossil animals. I have a conviction that the progress of science will considerably diminish the proportion of identical species; a closer scrutiny and more extensive comparison of specimens resulting in the discovery of constant, though inconspicuous characters which shall be ultimately conceded to be specific.

It is true also that in molluscous palæontology, recent geology and botany, the number of species common to the two continents has been considerably reduced of late years; a large number of American representatives of European species at first considered identical from their striking and obvious coincidences, having, on closer study, afforded constant, though less conspicuous differences.

2d. The Permian, Triassic and Jurassic rocks have hitherto furnished us but few species for comparison, but the material is increasing, and I have now on hand quite a collection which has not yet been studied. Enough is already known to show that the great revolution which took place in Europe at the close of the Permian epoch was matched by a parallel though less sudden change in the flora of America.

Here as there the Lepidodendroid trees, the *Sigillariæ*, the *Nœggerathiæ*, the *Asterophyllitæ*, and the great variety of ferns that gave character to the Carboniferous vegetation, were superseded by *Voltzia*, *Tæniopteris*, *Camptopteris* and a varied and beautiful Cycadaceous flora, in which were many species of *Zamites*, *Pterophyllum*, *Nilssonia*, etc., the representatives of those of the "Age of Gymnosperms," which culminated in the Jurassic epoch of Europe.

During this great interval the generic correspondence between the floras of Europe and America was perhaps as plainly marked as during the Carboniferous age, but the relative number of identical species was apparently smaller.

3d. At the commencement of the Cretaceous epoch the flora of the continent was again revolutionized and the vegetation of its temperate portions given the general aspect that it now presents.

This statement will surprise many, for the flora generally ascribed to the Chalk period is greatly different from that of the

present. Unger has thus represented it, and Brongniart calls it a transition from the great Cycadaceous flora, of the Jurassic period, to the Angiospermous flora of the Tertiary. In Europe the Cretaceous flora was, apparently, more like that of the Lias and Oolite than in this country, for while the genera *Salix*, *Acer*, *Populus*, *Alnus*, *Quercus*, &c., were then introduced there as here, its general aspect was modified by the presence of numbers of *Cycadaceæ*, and its sub-tropical character attested by fan-palms.

We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but as far as at present, known, our plants of this age present an *ensemble* quite different. I have now some sixty to seventy species of Cretaceous plants collected in New Jersey, and in various parts of the great Cretaceous area of the interior of the continent, all of which indicate a flora very similar to that now occupying the same region; many, perhaps most, of the genera being now represented in our forests—such as *Liriodendron*, *Platanus*, *Acer*, *Populus*, *Salix*, *Alnus*, *Fagus*, &c. These specimens have been collected in localities included between the 36th and 41st parallels of latitude, but range from the 74th to the 110th of longitude. Nowhere within this area have I yet detected any traces of palms or any indications of a tropical climate. At the base of the Yellow sandstone series of New Mexico, (Lower Cretaceous,) I have found a varied and interesting flora, containing *Pterophyllum*, *Nilssonia*, *Camptopteris*, &c., with a few Angiosperm dicotyledonous leaves. This is evidently the point of junction between the Cycadaceous flora of the Jurassic age and that of the Chalk; for in the entire overlying Cretaceous strata, 4000 ft. in thickness, though Angiospermous leaves are abundant, those of Gymnospermous plants were nowhere discovered, nor any traces of palms, either leaves or stems. The sandstones of the Cretaceous series contain immense numbers of silicified trunks but they are for the most part coniferous.

4th. For the glimpses I have obtained of the Tertiary flora of North America I am mainly indebted to the kindness of Dr. Hayden, who has spent several years in most successfully exploring the geology, botany and zoology of the country bordering the Upper Missouri. Among his rich collections are fifty or more species of beautifully preserved fossil plants from the Miocene, which have been put in my hands for examination, and of which descriptions will be published immediately after my return to Washington.

Not having the specimens, or my notes on them, with me, I can speak only generally of the flora they represent. I remember, however, that they include species of *Platanus*,—one of which closely resembles Unger's great *P. Hercules* and is perhaps

as large; *Populus*, *Acer*, *Castanea*, *Sapindus*, *Carpinus*, *Ulmus*, *Diospyros*, *Quercus*, *Salix*, *Taxodium*, and others which indicate a flora in all its general aspects similar to that now occupying the Valley of the Mississippi. A few plants in the collection would seem to have required a somewhat warmer climate than that which the localities where they are found enjoy at present; but there are no palms among them, nor any of the tropical genera *Cinamomum*, *Sterculia*, *Dombeyopsis*, &c., so common in the Tertiary strata of Europe.

In the enumeration of the Miocene plants of the Pacific coast, given by Mr. Lesquereux in the May number of this Journal,* I find also evidence of a marked and interesting difference of temperature during the Tertiary epoch, in different parts of the North American continent, under the same parallels of latitude. Mr. Lesquereux finds in Dr. Evans's collection Palms *Salisburia*, *Cinamomum*, &c., which indicate, at least a sub-tropical climate; a flora quite unlike that from the Miocene of the Upper Missouri, although, as he remarks, similar to that of the Miocene of Europe.

I am tempted to dwell for a moment on the interesting glimpses of the physical geography of our continent in geological times, which these facts and others that have come under my observation afford; for, to you, who have done so much toward the elucidation of its geological history, this cannot, I am sure, be a matter of indifference, but my letter has already grown to an unreasonable length. Let me then close with a few generalizations, referring you to my reports for all details of fact sustaining them.

1st. A large continental area occupied the place of the interior of North America from the earliest Palæozoic ages.

2d. During the Carboniferous epoch this land sustained a vegetation similar to that of the Coal period of Europe and Eastern America, though far less varied.

3. Through the Triassic and Jurassic ages the sediments from the land were strikingly like in mineral character to those of the same age in the Old World: and the flora was characterized by a preponderance of Cycadaceous plants, analogous to those of the Jurassic of Europe.

4th. In the Cretaceous age, the central nucleus of the continent was sufficiently extensive to furnish from its ruins arenaceous sediments that now cover more than half a million square miles. These sediments contain vast deposits of carbonaceous matter, mainly derived from the land plants which covered the continent. As far south as lat. 35° these plants were for the most part Coniferous or Angiospermous, and included many genera now characteristic of temperate climates.

Through the Tertiary epoch our continent had nearly the form and area it now has, the Tertiary deposits merely skirting its

* xvvii, [2], 361.

borders. The Marine Tertiaries are nearly limited to the shores of the present oceans, while the patches of strata of that age found nearer the centre of the continent are all, so far as I have observed or heard, of fresh water or estuary origin. Between the western base of the Sierra Nevada and the Mississippi there are, I believe no Tertiary beds not of this character, and the larger part of the great central plateau has never been covered with Tertiary or Drift sediments, but has, since the close of the Cretaceous epoch, been as now, dry land.

The facts which I have enumerated seem to indicate that over this ancient land the isothermal lines were curved much as now, and that during the Tertiary ages there was, perhaps, as great a difference between the climate of the Pacific and Atlantic water sheds as exists at present.

ART. XXI.—*Abstract of a Meteorological Journal, kept at Marietta, Ohio:* lat. 39°·25 N. and lon. 4°·28 W. of Washington City; by S. P. HILDRETH, M.D.—For 1859.—[Thirty-third annual report.]

1859. MONTHS.	THERMOMETER. Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Inches of rain and melted snow.	Prevailing Winds.	BAROMETER. Maximum.	Minimum.	Range.
January,	33°·00	63	4	17	14	3·10	W., S.W.	29·85	28·95	0·90
February,	37·77	62	10	10	18	7·20	S.W., & E.	29·60	28·85	0·75
March,	48·48	77	25	12	19	5·08	S.E., & S.W.	29·70	28·80	0·90
April,	52·03	83	28	14	16	6·46	N., & N.W.	29·60	28·85	0·75
May,	67·20	90	46	21	10	1·56	S.W., & N.	29·60	29·10	0·50
June,	67·23	93	33	17	13	4·62	S., W., & E.	29·70	29·20	0·50
July,	74·23	102	49	23	8	1·08	E., S.E., & N.	29·73	29·20	0·53
August,	72·10	95	46	15	16	4·46	S., & N.	29·53	29·30	0·23
September,	63·51	86	46	12	18	4·95	S., & W.	29·70	29·10	0·60
October,	49·12	79	26	17	14	2·79	S.W., & N.W.	29·65	28·85	0·80
November,	45·50	72	20	20	10	2·08	S.S.W., & E.	29·85	29·00	0·85
December,	30·48	71	5	12	19	5·17	N. & W.,	29·75	28·80	0·95
Mean for year,	53·38					Rain 48·55 inches.				

☞ *Remarks on the seasons.*—The mean temperature for the year 1859 is 53·38, which is somewhat above the average for this locality.

The amount of rain and melted snow is 48·55 inches. The average in a series of years, being forty-two inches, falling occasionally to thirty-two inches, and again rising, as in 1858, to near sixty-two inches, so that our climate is quite variable in this respect. The number of cloudy days bear testimony to the humidity of the year.

Winter.—The mean of the winter months is 37°·14, which may be considered very mild for this latitude. The thermometer was at no time as low as zero, so that it did not make ice sufficiently thick for filling ice houses, only two or three inches being the extent of the best, and the main supply for summer use was brought from the heads of the Muskingum river. Steam boats were laid up only a few days, by the floating ice, during the winter. In the eastern states the cold was excessively severe. On the tenth of January, after a great snow storm on the eighth of that month, extending from the borders of Pennsylvania to Maine, the mercury sunk in Salem, Mass., to 23° below zero, in the city of New York to 11° below, and at Ogdensburgh, N. Y., to −38°. The extreme cold on the Atlantic coast was said to be greater than at any time during the last seventy years.

Spring.—The mean for the spring months is 55°·90, an unusual high range, being nearly four degrees above 1858, and more than ten above 1857, that being only 45°·89, so that there is a wide range in the temperature of our springs, which is most strikingly apparent in the blossoming of trees, especially that of the peach, there being a variation of not less than forty days in the opening of the fruit buds of this highly prized tree. The later the bloom is retarded the greater the chance for a crop, but so variable is the climate of southern Ohio, that only one season in three can be counted on for the production of this delicious fruit. The apple crop is rather more certain, and yet nearly every other year is a failure from the blighting effects of late spring frosts. But for this drawback it would be one of the most productive countries in the world in fruit, and the valley of the Ohio as celebrated pomologically as it now is for the growth of Indian corn. The unusual heat of the spring is chiefly attributable to the month of May, which was 67°·20, or six degrees above the average temperature, which is sixty-one degrees. The heat was nearly that of June, and rarely experienced, as there is commonly a difference of ten or twelve degrees in these two months. The peach was in blossom this year on the 28th of March, and the apple on the 12th of April. In 1857 the peach opened on the 2nd of May, and the apple on the 9th.

Summer.—The mean of the summer months is 71°·19, which is not much below the average, notwithstanding the uncommonly low temperature of June. The unprecedented occurrence of a severe and destructive frost as late as the fifth of June overwhelmed the country with fear and astonishment; at a period in the growth of wheat usually considered as past all danger of this kind, a sudden change of temperature in one night spread destruction and ruin to a large portion of the fields of this important cereal over all the central portions of the valley of the

Ohio, and extending from Iowa to northern New York. Indian corn shared largely in this calamity. The warmth of May had hastened the growth of this plant in many fields to the height of twelve or eighteen inches. In all such cases the fields had to be replanted, but where it was only a few inches above the surface it recovered from the injury, and produced a fair crop. The wheat being in full head, and much of the grain in the milk, was entirely ruined. Potatoes were badly frosted, but in a good measure regained a healthy state. Peaches and apples, which in most orchards had attained the size of almonds, were so much damaged as to fall from the trees in a few days, and only certain favored localities ripened any fruit. So serious an injury from untimely frost has not been experienced since the first settlement of the state in 1788. In the year 1834, severe frosts visited Ohio as late as the middle of May, but the wheat crop was not so far advanced, being only in blossom, and by throwing up new stalks from the uninjured roots, produced finally an abundant harvest. Providentially the autumnal frosts of 1859 were retarded until near the close of October, and the late planted fields of corn were fully matured, to the great delight and wonder of the husbandman, for the failure of this grain would be a more serious calamity than that of wheat, as both man and beast largely depend on it for sustenance.

Autumn.—The mean for the autumnal months is 52°·71, which is rather below the average, but was sufficient to ripen all the late crops. Sweet potatoes were uncommonly good in quality, and abundant in quantity. Buckwheat was largely cultivated, partly in place of the common grain, and produced a great yield. The Catawba and other grapes ripened well, and abounded in saccharine principle, so necessary in making good wine. The smaller fruits were plentiful, so that on the whole we have more cause to be thankful, rather than to complain of the dealings of Providence in the past year.

Floral Calendar, &c.—February 25th, Bluebird heard; 27th, Yellow garden crocus in bloom; March 4th, Many birds of passage seen and heard; 9th, White crocus; 12th, Hepatica triloba; 14th, Golden bell or Forsythia viridis, Acer rubrum, (Red maple), Ulmus Americana; 16th, Hepatica acutifolia; 21st, Grass quite green in pastures; 22d, Magnolia conspicua, Claytonia Virg.; 26th, Red cherry, Balm of Gilead, and Sugar maple; 27th, Crown Imperial; 28th, Peach tree, Red Pyrus Japonica, Hyacinths; 29th, Sanguinaria Canadensis; 31st, Gooseberry.—April 1st, June berry; 2d, Dandelion, Pink colored Pyrus-Japonica, Cherry and Plum, Primroses; 4th, Flowering almond, Anemone nemorosa; 5th, Phlox divaricata, Dielytra cucul.; 7th, Annona triloba, Papaw; 11th, Burgundy pear, Trillium grandiflorum; 12th, Double flow.-Peach, Siberian crab, Spiræa prunifolia, Cercis Canadensis, or Red bud; 13th, Apple tree;

14th, Chickasaw plum, Strawberry, Sedum ternatum; 17th, White ash tree; 22d, Cornus Florida; 24th, Dodecatheon Amer.; 27th, Harebell; 28th, Vernal snow drop; 30th, Tulips.—May 2d, Tree peony, var. papaw; 4th, Haw tree, Dicentra spectabilis; 5th, Mountain ash, Aquilegia Canad.; 6th, Magnolia tripetala, Viburnum, (Snow ball); 8th, European Horse chestnut; 10th, Rose colored peony, Yellow Harrison rose; 12th, Blackberry and Robinia Pseudacacia; 15th, Rose Acacia and Annual roses; 16th, Iris Persica, Crimson peony; 17th, Purple peony; 18th, Moss rose; 19th, White peony, also several new peonies from seed planted five years ago, bloom first time; 21st, Peas on table, planted in January; 22d, Syringa Philadelphica, Strawberry ripe; 24th, Bulbous Iris; 26th, Foliage of trees unusually rich and fine; 27th, Fragrant peony, and large rose colored; 28th, Linocera flexuosa; 30th, Erigeron annuum.—June 1, Star of Bethlehem; 2d, Common cherry, ripe; 5th, Severe frost, killing wheat, corn and fruit, made ice in a bowl of water, half an inch thick, a few miles west of Marietta; 12th, Canterbury bell in bloom; 19th, Red raspberry ripe; 23d, Pennsylvania lily in bloom; 27th, Wheat harvest begins in fields that escaped the frost, on high hills, or where protected by the fog from the rivers; 28th, June apple ripe.—July 5th, Chestnut tree in bloom; 8th, Sweet bough apple and Gravenstein ripe; 12th, Gladiolus flori; 13th, Blackberry ripe, but a large portion destroyed by frost, American broom in bloom.—August 8th, Catherine pear ripe; 13th, Watermelon ripe.—September 1, St. Michael pear; 15th, Seckel pear fully ripe.

In every month during the past year there has been more or less frost, as in the year 1816.

ART. XXII.—*Geographical Notices;* by DANIEL C. GILMAN.

No. XI.

BIOGRAPHICAL SKETCH OF DR. KARL RITTER.—The death of Dr. Karl Ritter, the father of the modern science of Physical Geography, and one of the most eminent and beloved of the scientific men of Germany, has already been announced in this Journal. We present herewith a sketch of his life, translated and condensed from a highly interesting tribute to his memory which is attributed to the pen of Dr. Kramer of Halle, in the Berlin *Zeitschr. fiir allgemeine Erdkunde.*

CHARLES RITTER was born at Quedlinburg the 7th day of August, 1779. His father, a man of noble character, fine feelings and a pious mind, was physician to the Abbess of the Convent there, and was much esteemed for his skill. However, in consequence of the slanders of an envious individual, he lost the

largest part of his practice, and, although his good name was restored after the lapse of two years, and his clients returned, grief and sorrow had so heavily weighed upon him during this time, that in the full strength of manhood, he succumbed to a typhous fever. He left an almost destitute widow with five little children, of whom the eldest, a boy, was ten years of age, the fourth, Charles, only five years old. This situation of the poor widow, a noble and highly educated woman, excited the utmost sympathy of her neighbors. All endeavored, either by words, or in a more substantial way, to make good the wrong which had been done to her husband. She found many sympathizers, away from home. Thus, the Prince of the adjoining Bernburg took care of the education of the eldest boy. Salzmann, the celebrated educator, a former associate of Basedow, had bought Schnepfenthal, and was about to open an educational institution there. He had made it a point to take a boy as his first pupil, gratis. A notice in a journal of the death of Dr. Ritter at Quedlinburg, who had left a widow with five little children, first attracted his attention. Soon after he sent two of his friends there, to make the acquaintance of the children and to see whether there was a boy amongst them, that would conform to his wishes. They decided in favor of little Charles. The mother, though with a sorry heart, assented, and, at the invitation of Salzmann, brought the child herself to Schnepfenthal. She was accompanied by one of her elder sons and Gutsmuths, then a candidate of theology and instructor of the children, who had not left them, although the mother had declared that she was no longer able to pay him his salary. A residence of a few days in Salzmann's house cemented the ties of mutual friendship and esteem, so that Salzmann, shortly before their departure, expressed a wish to keep the older boy also. To Gutsmuths he proposed to remain in Schnepfenthal as a teacher. This had been a secret wish of the mother, but she did not think it possible. Ritter accordingly came to Schnepfenthal, the first pupil of the new institution, and remained there for eleven years, until he went to the university. This lovely spot, which Ritter always considered his true home, was situated at one end of the 'Thueringer Wald' and was surrounded by a most charming landscape, having in one direction a view of a far extending fertile plain, richly adorned with cities and villages; in the other, there rose well timbered mountains of various shapes, intersected by fine valleys. All around was activity and life. Here he received from his early youth the most vivid impression of the glory of God's creation, of the variety of formations on the surface of the earth, and their special relations to the life upon them. Here Ritter grew up under the guidance of excellent men and skillful teachers. Those that ex-

erted the greatest influence upon him, were Salzmann himself, Bechstein and Gutsmuths, the latter of whom continued here also, to take special care of young Charles, and probably implanted in him a love for geographical knowledge.

The method of instruction was that suggested by Basedow, and tried first in the Philanthropin at Dessau, but it was freed from those vain and needless peculiarities that adhered to it there. Classical languages were less studied, but the most attention was paid to all those sciences and accomplishments, which stand in direct relation to life, and among these the modern languages occupied a more prominent place, than anywhere else. To this an unusual impetus was given by scholars from different countries, who thronged, soon after the opening of the institution, to Schnepfenthal. By physical training, and by strengthening the character and intellect, a general and equal development of body and mind were especially aimed at, and, although practical rationalism pervaded the whole institution, darkening a little the deepest sources of true blessings, there still reigned piety, love, and the purity of high moral sense. Under these influences all those noble qualities of Ritter's heart and mind were developed, that distinguished him so much in after days. The future lay dark before him, and he had not decided upon his course in life, but he felt a strong desire to study, of which however there was as yet no prospect. His mother, though married again several years after the death of her first husband to the celebrated pedagogue Zerrenner, was not able to provide for him. But Providence interposed here also. A rich merchant from Frankfurt on the Main, associated with the large firm of Bethmann, Mr. Hollweg, visited the institution at Schnepfenthal and became very much interested in the young Ritter. After hearing of his circumstances, he declared himself willing, on the recommendation of Salzmann, to furnish the necessary means of study, upon the condition however, that Ritter, after the completion of his studies, should enter Hollweg's house as instructor of his children.

So Ritter went, at the age of 17 years, to the University of Halle, and was matriculated November 2, 1796, as *studiosus cameralium* under the prorectorate of Curt Sprengel. Here he remained for two years. Halle was then the centre of great scientific activity. F. A. Wolf especially was then in the height of his renown. Ritter did not pursue a specific course of studies, as his previous education had not been directed towards that channel, which however he sometimes regretted in later life. He often mentions A. H. Niemeyer, to whose circles he had access, and in whose house he lived, and who exercised upon him an important influence in improving his mind and inciting him to farther study. Niemeyer occupied then a prominent place

in the pedagogic world, and that work by which he became most known, "Grundzuege der Erziehung und des Unterrichts" (or, Principles of education and instruction), first appeared in 1796, and must have been of special interest to Ritter, as he was himself preparing for the calling of an instructor.

In 1798 Ritter left Halle and entered Hollweg's house as instructor of his four children, especially of the two boys, one six, the other three years old. It was a great change for the young man of 19 years, to step out from the quiet circles in which he formerly had moved, into the midst of a world quite unknown to him, and to move among the aristocracy of a mercantile city. He had to struggle with many difficulties. But he went to his task in all earnestness, and with the ardor of a true and powerful mind, conquering all impediments so completely, that he gained results such as but few instructors can boast of. This was especially true in the case of his younger pupil, the other having died in the bloom of his youth. Ritter conducted the education of the former until he went to the University, and this pupil is the present minister of clerical, educational and medicinal affairs in Prussia, von Bethmann-Hollweg. Equally successful was he in the education of a son of the celebrated S. Th. Soemmering, and out of this relation of teacher and pupil grew the most intimate friendship and love, which lasted for life. During his stay in Hollweg's house Ritter came in contact, and even into nearer relations, with many eminent men, and by the intercourse with them his ideas expanded and became freer and more independent. Amongst those that exerted the greatest influence in this respect, S. Th. Soemmering must be mentioned above all, a man of great genius and deep scientific knowledge. Ritter thus speaks of him in the introduction to the second edition of his 'Erdkunde :' 'If in the explanation of the laws of the geographical relationship of all animated nature there should be prominent some interesting opinion and view, then the author is indebted for this whole tendency of investigation to the long, instructive, and I say it with pride, familiar intercourse with a noble man, S. Th. Soemmering, a man who is an honor to his century and nation; for his spirit filled others also with the premonition of the depth of nature, which his own genius had penetrated into its most hidden mysteries.' Ritter was also befriended at Frankfort by J. G. Ebel, the author of the classical work on Switzerland. This was not only of the highest importance for him during his repeated travels to Switzerland in relation to the knowledge of this country, but it also impelled him to farther study. Ritter, speaking of Ebel, says in the above-mentioned introduction: 'What the present work may have of vivacity and warmth, it owes to an intercourse of many years with this excellent man at the time I commenced it.'

Ritter's mind moreover was much aroused by the daily intercourse with men of equal aim, and inspired with the same ardor for the education of youth, such as E. Mieg and J. B. Engelmann. Besides he sometimes came into transient but important intercourse with men of eminence who travelled through Frankfort. He met (to mention only a few names) Alexander von Humboldt and Leopold von Buch in Hollweg's house. But even life itself in this old interesting city, uniting so many instructive elements, showing so many different relations of a most various character, and being situated in the midst of the district of the largest river in Germany, always invited to new observations, wanderings and study.

Ritter used with the greatest ardor all these opportunities to acquire information. The time of his sojourn at Frankfort was a time of the most various studies. So he applied himself with much zeal to the classics, and read with the assistance of his friends, F. C. Matthiae and J. F. Grotefend, then at the head of the gymnasium at Frankfort, the most important works of the Greeks and Romans, but the tendency of his mind towards geography always appeared with marked prominence. In order to become entirely at home in this department, he not only studied thoroughly the most important works on the subject, but he also made observations of his own in frequent excursions to different parts of the country. The ability to draw with the greatest ease those objects in a landscape which were important to him, and so to fix them forever, was of much service to him. He always brought a number of characteristic sketches home from his journeys, which served both for himself and others as illustrations of his observations. This tendency towards geography was manifested in his first contributions to the 'Neuen Kinderfreund,' edited from 1803–1806 by Engelmann in connection with his pedagogic friends, but it became yet more apparent, when in 1806 he published his six charts of Europe, and not long after, in 1811, when his geography of Europe (2 vols.) appeared. In both works the peculiarity of his geographical perception is thus early indicated. They are the groping essays, the *incunabula* of what was lying in his mind. But, before his ideas could come to maturity and light, other preparations were to be made. As such, in different respects, must be considered the journeys which he, from the year 1807, repeatedly undertook with his pupils to Switzerland and Italy, and the last of which, commenced in 1811, comprised several years. These travels must indeed have been a rich source of instruction to his observing and powerful mind, that was so well prepared and matured by assiduous study and labor. Just these countries are the most expressive representations of the most important and most various geographical types which Europe has to show.

Switzerland, the most important parts of which he crossed in different directions, impressed him deeply with the grandeur and glory of a majestic and infinitely rich nature, which invited irresistibly to the study of this gigantic structure. Italy, on the other hand, which he passed through down to its southern point towards Sicily, furnished him important information in reference to volcanic activity, exhibited to him the relation of land to sea, showed the effects of climatic differences and the close connection of the nature of the country with the development of its people. Those treasures of art, which Italy possesses so abundantly above all other countries, must have added their proper share also to his store of knowledge laid up in his naturally fine and carefully educated mind.

During his travels in Switzerland he met many men of eminence and note; amongst them Pestalozzi, von Tuerk, Niederer and many others of that district. He spent many happy and instructive hours in their society, and between him and many of them a mutual friendship was established. He always remembered Pestalozzi, whom he often visited at Iferten, with reverence and gratitude, and had a picture of him in full length in his study.

The most important point in Switzerland for him was Geneva, where he remained from the middle of the year 1811 for more than twelve months. This city was then particularly noted for the active part it took in the cultivation of science, especially natural science, and was much distinguished by the fine tone of its society. Saussure, the first man of the city and the state, had shortly before died, and his pupils, men of European fame, such as M. A. Pictet, de Candolle and others, were considered the centres of the higher circles. With the first, Ritter became very intimate and to him he owes many valuable hints. St. Gervais, close at the foot of Mont Blanc, was another very interesting point for Ritter, as it offered to him an opportunity of observing the nature of high mountains in all their details. From this spot he made that tour around the Mont Blanc, of which he gives such an interesting and instructive description, in explanation of the bas-relief executed by Kummer.

During his travels in Italy, Rome most especially attracted his attention. It was not only the centre of numerous monuments of history and art, but he also met here men like Thorwaldsen, Overbeck, Cornelius, and others, who by their genius and love for the arts, had raised them again to a most flourishing state. By intercourse with such men, Ritter's insight into the nature of Art was much enlarged.

Thus variously enriched, he returned home, and soon commenced that work, which was the chief production of his whole life, and which will make his memory immortal. In order to

prepare it, he went in 1814 to Goettingen with his two pupils, who at that time were beginning their academical studies. Here he used all the means of learning within his reach, he searched out the rich treasures of the library, was in active intercourse with the masters of science, (Hausmann especially was dear to him,) and did not disdain to enter again as a student the auditoriums of the professors and to hear lectures on the most various subjects. After a residence of two years he went to Berlin (1816) and there gave his work its last finish, after which it was put to press. Next year he went again to Goettingen to superintend the publication of his work, of which the first part in 1817 with the title, 'Erdkunde im Verhaeltniss zur Natur und Geschichte des Menschen oder allgemeine vergleichende Geographie als sichere Grundlage des Studiums und des Unterrichts in physikalischen und historischen Wissenschaften,' or, Geography in relation to the nature and history of man, or general comparative geography as a safe foundation in studying and teaching physical and historical sciences. In this work geography had been entirely remodelled and changed. It had indeed been raised to the rank of a true science, constituting the link between the natural sciences and history. The first part contained Africa and part of Asia; a year after, the second part appeared, which completed Asia.

We abstain from giving a detailed description of this work, as it is well known, and as the method, in which geographical matter is treated here, has been adopted throughout the whole scientific world. Ritter's aim is briefly indicated by its title; a more detailed account of its leading ideas however is given by himself in his introduction to the second edition of the first volume, which appeared in 1822. Ritter's intention was, to give with the greatest accuracy a vivid image of the formation of the superficies of the earth in its horizontal and vertical dimensions by means of a conscientious and careful use of all existing sources, and to represent and explain the characteristic qualities of its parts and their relation to each other and to the whole earth, but at the same time to make it serve as a substratum of all animated nature, and as a foundation and condition of the development of the different nations and the whole human species in their manifold mutual relations to one another.

This was a stupendous task, but Ritter performed it marvellously well. Its execution required a combination of great and varied talents, such as rarely ever have been or will be found, cultivated by deep and assiduous study. In it we see powerful and truly ingenious displays of general geographical intuition and combination, we perceive a care, that indefatigably penetrates into the deepest recesses and most minute details, we find evinced an extensive knowledge of the natural sciences

and a perfect command of extensive historical materials, and lastly, a truthfulness and thoroughness of learned inquiry combined with the rare gift of a rich, fresh, vivid and expressive representation. Truth and knowledge of the living God were the springs that actuated his mind and after which he aspired. Hence his humility, hence his close and perfect application, his concentration upon the subject before him. No difficulty ever deterred him in his investigations, although the matter before him was continually and vastly accumulating. His work was to him, as he wrote in his diary, when, after a long interruption, he again commenced his labor, 'his song of praise to the Lord.'

When Ritter had completed the first two volumes of his geography (Erdkunde), in connection with a work that was the direct result of his Asiatic studies, 'Vorhalle Europaeischer Voelkergeschichten vor Herodotus um den Kaukasus und an den Gestaden des Pontus,' or, 'Vestibule of the history of European nations before Herodotus around the Caucasus and at the shores of the Pontus,' he received in the year 1819 a call to be Professor of history at the Gymnasium of Frankfort. In the fall of the same year he married. In September 1820 he accepted a call as Professor extraordinarius of history in the Military School and the University of Berlin.

At Berlin the second half of his life commences, and here the richest fruits of his former labors and preparations matured. Berlin was indeed the most favorable city for such an object as he had in view. Nowhere were the means for study and instruction so abundantly and generally supplied. Both the University and the Military School displayed great scientific activity, which was kept alive by eminent scientific men, into whose circle Ritter soon entered as a highly esteemed member. His lectures were soon well attended at both institutions. Besides he kept up an active intercourse with other scientific celebrities, among whom Leopold von Buch, but especially Alexander von Humboldt may be mentioned, whom he highly esteemed, and with whom he lived on terms of the most intimate friendship. Moreover, Ritter lived here in the midst of his nearest relations; one brother lived in Berlin, another near the city, and among his dearest friends, was his former pupil, Hollweg, then also professor at the University. All this encouraged him much, and instigated him in various ways to still farther study and advancement. Ritter's activity was now, besides the duties of his office, chiefly directed towards preparing a second edition of his Erdkunde, of which the first volume appeared in the year 1822. It was much enlarged and in every respect more complete than in the first edition, although now only comprising Africa. The continuation of this work however suffered a long interruption

and the chief reason for it was, that his official duties claimed his attention more and more. Accordingly he entered, though only for a short time, as a member, the scientific examining commission for history and geography, and soon after the death of his friend Woltmann he also undertook the historical lectures at the Military School.

In 1825 he was appointed Director of Studies to the corps of cadets. Besides, he instructed Prince Albrecht of Prussia in history for many years, and received, especially during the winter months, frequent invitations from the Crown-Prince to lecture on history and geography before him and some of his nearest relatives and friends. Similar invitations came also from other sides, and he very often complied with them. Thereby a very considerable part of his time and strength was claimed, as he was always wont to apply the utmost care and scrupulousness to all his subjects. Nevertheless by his extraordinary diligence, which was a necessity to him, assisted by a robust healthy body and by his collectedness and freshness of mind, he still found leisure to work and to promote and advance his scientific labors, which were never lost sight of. The external fruits of these labors were however confined in that period, to his reports in the Academy of Science, of which he was a member after 1822, and to brief essays, like the one on India in the Berlin Almanac for 1824. Many of the results of his studies were communicated to the geographical society, which he, in 1828, had founded in connection with several friends, and of which he was the chief supporter. Those travels which he regularly undertook during the long fall vacations were of the highest importance for him in every respect. They not only served him for bodily and mental recreation, but were also very useful for the advancement of his geographical studies, whether he was occupied in the observation of nature itself, or by investigations in important geographical centres, as Vienna, Paris, London, and other places. These journeys extended in very different directions over the countries of Central Europe and sometimes occupied the larger part of summer. The most extensive and important were: a tour to Greece, Constantinople, through Bulgaria, Wallachia, Sieben-Burgen and Hungary; repeated travels to Paris, through the southern, and at another time, western part of France, and the Pyrenees; through Belgium and Holland; through Denmark, Sweden and Norway; to London, and through a part of England. He often visited and explored, but always in different directions and with different objects, the middle and southern part of Germany, the system of the Alps in its different parts, Switzerland, and the northern part of Italy. The results of these journeys were a great variety of impressions and observations (mostly recorded and narrated in his detailed

and highly interesting letters to his family), oral communications of a most varied character, and the establishment of manifold personal relations and connections.

In the year 1831, Ritter withdrew from all business and labor foreign to his geographical studies. He felt that if he would advance geographical science, which he considered the task of his life, he must concentrate his powers. Thus, when he again had leisure, the fruits of his studies became more apparent. There now appeared, from the year 1832, in quick succession, that series of volumes on Asia, of which he concluded the nineteenth a few weeks before his death. This work will be a lasting monument of his genius, and a standard work for all ages, however great the progress of geographical science may be hereafter.

The author's name grew in proportion with the progress of the work, his acquaintances increased in all civilized countries of the world, and his influence upon the course of geographical investigation and science was greatly augmented. He became one of the most important personal centres for the science, since he possessed an immense store of knowledge, and a sound judgment, and took a most active interest in all questions relating to this subject. He entered into everything, even if trifling or troublesome, with an amiability and urbanity that never tired.

He received marks of acknowledgment and distinction of all kinds. Most of the learned societies, in and out of Europe, made him a member, many Orders were given to him, and his Sovereign gave him frequent proofs of his personal favor during the many years of his residence at Berlin. Ritter occupied undoubtedly, as savant and author, one of the most exalted positions among his contemporaries. But he was not less great as a teacher also. There were few lecturers who exercised such an invariable power of attraction as he did. When in 1820 he first announced his lectures on general geography, no hearers came forward, and in the course of the term only a few presented themselves. Ritter began his lectures, but even in the next semester hearers were yet scarce, and this must not be wondered at, as but a few of the students had heard anything of Ritter, and the great majority of them considered geography as something hardly worth the hearing. This state however soon changed, and already in 1823 Ritter wrote in his diary: "Full auditory, I must take a larger one." The numbers of his hearers increased from year to year, so that sometimes even the largest auditory scarcely could hold them. Ritter was now looked upon as the one whose lectures had to be attended by every student of a high scientific aim. Which of his numerous hearers does not remember with gratitude the pleasant and instructive hours of his lectures? Ritter showed a most perfect

tact, acquired by many years' experience; he knew how and what to select from the immense store of matter over which he had a perfect command; he knew what was best adapted to a verbal exposition and from what the greatest benefit would be derived. His delivery showed that he had deeply and thoroughly mastered his subject, which he always elucidated by drawings, placed on the black-board with great ease. Every one of his hearers felt the importance of the subject, perceived Ritter's deep scientific researches, and was delighted at the results, which were made so accessible, and at the improvement gained by means of them. His lectures were always instructive and always excited to farther study. His delivery was dignified, but everywhere and under all circumstances unassuming. His purity of mind, his modesty and amiability shone forth everywhere, and exercised a peculiar charm especially on those that came in closer contact with him. None ever approached him without meeting a most friendly and hearty reception. No effort in science, however imperfect, was made, that he did not acknowledge and encourage by his counsel and assistance. Egotism was entirely foreign to him; he was the truest and most affectionate friend; in his family most tender-hearted and loving, and his greatest pleasure was to see those around him happy. He himself was without children, but he was a father to many that were comparatively strangers to him. His mildness of temper exercised a most soothing influence upon all; his peace of mind, pervading his whole nature, could not easily be shaken, even by severe losses, such as the death of a dear sister and of his beloved wife in her full strength, both occurring in the course of a few days. These noble qualities of mind were the precious fruits of a strong and living faith. Ritter was a Christian in the full meaning of the word, although never saying very much on the subject and never raising himself up as judge of the religious belief of others. The holy word of God accompanied him everywhere, and to confirm its truth by the results of his investigations was his highest joy. His own words found after his death, are the best testimony: 'Although at present, while preparing for a journey to the western part of France and the Pyrenees, I am healthy and well, life nevertheless lies in the hand of God, whose mercy and grace has guided my fate so wonderfully and gloriously, that I cannot but sing to him, the Allgood, praise and glory with all my power, in all my thoughts and actions. Should it not please him to let me return to my beloved family and to my calling, but should he assign to me another place in his heavenly kingdom, that I may obtain happiness, a happiness which already here sometimes has moved me to tears of joy, then I ask my friends not to grieve over my going home, for all that the Lord does is done well.

My eternal fate my Savior in his great mercy will decide. In deep acknowledgment of my infirmities and sins, I am still full of trust and confidence, since I know that my Redeemer lives, who will make his people partakers of the mercy of the Eternal and Just one.'

Ritter's health was generally good. His constitution was strong and was hardened by exercise. His numerous travels, often on foot, renewed his strength, when weakened by close application to his fatiguing scientific labors. In his last years, however, many infirmities were felt. The Teplitz medicinal springs had relieved him several times, and Ritter tried them again in July 1859, but this time without relief. Great heat and hot baths seem to have weakened him more, and this weakness was still more increased by frequent hemorrhages from the bladder; he lost his appetite, and his strength begun rapidly to fail, even when his appetite partially returned. He died September 28, 1859, at 10 o'clock A. M., and was interred October 1, in the Marienkirchhof by the side of his beloved wife, who died in 1840.

MR. LENTZ'S REPORT OF HIS EXPLORATIONS IN PERSIA AND AFGHANISTAN.—We have received through the Smithsonian Institution, Washington, reports of the meetings of the Imperial Geographical Society of St. Petersburg, Oct. 7, and Nov. 4, 1859. From the former of these we translate the following account of the Russian expedition under Messrs. Khanikoff and Lentz into Afghanistan and Persia.

Mr. R. Lentz, who took part in the expedition to Khorassan, presented some interesting information in regard to the scientific results of his travels during the sixteen months which he passed in Persia and Afghanistan.

The main object of Mr. Lentz in his travels was to determine the geographical position and elevation of the places, which he visited; to investigate the three elements of terrestrial magnetism (declination, inclination and force of tension); to ascertain the heights of the mountains; and finally to make meteorological observations.

The expedition arrived at Astrabad, in the province of the same name, early in April 1858. As Mr. Khanikoff, to whom the direction of this expedition was confided, had gone to Teheran, Mr. Lentz employed his time, most profitably, during the absence of the former, in observing the movement of the chronometers and in ascertaining the absolute longitude of the city of Astrabad. He also determined at this place, in the same manner as at Ziared, the three magnetic elements, ascertained the height of some of the summits in the Albourz chain, and made meteorological observations.

Towards the middle of May, the expedition left Astrabad, and, having passed Albourz, stopped at Schakhroud. Mr. Lentz observed here, as everywhere in his travels, the movement of the chronometers, and determined again the three magnetic elements and the latitude of a great number of peaks in the Albourz chain.

On the first of June, when Mr. Khanikoff had returned, the expedition directed its course towards Meschel, the actual capital of Khorassan. During the whole route Mr. Lentz took great pains to determine the greatest possible number of geographical points and ascertain the elements of terrestrial magnetism. The five weeks of his sojourn at Mesched were spent in works of this kind, and he also made an excursion in the neighborhood in order to effect astronomical and barometrical observations.

The expedition left Mesched in the first part of August and reached Herat during the first days of September; having already determined a considerable number of geographical points, the position of which had been previously unknown.

In Khorassan, Mr. Lentz was chiefly occupied in determining with the greatest possible accuracy the absolute longitude of the most eastern point touched by the expedition. From Herat Mr. Lentz advanced to Tebbes, which is situated at the eastern limit of the salt desert of Khorassan, and then to Birdia-Sand, and succeeded in collecting many valuable additions to science.

In the midst of February 1859, the expedition left Khorassan and took its course towards Lasch, a fortified city and the capital of a little state of the same name. The observations made in this country are particularly interesting. They show that the terrestrial surface rises gradually from Herat to the passage Senhighé-Sia, near the city of Sabzor (Kingdom Hérat), where this elevation reaches a height of 5000 English feet; from this point the country gradually descends to a lake, the waters of which, however, have still an elevation of 1200 feet.

The expedition stopped, in its course, on the eastern shores of lake Zaré, near the place, where the river Kharoud or Adraskan empties into it. Mr. Lentz determined the absolute height and the geographical position of this point. The appearance and the dimensions of the lake change continually, sometimes the northern portion of it is dried up, and only its southern part is seen; sometimes the reverse takes place, according to the quantity of water, furnished by the three principal rivers, which empty into it, viz.: the Kharoud and the Ferraroud in the north, and the Khilmend, which flows not far from Kandahar, in the south. Sometimes it happens, that the waters of the lake divide into two parts, one towards the north, and the other towards the south, and are kept separated by a band of completely dry land as was the case at the time of the expedition. This at least is the ac-

count given by some Persian travellers, who had recently left the eastern shore of the lake and declared that they had crossed it by following this tongue of land.

Beyond the village of Nekh, the expedition came to a desert of 250 versts in extent. This they crossed in its narrowest part between the villages of Serri Tschakh and Dekhi-Seïf, at a place where in a length of 200 versts no trace of water was found. After a journey of four days they reached Kirman.

This place might be considered as entirely unknown up to this time. It appears from barometrical observations made there, that from lake Zaré (1200 feet high) the surface of the earth rises again up to the villages of Nekh and Serri-Tschakh (4000 and 5000 feet high), when the country gradually descends to its lowest point (900 feet high) in the desert at a place called Schakhri-Lut, but rises again as far as Khubbis, which is situated at the foot of the mountains, at an elevation of 1500 feet, and reaches its maximum (8000 to 9000 feet) at the top of these mountains, then it falls again towards Kirman, which however is still found to be 5500 feet high.

Over the whole area, which extends from Esd to Ispahan, (the point toward which the expedition directed its course,) the countries which border south and east upon the great salt desert, are 3000 to 4000 feet above the level of the sea; the same is the case with those countries which separate Schakroud from Mesched along the northern side of the desert; only in two places more considerable heights are found.

From Ispahan the expedition passed through Zerghendé, 10 versts from Tehran, went beyond the village of Firouska, reached the provinces of Mazanderan and Astrabad, and followed the course of the river Talar, which empties into the Caspian Sea.

Mr. Lentz determined, during his travels in Persia and Afghanistan, about two hundred geographical points; at twenty-eight points he could determine the three magnetic elements (intensity, declination and inclination); at twenty-nine other points, however, he observed only two of them. He ascertained with the aid of the trigonometrical calculus, the heights of about two hundred mountain summits, and about four hundred others were measured by him, and his travelling companions, with the aid of the barometer. Thus Mr. Lentz traced a profile of the whole country, which the expedition visited. Mr. Lentz gives also some interesting results of his meteorological observations, which he never neglected during the whole voyage. The barometrical observations show, with an incredible regularity, the slightest variations of the atmospheric pressure; the variations of temperature are also very regular, and it is interesting to state, that the temperature reached constantly its maximum about four hours after noon, and not two hours, as it is generally the case in our climate.

Throughout Khorassan the air is usually very little charged with vapors and its average hygrometrical state varies between 20 and 30 per cent. At Schakhroud, Mr. Lentz found only 17 per cent of moisture in the atmosphere, and in the desert near Kirman, only 14 per cent."

SCHLAGINTWEIT'S ETHNOGRAPHICAL COLLECTIONS.—Mr. Joh. Ambr. Barth, of Leipsic, has offered for sale a large collection of plaster-casts taken from the heads, hands and feet of individuals in the different castes and tribes of India, and has published a carefully prepared catalogue of the series. We make the following extract from his announcement:

"Messrs. Hermann, Adolphe and Robert de Schlagintweit, the enterprising travelers in India and High-Asia, having, since the year 1854, had charge of a scientific mission from the India House, have been enabled during their travels, in addition to their researches in physical geography and geology, to devote much of their time to ethnology.

"The various countries through which they passed, some of which have hitherto been but little explored, and others never reached by Europeans, afforded peculiarly advantageous opportunities for pursuing their ethnological researches.

"Besides measurements and photographs, they, in collecting their materials, made also casts of the features of living persons taken in plaster of Paris; 275 casts of faces were thus made, and 37 of hands and feet.

"The moulds have been reproduced by galvanoplastic deposits of copper, which gives without the least contraction the most minute irregularities of the skin with great perfection. This first series however was found not sufficiently strong, and the attempts which have been made to produce the heads in as great perfection as possible, have led to a different method, consisting in making strong metallic casts of zinc the basis, coated with a galvanoplastic deposit of copper, varied in color according to the different degrees of color of the native tribes. To exclude as perfectly as possible the change of the tints by the gradual oxydation of the copper, a thin stratum of colorless varnish has been put most carefully over the heads.

"Together with these casts, measurements of the various proportions of the skull and the body have been taken, which will be spoken of in detail in the work, which Messrs. de Schlagintweit are about to publish under the auspices of the President and Council, now at the head of Indian affairs, who having the same interest in science as the Court which preceded them, have found this collection an object of particular importance.

"This work, 'Results of a scientific Mission to India and High Asia,' is independent of this collection: it is in progress of being printed and is published by F. A. Brockhaus, publisher at Leipzig.

"The ethnographical part of Messrs. de Schlagintweits' work will chiefly treat of characteristic features obtained by well defined measurements, constantly referring to the casts made. Many other individuals, together nearly 400 persons, have been carefully measured, and the zealous labors of other distinguished, particularly Indian, ethnologists, amongst whom we name Buist, Carus, Cunningham, Davis, Walter Elliot, Falconer, Hodgson, Hooker, Humboldt, Morton, Latham, Owen, Rawlinson, the Stracheys, Sykes, &c., will be found to have been carefully studied for the purposes of scientific generalization.

"We scarcely need add how important objects these facial casts will be for all those who take an interest in such researches, while the interesting nature of the objects themselves, as well as the careful and novel mode of their execution, will render them a most beautiful and important addition to public and private museums.

"This collection has met with great approbation; we mention as particularly important the well known personal interest of the late Baron v. Humboldt."

ADOLPHE SCHLAGINTWEIT'S DEATH IN TURKISTAN.—Our readers are already acquainted with the fact that one of the bold brothers, whose expedition to the Himalayas has attracted the attention of the whole scientific world, fell a victim to his energy. From the surviving brothers, Hermann and Robert, we have received a printed document in which are given all the official reports which have yet reached them in respect to the fate of Adolphe. The conclusions at which they have arrived are thus concisely stated.

"The information from India and Russia, collected from natives by European officers of the adjoining districts, concur but too accurately in establishing the fact, that Adolphe Schlagintweit was killed at Káshgar in Turkistán (Central Asia) in August, 1857, falling a victim to his scientific mission.

"He was recognized as a European after having passed the Karakorúm and Küenlüen, in disguise, where before us no European had ever travelled; he had taken a route more westerly than ours, and had succeeded in penetrating far into Central Asia.

"The reports which have reached us are so various, that they do not of course all agree, as to the immediate cause and particulars of his death; yet it is evident from all of them, that the political condition of these countries, and the circumstance of the deceased being recognized as an officer of the Indian Government, notwithstanding every precaution, essentially contributed to his tragic end. Even with the lively sympathy ever so energetically evinced by England, in the fate of scientific travelers, it will scarcely be possible to succeed in bringing the murderers of our brother to account.

"According to some reports he perished in consequence of having taken up the cause of some captive Bhot-Rajpúts, British subjects, interceding for them, that they might not be executed or sold as slaves. Other accounts state the immediate cause of his death was, his having been recognized as a European, and fallen by the hand of fanatic Mussălmáns.

"Notwithstanding our most zealous exertions for some months past, in endeavoring to obtain his manuscripts, drawings, etc., we have not yet been successful in learning anything definite about them: still, however, many very important geographical communications have been made to us by his followers, and we are not without hope that from the active sympathy which the Indian Government has always displayed in our scientific mission to India and High Asia, nothing will remain untried that can tend to the rescue of his last papers."

LETTER FROM DR. LIVINGSTONE.—At the January meeting of the American Geographical and Statistical Society of New York, the following letter from Dr. Livingstone, the celebrated explorer in South Africa, was read by D. W. Fiske, Esq., Librarian of the Society:

Tette Zambesie, Feb. 22, 1859.

"*My Dear Sir*—Having been elected a member of your Society, I take the liberty to send you a short account of our attempt to open the interior of Africa, in the hope that, though it may not appear interesting to your members, it may, at least show my good will and desire to perform a corresponding member's duty.

We entered the delta of the Zambese in May, 1858, taking the most southerly branch we could find, but after ascending about seventy miles we found it impossible to enter the Zambese by that, as the points of junction were filled up with reeds and other aquatic plants. You may have a clearer idea of the region if you bear in mind the fact that the Zambese has in the course of ages formed a delta, which juts out into the ocean, and forms the most prominent part of the coast. The prevailing winds of these quarters beat, almost constantly, against the head of the promontory. These, aided by the oceanic currents, have helped to dam up the main stream, but the pent-up waters have escaped sideways. The main stream called Gualeo enters at the point of the promontory most exposed. To it we went after leaving the southern branch, but saw no possibility of entrance during three days, though her Majesty's ship Lynx has since found a channel in it, after a search of ten days. We then proceeded to examine the side branches and the very first we came to called Kongone, was all we required. There are other good ports, but all in the side branches. There are also communicating branches between these, and those within the influence of the tides are generally deep.

Having got into the main stream, we found that we had, in going to it and spending a month there, allowed the water to fall considerably. It was, also, so very much lower than usual that the Portuguese prophesied that we could not ascend ten miles. It was said, also, that war was raging, and no one would be allowed to go up, even if he could. Our ship drew nine feet seven inches, and she was under engagement to go to Ceylon. We, therefore, to avoid detention in the river, sent her off, and went up to the seat of war in a small steamer, drawing two feet six inches. We had no difficulty with the "rebels," as they were called—indeed, we got pilots from them, and continued ever after on the best of terms with the Portuguese. They were called "rebels," as they had all been runaway slaves, and bore the marks, in brands on their chests, of their former servitude. Slaveholders here must be civil, for it is so easy to run away, that if slaves go to the Landius, who are of the Zulu family, they never deliver them up. I have never heard of but one case to the contrary, and the owner—a great favorite of theirs—was obliged to give them his full value. This is a digression, but I may finish off by saying that the Portuguese governor attacked the rebels, and they retired before him, there being plenty of iron for all parties.

We continue carrying on luggage up the river till November, when it reaches its lowest point; and with care a flat-bottomed boat would do business even then. We know it now at its very worst, and, as it spreads out to from one to three miles in breadth, it is in many of the crossings not more than two and a half or three feet. Just now the water stands twelve feet above low water mark in November, and we are all quite sure that during at least eight months in each year a steamer of four or five feet could trade without embarrassment. The reason why so little has been known about the Zambese river, has been the branching in the stormy promontory by which it was hidden from navigators. And their easy chair geographers, dreaming over the geography of Ptolemy, actually put down the Zambese as flowing into the sea at Quilimane, which, in his days, it probably did, though not a drop of Zambese water, in ordinary circumstances, reaches that part. Had some branch of the Anglo-American family planted their footsteps on its banks, we are such a babbling newspaper set, the world would have known all about it long ago; and no one would have ventured to play with this river as has been done, making it lose itself and flow under the Kalobaro desert. You may form a better idea of its size if I tell you of one of the branches. We ascended the Shire lately, fully a hundred miles from the confluence, and found it with a two fathom channel all the way up. It varied from 80 to 150 yards in width, and contains no sand banks. It flows in a

beautiful fertile valley, about twenty miles high, and fringed with mountains of great beauty, well wooded to the top. Mora M. Vala we ascended, and found it 400 feet high. (This, by mistake, is placed on the wrong side of the Shire, in my map.) It was well cultivated on the top, and had several fine little fountains, the waters of which were slightly chalybeate; they have a hot sulphurous fountain at the base, (temperature 174° Fahrenheit). The people had many sweet potatoes, holcus sorghum, and other grains, and pine apples, lemon and orange trees. They were very hospitable, and independent. The vegetation is very different from the plains, and so is the climate; yet with all these disadvantages, no use has been made of it as a sanitorium by the Portuguese, and as far as we can ascertain, this river has never been explored by Europeans before. One part of the luxuriant valley of the Shire is marshy, and abounded in lagoons, in which grow great quantities of the lotus plant. The people were busy collecting the tubers, which, when boiled or roasted, resemble chestnuts. They are thus Lotophagi, such as are mentioned by Herodotus. Another part of the valley abounds in elephants. My companions estimated the numbers we saw at eight hundred. Herd upon herd appeared as far as the eye could reach; and noble animals they were. We sometimes chased them in our little steamer, for the shore branches off occasionally and forms islands. The upper part of the valley is well peopled, and many of the hills are cultivated high up. But never having seen Europeans before, they looked on us with great suspicion. They watched us constantly, well armed with bows and poisoned arrows, ready to repel any attack, but no incivility was offered when we landed, nor were our wooding party molested. We obtained what may be considered reliable information that the Shire actually does flow out of Lake Nyanga. We were brought up by a cataract, but five days beyond this point the water is smooth again, and Arabs come down in canoes from Nyanga thither. Seeing the suspicions we had aroused, we deemed it unsafe to leave the vessel and go overland. But no collision took place. The greatest coward fires first, so, thinking we had as much pluck as they, we did not lift a gun, though we were ready to fire, or rather shoot. We did nothing to make us ashamed to return, and mean to do so next month; and if we have their confidence we may go farther. They had abundance of provisions, and sold them at a cheap rate. Also cotton of two kinds—one indigenous, short in the staple but very strong, and woolly to the feeling; the other very fine, and long in the staple. We brought a number of specimens of their spindles and yarn, and it was quite equal to American uplands; did not offer them any American seed. The cotton plant is met with everywhere, and though burned down annually springs up again as fresh and strong as

ever. They grow sugar cane too, bananas, &c. The men are said by the Portuguese to be very intelligent, but very mild. The women wear the lip ornament, round one of which I put my pen. The slit is made in the upper lip, at first, by a ring in childhood. The ends are gradually pressed closer together, and cause absorption till a hole is made. This is enlarged by bits of reed, until in a lady of fashion a ring, either hollow or cup shaped, is inserted, and the edge of the lip protrudes beyond the perpendicular of the nose at least an inch. I am thus particular in case our own ladies, who show a noble perseverance when fashion dictates, may wish to adopt lip ornaments.

Above this we have a rapid, called Kebra, or rather Kaora-basa. When the water is low it shows a deep grove, with perpendicular sides. When steaming up this the man at the lead kept calling "no bottom" at ten fathoms, and the top of the walls of the grove towered from 50 to 80 feet above our deck. It is from 60 to 80 yards wide, but at this season is comparatively smooth. There were some cataracts in it which high water obliterates. This steamer is too weak to ascend. She being only ten horse power, and her plates $1\frac{1}{16}$th of an inch thick, we dare not try her in the rapids. We shall work down here some time yet. I long to lead back my faithful Mackalolo, who are still at Telle, though thirty of them died of the small pox, and six were killed by a neighboring chief.

I shall refer to one point more before concluding. We were warned by the fate of the Niger expedition not to delay among the mangrove swamps of the Delta—the very hot beds of the fever. We accordingly made all haste to get away, and we took daily a quantity of quinine. The period of the year I selected, though not the most favorable for navigation, was the most so for health; and, thank God, our precautions were successful.

The Kroomen from Sierra Leone have had more of it than we, until a short time ago, when, it being the most unhealthy season of the year, and even to the natives, three of us have had touches of the complaint, but are all now quite well. I have never had a day's illness since my return. We find, too, that so far from Europeans being unable to work in a hot climate, it is the want of work that kills them. The Portuguese all know that so long as they are moving about they enjoy good health, but let them settle down and smoke all day, and drink brandy, then—not a word about brandy in the fever that follows—the blame is all put on the climate. I am, &c. DAVID LIVINGSTONE.

KRAPF'S RESIDENCE AND TRAVELS IN EASTERN AFRICA.—Messrs. Trübner & Co. of London announce as nearly ready for publication a work which is likely to rival in interest the recent volume of Dr. Livingstone. We refer to the narrative of a Missionary Residence in Abyssinia by Dr. J. L. Krapf—one of

the agents of the Church Missionary Society of London. It will be recalled by our readers that it was by him and his comrade Rebmann that intelligence was first given to the civilized world of the possible existence of snow-covered mountains near the equator—the famous Kilimandjaro. The observations of these missionaries have given rise to many warm discussions, and the correctness of their opinion has been earnestly disputed. In the forthcoming volume we may anticipate that this controverted point will be examined with thoroughness and detail. Aside from this discussion, the work of Dr. Krapf will abound in interesting comments upon his missionary life. His land journeys, which were mostly upon foot, extended 9000 miles. We quote the following from the prospectus of this work.

"Two things may be said of Dr. Krapf which can be affirmed of no other modern African traveler. He has traversed Abyssinia from north to south and from east to west; and further, he has explored the whole coast of Eastern Africa, from Suez to the 10th degree of south latitude, and inspected every place of importance to be found on it. Such journeys and voyages would alone bestow a high value on a volume like the present one, which communicates their most important results. But more than this, the large and interesting country which stretches from the Equator to the 5th degree of south latitude was, from the eastern coast inwards, all but a *terra incognita*, until it was traversed, on foot, by Dr. Krapf, and by his colleague and fellow-worker, the Missionary Rebmann, whose experiences are also included in this work. From the Mission-station at Rabbia Mpia, on the coast, these brave and fearless men prosecuted journeys for at least three hundred miles into the interior, exposed to every possible peril and privation. These journeys were repeated by different routes—the dangers incurred on one seeming only to stimulate to self-exposure to greater dangers on another. Rebmann's three journeys to Dschagga, Krapf's two journeys to Usambara, and two more to Ukambani, in the course of which they explored regions and visited—Bible in hand and Gospel on lip—populations never before seen by European, have rarely been exceeded in interest—religious, adventurous, and geographical. The story of Dr. Krapf's abandonment and wanderings in the wilderness, during his second journey to Ukambani, carries the reader back to the old days of adventurous travel. Scarcely in the whole annals of modern missionary effort has there been anything equal to the spectacle displayed in this section of the volume, of two individuals, each isolated, pursuing again and again, on foot, without external encouragement of any kind, and in the face of every possible obstacle, journeys among ignorant and savage heathen, far away from help, or the hope of help, and confiding solely in the guidance and support of Providence. The

splendid geographical and ethnological results which were among the rewards of these daring pilgrimages will be found fully chronicled for the first time in the present volume."

SPEKE'S EXPLORATIONS IN EASTERN AFRICA.—At a recent meeting of the Royal Geographical Society of London, Capt. Burton and Capt. Speke both gave a narrative of their explorations in Eastern Africa, which are of particular importance, as our readers are well aware, in connection with the long disputed problem of the sources of the Nile. So much interest has been manifested everywhere in this expedition that we regret that our limits will not permit us to reprint entire the discussion to which these two papers gave rise in that learned association. Sir R. I. Murchison, Col. Sykes, Mr. Macqueen, Mr. Galton, and other well known gentlemen presented their views upon this important topic, a report of which will be found in the Society's Proceedings, vol. iii, No. 6. From the same source we extract the following statement of the remarks of Capt. Speke.

"The region traversed by Captain Burton and myself is divisible into five bands. They all run parallel to the coast, and each of them is characterised by special geographical features. The first is low land between the coast range and the sea. Its breadth is about 120 miles, and its average slope not more than 2 feet per mile. Forests of gigantic trees, and tall grasses, cover its surface. The second band is the coast range of mountains. These are hills in lines and in masses, intersected by valleys, through which the rivers of the east coast find their way. This range is easily crossed, and nowhere exceeded 6000 feet, adjacent to the line of road taken by our travelers. It is capable of cultivation, though neglected, because the slaving forays to which it is subjected drive away the inhabitants. The third band reaches to Unyanyembe. It is a dry plateau, with a slight inclination toward the interior, and ranging in height between 3000 and 4400 feet. Tributary streams, running southwards to the Ruaha, intersect it. The fourth zone is a continuation of the above, but it is better watered, and is studded with granite hills. Here is the water-parting between the streams that run eastward to the Indian Ocean, and westward to the Tanganyika Lake. The Nyanza Lake is situated in this band. The fifth band is a remarkable slope, that inclines to the shores of the Tanganyika. It sinks no less than 1800 feet in 45 miles; it is exceedingly fertile, but harrassed by marauders of the Watuta tribe.

On arriving at Ujija, the party found that the only boats to be had were wretched canoes; while the troubled state of the country rendered it unsafe to explore the lake unaccompanied by a large escort. There was, however, a small sailing craft belonging to an Arab, on the other side of the lake, which would be large enough to contain the entire party; and Captain Speke started

to hire her, with seventeen savages, as a crew, and four of his own men. He first coasted to Kabogo, a bold promontory usually selected as the starting point, when the lake has to be crossed, and reached it in five days. He describes the shore as wild and beautiful, affording many convenient harbors, and requiring but a little art to make it quite a fairy abode. There were no inhabitants, but an abundance of game,—hippopotami, buffaloes, elephants, antelopes, and crocodiles. The passage across the lake, a distance of 26 miles, was made rapidly and safely, and Captain Speke was cordially welcomed by the Sultan of the country on the opposite side. The owner of the sailing boat was there also, and was ready to afford every assistance; but he himself was on the point of starting on an ivory expedition 100 miles into the interior, and the crew of his sailing boat were, at the same time, his armed escort: he could not therefore spare them. What made the disappointment doubly vexatious, was that this Arab desired Captain Speke's companionship in his intended journey, and he promised the boat on his return. Had Captain Speke been unfettered by time, this would have been an excellent opportunity of farther travel. As it was, he was obliged to go back to Ujiji without the sailing boat, and proceeded with Captain Burton to a more extended exploration of the Tanganyika Lake, which lasted a whole month. The mapping of its southern portion depends on information given by this Arab.

On returning to Unyanyembe, Captain Burton's continued illness again made it necessary for Captain Speke to proceed alone to the northward to explore the Lake Nyanza. He went with thirty-three men, through a line of populous country, less visited by strangers than that which he had hitherto traveled on. There were numerous petty sovereigns who were hospitable enough but very troublesome. The view of Lake Nyanza, with its numerous islands, reminded Captain Speke of the Greek archipelago. The islands were precisely like the tops of the same hills that studded the plains he had just traveled over. In fact, the lake had the features of a flooded country rather than those of a sheet of permanent water, with well marked banks. Its water is sweet and good: those who live near it drink no other.

Captain Speke's explorations did not extend beyond its southern shores. The more northern part of his map is based on native information, especially on that of a very intelligent Arab, whom he has previously met with in Unyanyembe, and whose data, so far as the shores of the lake, were found by Captain Speke to be remarkably correct. This Arab had traveled far along its western shores. In thirty-five long marches he reached the Kitangura river, and in twenty more marches, Kibuga, the capital of a native despot. Between these two places he crossed

about 180 rivers, of which the Kitangura and the Katanga were the largest. The former is crossed in large canoes; the latter, though much larger and broader, is crossed during the dry season by walking over lily leaves; but in the wet season it spreads out to an enormous size, and is quite unmanageable. The rainy season is very severe in these parts. No merchants have gone farther than Kibuga; but, at that place, they hear reports of a large and distant river, the Kivira, upon the banks of which the Bari people live. This river is believed by Captain Speke to be the White Nile."

Sir R. I. Murchison in reviewing the labors of the two explorers, remarked that "they have, by means of astronomical observations, fixed the position, the longitude and latitude of these two great lakes, and have shown you that whilst one is like other lakes, of which we had previously heard, situated on a great plateau, the other is situated at such an elevation that, as Captain Speke has explained to you, it may very possibly be found to feed the chief sources of the Nile. I will not now argue that difficult question, because I am quite sure there is one gentleman here, if not others, who may dispute that inference. I will, therefore, first call attention generally to the great importance of these discoveries. My friends here have not only traversed the district and furnished us with a good picture of the manners and customs of the inhabitants, but have also brought home rock specimens which enlighten us as to the fundamental features of this country; and to these rocks I will for a moment advert. Captain Burton placed before me this morning certain specimens which show me that at an elevation of upwards of 3000 feet above the sea and towards the interior there are fossilized land shells, showing that from very ancient periods the lands have maintained their present configuration. These deposits, whether purely terrestrial or lacustrine, have been consolidated into stone, and show that the existing internal condition of Africa is that of ages long gone by, as I took the liberty of pointing out to the society some years ago, when treating of Livingstone's first explorations. Another striking feature in connection with this great zone of country is this. You will observe that our friends spoke of remarkable herds of oxen on the banks of the lake Tanganyika, and tribes of people between that vast lake and the coast range, who are a thriving, peaceful, agricultural population, whilst the adjacent districts in the north and south are frequently disturbed by wars for slave-hunting purposes. This is a great fact as indicating a broad line of route by which we may hope hereafter to establish intercourse with the interior country. There is another important fact, though I do not think Captain Speke alluded to it, namely, the absence of that great scourge of parts of southern Africa, the *Tsetse* fly.

With regard to the physical geography of the country, it is remarkable that all the adjacent rivers fall into the great Tanganyika lake, which was formerly supposed, on the contrary, to afford the sources of the Zambesi river. All theory, therefore, on this subject is now set at rest. Lastly, we come to the subject which is likely, as I said, to give rise to much discussion, and that is the theory upon which I think my friend Captain Speke may rest his claim to our most decided approbation. On my own part I am disposed to think that he has indicated the true southernmost source of the Nile. Now, in saying this I do not mean to deny that the great mountains flanking the lake on the *east*, of which a point or two only is marked on the map before us, do not afford the streams which flow into this great lake. That must probably be the case on the east, just as Captain Speke ascertained from the Arabs that the so-called "Mountains of the Moon" feed the same lake from the west by other streams. You must here recollect that the same Arab sheik who gave him the information which turned out to be correct concerning the existence of the lake Tanganyika also told him of the existence of the Nyanza, which lake was found to be exactly in the position indicated. As Captain Speke has determined that this great lake Nyanza is nearly 4000 feet above the sea, it may well, indeed, be the main source of the White Nile. Everything (as far as theory goes) being in its favor, this view is farther supported when we reflect on the fact that the tropical rains cause these upland lakes and rivers to swell and burst their banks at a period which tallies very well with the rise of the Nile at Cairo. These, then, are grounds which I think must go to strengthen the belief of Captain Speke, and I may, therefore, repeat what I stated at the anniversary, that highly worthy as Captain Burton was to receive a gold medal, not only on account of this great expedition which he led, but also for his former gallant and distinguished expeditions, Captain Speke, who now sits at your Lordship's left hand, is also entitled to a gold medal of the Royal Geographical Society."

H. SCHLAGINTWEIT ON THE SALT LAKES OF THE HIMALAYAS.—At a recent meeting of the Royal Geographical Society of London, Mr. H. Schlagintweit exhibited some chromo-lithographic sketches of the Himalayan Mts., and in commenting upon the remarkable erosion which takes place upon that range, he spoke as follows of the salt-lakes which form a peculiarity of that region:

"Another consequence of the erosion is the gradual drainage of fresh water lakes, or their conversion into salt water lakes. It is very characteristic for the Himalayas, and in this respect they differ essentially from most other mountain systems in the world, that hardly any fresh-water lakes now occur. The only

few lakes of any considerable extent which have been made known by Captain Strachey, Captain Speke, and Major Cunningham, as well as those we visited besides, are all salt water. But the explanation we think we must give of this phenomenon is different from the explanation formerly given. Some have thought that a raising of the country might have caused a general drainage. We think that supposition rather improbable, from the recent strata round these salt lakes being all horizontal, and the outlets of these salt lakes being in a different direction in reference to the horizon. If any raising of the country had effected the drainage of the salt lakes, the effect would have been a perfectly different one, according to the position the outlet of these lakes had in reference to the points of the horizon, a modification which is nowhere met with.

"The Tso mo Ri ri and the Tso mo Gnalari, the two great salt lakes of Rupchu and Pankong, of which drawings are presented, happen to be a good example of two large lakes, being about equally salt, with differently directed former outlets, and with quite horizontal banks of detritus and of watermarks along their circumferences. The gradual progress of the erosion of the valleys seems to us to be also the chief cause of the gradual transformation of freshwater lakes into saltwater lakes in Tibet.

"By this progressive excavation thousands of square miles, still marked as former lakes by the form of the surface, have been emptied, and the consequence is that the local evaporation could no more keep the equilibrium with the precipitation; in consequence the lakes, of which parts remained undrained on account of their greater depth, now gradually became more and more salt."

JOURNAL OF THE ROYAL GEOGRAPHICAL SOCIETY OF LONDON.—We have just received the twenty-eighth volume of the Journal of the Royal Geographical Society of London. Like the previous parts of this series it is full of important contributions to our knowledge of the physical geography of every country where British enterprize is manifested. We have heretofore quoted from the anniversary address of the President, Sir R. I. Murchison. To many of the other articles we shall have occasion to allude. The following is a statement of the contents of the volume:

Articles—1. Journal of the North Australian Exploring Expedition; under the command of Augustus C. Gregory, Esq. (Gold Medallist, R.G.S.); with Report by Mr. Elsey on the health of the party.—2. Notes on the Physical Geography of Northwest Australia; by Mr. James S. Wilson, Geologist to the North Australian Expedition.—3. Journey from Colesberg to Steinkopf in 1854–5; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—4. Journey from Little Namaqualand eastward, along the Orange River, the Northern Frontier of the Colony, &c.,

&c., in August 1856; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—5. A Coasting Voyage from Mombasa to the Pangani River: Visit to Sultan Kimwere: and Progress of the Expedition into the Interior; by Captains Richard F. Burton, commanding the East African Expedition, and J. H. Speke, F.R.G.S.—6. Explorations in the Desert East of the Haurán, and in the Ancient Land of Bashan; by Cyril C. Graham, Esq., F.R.G.S., &c.—7. Contributions to the Knowledge of New Guinea; by Dr. Salomon Müller,—8. On the supposed discovery, by Dr. E. K. Kane, U.S.N., of the North Coast of Greenland, and of an Open Polar Sea, &c., as described in the 'Arctic Explorations in the years 1853, 1354, 1855;' by Dr. Henry Rink, M.D., Inspector in Greenland for the Danish Government.—9. The Yang-tse-Keang, and the Hwang-Ho or Yellow River; by William Lockhart, Esq., F.G.R.S.—10. Extracts from a Journal kept during a Reconnaissance Survey of the Southern Districts of the Provinces of Otago, New Zealand; by J. Turnbull Thomson, F.R.G.S., Chief Surveyor.—11. Observations relative to the Geographical Position of the West Coast of South America; by Carlos Moesta, Director of the National Observatory, Santiago de Chile, May 29, 1856.—12. Excursion made from Quito to the River Napo, January to May, 1857; by Dr. William Jameson.—13. Description of the State of San Salvador, Central America; communicated by John Power, Esq., F.R.G.S., of Panama.—14. On the Latitude and Longitude of some of the principal places in the Republic of Guatemala; by A. van de Gehuchte.—15. On the Fine Regions of the Trade Winds; by Thomas Hopkins, M.B.M.S., Vice-President of the Manchester Literary and Philosophical Society.—16. Remarks upon the Amount of Light experienced in high Northern Latitudes during the absence of the Sun; by Captain Sherard Osborn, R.N., C.B., F.R.G.S., Officier Légion d'Honneur, &c.—17. Notes on the River Amúr and the adjacent Districts; by MM. Peschurof, Permikin, Shenurin, Vasilief, Radde, Usoltzof, Pargachefshi, &c.

Illustrations.—1 and 2. Map to illustrate the Route of the North Australian Expedition, and Mr. Wilson's Paper on the Physical Geography of N.W. Australia.—3 and 4. Map to illustrate Mr. Moffat's Journey from Colesberg to Steinkopf; and from Little Namaqualand Eastward, along the Orange River.—5. Map to illustrate the Progress of the East Africa Expedition.—6. Map to illustrate Mr. Cyril Graham's Explorations East of the Haurán, &c.—7. Map to illustrate Dr. Rink's Paper on Dr. Kane's Arctic Explorations.—8. Map to illustrate Mr. Thomson's Survey of Otago.—9. Map to illustrate Capt. Sherard Osborn's Paper on Light in the Arctic Regions.—10. Map to illustrate Notes on the River Amúr.

Yale College Library, March, 1860.

ART. XXIII.—*On the Species of Calceola found in Tennessee: Calceola Americana;* by Prof. J. M. SAFFORD.

FOR many years it has been known that a species of *Calceola* occurs in the marly and glade-forming limestones of Western Tennessee. This species has been considered to be identical with the European *C. sandalina* of Lamarck, an error (for such I hold it to be) which has contributed much to the confusion that has existed with reference to the age of the limestones mentioned. Individuals of the species are frequently found upon marly glades of Decatur, Perry, Wayne, and Hardin counties. The identity of the species with *C. sandalina* (and a few other determinations of the same kind) once taken for granted, it was an easy matter to designate the rocks of these glades "Devonian."

Since my attention has been called particularly to this species, I have regarded it as distinct, and now propose for it the name *Calceola Americana.*

In the first place, its different geological position would, at least, indicate a distinct species. It is without doubt an Upper Silurian fossil, and moreover belongs exclusively, so far as my observations have extended, to the Niagara Period.* The grey marly limestones of *the glades*, although much alike lithologically, are generally easily separable, by their fossils, into two beds, the lower one representing, in part, the Niagara Period, and the other the Lower Helderberg. It is to the former of these that our *Calceola* belongs. Among its associates are *Orthis elegantula, Platyostoma Niagarensis, Caryocrinus ornatus, Eucalyptocrinus decorus*, &c. *Halysites escharoides* and *Cladopora reticulata* have been observed in a local coralline limestone resting upon the bed containing the *Calceola.*

In the second place, the characters which separate it from *C. sandalina* are well marked. In general form, it is much like the European species, but differs in the following particulars:†

1. In *C. sandalina* the central cardinal process or tooth of the large valve is divided longitudinally by a shallow linear groove, making the tooth apparently double; in all my large valves of *C. Americana* this tooth is not grooved, but, on the other hand, is rounded and smooth along its summit; it is moreover longer and larger than in the European species.

* The *Niagara Period*, as here used, is equivalent to the New York rocks from the Oneida Conglomerate to the Niagara Group inclusive.

† In making these comparisons I have before me forty specimens of the American species and seven of the European. Of the first, one is an excellent specimen with both valves united, two are good specimens of the small valve, and the remainder are large valves. The small valves are seldom found. I have seen altogether four of them. Of my European specimens, one is entire with both valves, another is an excellent small valve, and the rest are large valves all in good condition.

2. In my specimens of *C. Americana* the rows of punctures, so conspicuous on the internal surfaces of *C. sandalina*, are not seen.

3. Within the largest valve of our species, in the older individuals, there is adjoining the hinge line, and on each side of the cardinal process, (but separated from the latter by a deep groove,) a prominent callosity. In very old specimens, these callosities nearly fill up the back portion of the cavity of the shell, and, at the same time, nearly obliterate the striæ or ridges which run forward from the hinge line. Most of the inner surface of the large valve has an irregular wavy appearance, indicating a vesicular structure, which, in fact, the mass of the valve has.

4. The small valve (the dorsal), so far as I have seen, has externally no proper cardinal area; its *apex* is not immediately over the hinge line, but is removed about one-fourth of the length of the valve towards the front, the cardinal edge being bevelled off from the apex to the hinge line. The lines of growth are prominent along this bevelled edge; so they are too on the cardinal area of the large valve.

5. The external surface of *C. Americana* is obscurely marked longitudinally in front by striæ, which, so far as they have been seen, are coarser and less numerous than in *C. sandalina*.

There are other points of difference which appear to be constant, but those given are sufficient to characterize the species. Upon a future occasion the fossil will be illustrated by the proper figures.

Lebanon, Tenn., Feb. 1, 1860.

Art. XXIV.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—3d Article.

In the two preceding numbers of this Journal* we have given observations of the Aurora of Aug. 28th to Sept. 4th, from numerous places in North America. We now continue our record of the phenomena, and intend in a subsequent number to present a summary of the observations made in other parts of the world. We are indebted to Mr. Benj. V. Marsh, of Philadelphia, for a considerable number of the following notices.

1. *Observations at Montreal* (lat. 45° 31′), *by* Dr. Archibald Hall.

August 28th about $8^h\ 20^m$ P. M. the sky was about seven tenths obscured by massive cumuli, when in the interval between them I observed streamers of a ruddy tint passing from the south towards the zenith. The wind was N.N.W. and blowing

* Vols. xxviii, p. 385; xxix, p. 92.

rather stiffly. About 10 P. M. the streamers seemed to converge towards the zenith in all directions, and to possess a deep ruddy tint. There was a large cumulus cloud in the W.S.W. and from a clear space beneath it a streamer shot upwards and distinctly traversed the cloud, illuminating it vividly. The same phenomenon was witnessed by another observer at the other end of the city.

At $2^h\ 10^m$ A. M. Sept. 2d, a brilliant aurora was seen in the vacant space between masses of huge cumuli and lasted until $3^h\ 30^m$ A. M. The sky was at first of a bright coppery red tint, and the light emitted so great that it was possible to read moderately large print by it. This space became interspersed with streamers of a rich roseate hue stretching to the zenith. The manifestation was chiefly observed in the W.S.W.

Sept. 2, at $9^h\ 40^m$ P. M. we had another auroral display. The streamers were mostly white, springing from three well-defined arches, stretching between the N.E. and N.W. They flickered magnificently about $10^h\ 20^m$ P. M. in the zenith, where they formed a huge corona having a tent-like appearance. These displays have been the finest seen here for many years, and it is to be regretted that on the two first occasions, clouds should so far have concealed them from our view.

2. *Observations at Montreal* (lat. 45° 31′), *by* Prof. CHARLES SMALLWOOD, LL.D.

Aug. 28th at 9 P. M. we had a splendid aurora extending over nearly the whole horizon with the exception of a small space in the south and S.W., varying in color from a pale yellow to deep orange and violet or crimson, and nearly as light as when the moon is at its full. The aurora was first noticed between $8^h\ 30^m$ and 9^h P. M., and this appearance lasted, with modifications, till nearly sunrise.

On the following night, Aug. 29th, there was also a fine display, but not to be compared in brilliancy to that of the previous evening. The sky was on this occasion cloudless, and a few streamers were occasionally seen tinted with a pale violet color.

The most remarkable incident was the unusual amount of atmospheric electricity present. At 9 P. M., Aug. 28th, the electrometers indicated a maximum of 250 degrees in terms of Volta's electrometer No. 1, of a positive character (but almost constantly varying in intensity); an amount equalled only during the thunder storms of summer, and the heavy snow storms of winter. The amount during the following day and night indicated a maximum of ten degrees, which is however somewhat above the usual average.

The appearances would lead to the opinion that the clouds might have been the medium of conducting the atmospheric

electricity to the earth, for the indications of the electrometers were such as are observed during the passage of clouds charged with electricity, and this phenomenon seems to have extended to the wires of the electro-magnetic telegraph.

The following day and night indicated a small increase on the usual amount of electricity, which may be owing to the continued presence of the aurora, or in some measure to the decrease in temperature.

Similar indications of the electrical state of the atmosphere during the aurora were never observed here, although its effect on the magnetic telegraph has been before witnessed.

3. *Observations at St. Paschal* (lat. 47° 40′ N., long. 67° 40′ W.), *communicated by* Prof. C. SMALLWOOD.

It was about 10 P. M. Aug. 28th, that the aurora was first noticed here. It was a magnificent display which threw out streamers from the zenith all around the horizon, and the light was nearly that of the day. I believe it was visible at Lake St. John on the Saguenay, lat. 48° 8′, long. 71° 9′.

4. *Observations at Halifax* (lat. 44° 39′), *by* Lieut. N. HOME, *of the Royal Engineers.*

Aug. 28th at 5 P. M., I remarked a long narrow belt of cloud from E. to W. having a peculiar orange-white appearance.

At 8 P. M. I observed this cloud (which in the interim appeared to be stationary) suddenly to become luminous, particularly at its eastern extremity. This cloud was about 10° wide, and appeared to extend from horizon to horizon; no other clouds were visible.

Soon after 8 P. M. two arcs of light N. and S. appeared, that to the south being the brightest. Under both these arcs the heavens were dark; but observers were uncertain as to whether the darkness was cloud or not. No stars were seen below the arcs, although quite visible above them.

At 9¼ P. M. the appearance was as if these two arcs were a small circle of the sphere, dipping to the south at an angle (measured by sextant) of 15° to the horizon, and 12° above it. The corona being formed at a point (by sextant) 10° south of zenith. There was only one band or arc of light, and that was continuous around the whole heavens. There were two remarkable patches, one due west, at an elevation of about 36°, having a red color; and the other east by north, at an elevation of 25°, having an orange color. These points were brightest during the whole display.

Two distinct sets of streamers appeared to be formed; one set from the arc of light, the other from the corona, which seemed to be constant or nearly so; as during the five hours I watched

the aurora, there appeared to be always light in or near the zenith, and always in the arc. The streamers were the variables, and appeared to work from W. by N. to south. I think they worked along from E. to W., but another observer said from W. to E. To the south they were so vivid and rapid it was not easy to tell.

A volume of light, as if a quantity of burning spirit was poured over the heavens, appeared to stream across from north to south quite independent of the streamers. The corona sent down rays, but it seemed to be only half way; the streamers from the arc meeting them and toothing in, appearing to alternate, short and long ones.

5. *Observations at Grafton, Canada West* (lat. 44° 3′ N., long. 78° 5′), *by* JAMES HUBBERT.

Aug. 28th at $8^h\ 30^m$ P. M. my attention was attracted by the peculiar appearance of the southwestern sky. Streamers and flashes of light of a pale yellow and red color were rising, sailing towards a point 8° south of the zenith, and meeting others from the N.W. and north. By $8^h\ 53^m$ the whole northern and eastern sky was a blaze of lurid light, which seemed most dense in a band seven degrees wide, extending from N.W. to S.E., along which there was a constant succession of streamers and nebulous patches, exhibiting every shade of white, yellow and red. Columns were now darting up from all parts of the horizon. The aurora hung along the south, in a line at a maximum height of 17°. This from $8^h\ 50^m$ to 9^h was very perfect; while a similar arch but much less regular was formed in the north, reaching to the east. The latter had an altitude of 27°, and like the other seemed to rest on a dark bank. The first corona that I observed was formed at 9^h, at an altitude of 70°. It was imperfect and vanished almost instantly; but was soon replaced by another in nearly the same spot. This in turn gave place to another still more complete. From $9^h\ 15^m$ to $10^h\ 15^m$ the drapery was gorgeous in the highest degree. A diffused light made surrounding objects very distinct. Cocks crew, and the animal world seemed to think that day was dawning.

I noted constant changes which were little more than a repetition of the above till 3^h in the morning. The corona was distinct from $12^h\ 37^m$ to $1^h\ 5^m$. The color was white, merging into every shade of yellow, crimson, scarlet, purple, and sometimes tinged with green. I listened with great earnestness, and once or twice thought I heard a rustling noise, but I think it must have been the wind. When the wind was hushed, as it was at intervals in the latter part of the night, not a sound could be heard. Just at 10^h the aurora, after nearly disappearing, became intensely brilliant, equalling the light of the moon at the

last quarter. The aurora continued till daylight, when it gradually faded away.

The evening of Aug. 29th was clear; and at $8^h\ 45^m$ the aurora was again visible, but very much less extended and brilliant than on the preceding night. There were thin, misty clouds of a nebulous appearance, with occasional streamers of a pale white light, sometimes merging into red.

On the night of Aug. 30th I observed no unusual appearance.

Aug. 31st the sky was covered with a dense mass of clouds; but the existence of the aurora was evident from the clearness of the night. After midnight the clouds disappeared, and the display was magnificent. All the characteristics of the night of the 28th were repeated; but the arch was rather lower along the southern horizon. A fiery bank was formed in the south, from which rays were constantly darting upward, and the whole sky was a gorgeous canopy of crimson and gold. This was most vivid from $1^h\ 15^m$ to $1^h\ 45^m$, but was continued till almost daylight.

Sept. 1st was cloudy, and I saw no indications of the aurora.

Sept. 2d there were dense clouds, yet the aurora might be occasionally seen. It was confined to the N. and N.E., and was particularly bright from $9^h\ 51^m$ to 11^h.

Sept. 3d was clear. At $8^h\ 50^m$ the aurora appeared in the N.N.E. and W. The light was yellow and white, with traces of crimson and green. At 10^h an imperfect corona was formed, but almost instantly disappeared. Others followed, but none of them were complete.

Sept. 4. The same phenomena were observed, but much diminished in brilliancy.

Sept. 5. No trace of the aurora was visible.

6. *Observations at Rochester, N.Y.* (lat. 43° 8′), *by* Prof. C. Dewey.

The aurora of Aug. 28th was exceedingly splendid both before and after midnight, with the corona a little south of the zenith; and exhibited many colors, with red or crimson predominant.

Sept. 1st. The aurora began late in the evening, and exhibited the usual appearances.

Sept. 2d at 1 A. M. it was cloudy, but very bright and red in the N.E.; the light increased rapidly and extended. At 2 A. M. there was a magnificent glow of red over the southeast, south and southwest; yellowish green, green and crimson, forming a gorgeous display quite down to the south horizon as seen from the housetops. A splendid corona was formed just south and east of the zenith, with splendid coruscations from towards the horizon up to the zenith. The streamers shot upward towards where the corona was formed, but none went to it.

Sept. 3d. The aurora was considerable at 9 P. M., and over the north, streamers were shooting upwards. At 10 P. M. there was a bright red space in the N.W. or W.N.W. with white and greenish-white bands on each side. The flashing of light upwards soon began, and the streams or clouds of aurora were splendid. At 10½ P. M. the corona began a little S.E. of the zenith, and was very splendid, towards which the streaming upwards was on all sides but less from the south. At 11 P. M. it nearly disappeared. This aurora was equal to that of November, 1837.

7. *Observations at Newburyport, Mass.* (lat. 42° 48′), *by* Dr. Henry C. Perkins.

The aurora of Aug. 28th was the most splendid ever witnessed at Newburyport by the present generation. About 7½ P. M. the eastern sky seemed to outvie the western, but with reversed colors, the pink of the morning taking the place of the golden hues of the setting sun. In a few moments these hues were repeated in the northeast and the west, and the yellowish-white luminous arch had passed the zenith and was fast covering the southern sky, and at 7¾ P. M. had enveloped Antares. At $7^h\ 52^m$ the star Tau Scorpii was at the southwestern edge of the luminous fringe. At 9 P. M. Lambda Scorpii marked its southern border. At 9¾ P. M. the northern border of a luminous arch passing from the east to the west, was marked by Nu in the right foot of the Swan, while the whole southern and southwestern heavens were glowing with streamers rushing to the pole of the Dipping-needle, the whole northern heavens being entirely destitute of the auroral light. At this juncture, in an instant as it were, the merry dancers sprang up from the northern heavens, and at 10 P. M. the whole celestial vault was glowing with streamers, crimson, yellow, and white, gathered into waving brilliant folds, a little to the south and east of the zenith, affording a canopy of the richest tints and most magnificent texture. The light was examined by the polariscope, and found not polarized. The stars were so lost amid the effulgence as to render it somewhat difficult to make out the constellations. Print might be read by the aid of a small lens, and the time ascertained from the watch by the simple light of the aurora.

During the evening of Sept. 1st the aurora was quite bright, and about a quarter to one (Sept. 2) it spread very rapidly, and soon enveloped the whole heavens. At about one the spectacle was magnificent, a perfect dome of alternate red and green streamers being formed, and the light being so great that ordinary print could be read as easily as in the day-time. It continued till morning.

8. *Observations at Lunenburg, Mass.* (lat. 42° 35′), *by* Prof. WILLIAM B. ROGERS.

The aurora of Aug. 28th has rarely been equalled in this latitude, and the meteor was repeated with more or less splendor for the eight following nights. The displays of Sept. 1st and 2d were scarcely inferior in beauty to that of the 28th, while that of Sept. 2d, in some of its features, was the most interesting of them all.

On the evening of Aug. 28th, throughout most of the northern half of the sky, the stars were dimmed by what seemed to be a luminous haze, which in some places quite eclipsed their light, and which itself glowed changefully with a golden and crimson coloring. In the earlier stage, the obscure space on the northern horizon had not assumed the usual arched form, and was sufficiently translucent to show a few flaky clouds, floating within its confines. At $8^h\ 20^m$ this dark space had become more opaque, and had moulded itself into a symmetrical arch, bounded by a broad luminous band.

At $9^h\ 30^m$ the display attained its highest magnificence. The dome of the heavens was hung around with white and golden and rose-tinted streamers converging from all quarters towards the magnetic pole. Over the glowing stripes of this marvellous pavilion there came broad flushes of the richest crimson light, until it suffused all the upper part of the sky, and the whole southern quarter except a narrow space next the horizon.

At $10^h\ 30^m$ nothing remained of this wonderful spectacle but a faint auroral arch low down in the north, accompanied by a few dim streamers.

The aurora recurred in great splendor between 1 and 2 A. M., Aug. 29th, when the crimson color was particularly remarkable. At $3^h\ 30^m$ A. M. there was a fine auroral arch in the north, with a long array of streamers rising from it.

Sept. 2d, a clear sunset was followed by a peculiar greenish and purplish light extending round the horizon, even beyond the north. Over the northeast quarter, the air to the height of 30° had a dark opacity, which had the effect of arresting the light coming from beyond.

At $7^h\ 30^m$ P. M. an irregular obscure space began to form along the northern horizon. At $7^h\ 50^m$ a faint arch of white light made its appearance, resting on the horizon a little north of the E. and W. points, and culminating some distance below the pole star. This continued to rise until 8 P. M., when its apex was within a few degrees of the pole.

At $9^h\ 20^m$ a low luminous segment showed itself on the horizon beneath the arch. The latter now resolved itself into an array of bright streamers, with equidistant shadowy spaces between them.

At 9h 30m the streamers had extended and grown brighter, while the low luminous segment, diffusing itself upward, had merged into the outer arch, which now reached nearly to the pole star. At this moment the arch began to send off successive waves of light, rapidly following one another towards and beyond the zenith. In a few seconds this wave movement gave place to more rapid and seemingly broken pulsations, flitting upwards in close succession through the northern, eastern and western quarters of the sky, and visible, though less distinctly, in the south. This wonderful appearance exhibited everywhere a convergency of the lines of motion towards a point considerably south of the zenith.

When these luminous phenomena were at their height, every spot to which the eye was directed, except the southern quarter near the horizon, was traversed by quickly successive flashes of white, greenish, and pale roseate light, all seemingly moving upwards.

At 10h 30m the pulsating movement again extended over all the northern and part of the southern half of the sky. Innumerable waves of white, yellowish and purplish light chased each other from every quarter towards the magnetic pole, while the crimson flush spread wider and higher from the west.

The various phases of this aurora recurred according to a somewhat uniform order of succession. First, the dark segment on the northern horizon took a regular arched form, and as it rose, became bounded above by a broad luminous curve, at the same time developing one or more bright concentric arches within. The streamers now shot forth from all parts of the luminous zone; and as these increased the upper arch faded away, as if it had expended itself in producing them. And now the lower arch took its place, to be obliterated in its turn by a like seeming process of exhaustion. At length, one of the grander effusions of light coming on, the whole arch was broken up, and the dark segment below was reduced to a shapeless mass. Then there occurred a comparative pause in the phenomena, until the dark segment again took form, with its one or more luminous bands, and a like cycle of development was repeated.

9. *Observations at Steubenville, Ohio,* (lat. 40° 25′), *from the Steubenville Daily Journal.*

The magnificent auroral display of Aug. 28th was unusually interesting. 1st. It covered a much larger space of the heavens than any we ever saw before, at least since 1835. 2d. It lasted from dark until daylight, appearing with the first approach of darkness and only disappearing as daylight gradually overpowered it. 3d. Instead of an arch, shooting up rapid and various colored rays, its first appearance was that of a luminous mist,

with barely perceptible rays along its southern border, and moving with the rolling motion of clouds, rather than the straight darting motion usually seen in auroras. 4th. It varied in intensity more than any we have ever seen before, twice fading nearly out, and remaining so for nearly half an hour or more, and then kindling up with greater brilliancy than before.

About 7½ P. M. it was a barely perceptible light in the northeast. As the darkness deepened, this luminous spot grew brighter, and moved to the south, till a little before 8 P. M. when the light spreading from it met that coming from the west, and formed an arch about half way between the zenith and southern horizon, and there its advance ended, and it began instantly fading out. It retreated just as it had advanced, only more rapidly, and at $8^h\ 10^m$ there were left only the two centres in the northeast and northwest with a fitful gleam between them. During this retreat, portions of the luminous cloud broke off and floated for some minutes far away from the main body, surrounded by deep darkness, like islands. One of them, and the most beautiful, was a long bright bar in the south, which extended more than half way across the sky from west to east, with a wide sea of darkness between it and the parent cloud, which gradually melted away and disappeared to the westward.

At 9 P. M. the light advanced again, this time with a blood-red tinge in the eastern and western portions, and passed clear to the south as before, but shooting up many and variously colored rays, sometimes from the east, sometimes from the west, sometimes from the north, and from all parts of an irregular luminous arch that bent over the northern horizon about twenty degrees above it. This display faded away in an hour, and at 10½ P. M. there was no light that would attract attention, more than is frequently seen in the north.

About 3 A. M. it blazed up with redoubled brilliancy, shooting up white rays far above the zenith, and making the earth as light as a full moon behind a mist could have done. This time the rays seemed to dart up in broad masses, giving the sky the appearance of being covered with slabs of light, which were tinged with red in the zenith, and rested on a broken irregular arch in the north that in some places fell to the horizon, and in others rose in angular openings to thirty degrees above. During this last display, the pulsations of the aurora were beautifully marked, the rays shooting up in a sort of volley, many hundreds together; while broken and separate masses of luminous cloud were seen in various parts of the sky.

10. *Observations at Burlington, N. J.*, (lat. 40° 5′), *by* BENJAMIN V. MARSH.

Aug. 28th an arch of light rose in the north, passed the zenith and descended to within about 20° of the south horizon by 8^h 30^m P. M. Soon after this, the whole space overhead was occupied by a dense unbroken cloud of milky whiteness. There was however up to this time a considerable number of small black clouds moving southward, which soon afterward disappeared entirely. These clouds were very thin, and we were for a while in doubt whether they were not patches of clear sky; but by watching their effect upon the stars, we satisfied ourselves that they were clouds.

Still later, about 20° above the south horizon, there was a dense whitish arch a few degrees in width, its lower margin being regular and well-defined. About 30° or 35° above the north horizon was the top of another arch, wider than the first, but not so regular or well-defined. Between these two arches were numerous streams and fragments of white auroral cloud.

Between $9\frac{1}{2}$ and $9\frac{3}{4}$ P. M. there was a perfect corona; the streamers on the south side were short, and mostly white, and moved pretty rapidly westward. Their number at one time was probably five or six. At one time the central space was perfectly clear; but afterwards the streamers ran through it to its centre.

11. *Observations at Crawfordsville, Indiana*, (lat. 40° 3′), *by* Prof. JOHN L. CAMPBELL.

Aug. 28th, the aurora began about 7^h 30^m P. M. with an unusual white light in the form of an arch in the north. At 8^h 45^m P. M. the white light appeared in two brilliant spots about 60° on each side of the magnetic pole.

At 9 P. M. streamers of white, red and pink light in circular currents about the magnetic pole (variation 5° 45′ east) extending beyond the zenith.

At $9\frac{1}{4}$ P. M. the streamers were concentrated into brilliant ones passing nearly along the magnetic prime vertical.

At 10 P. M. streams of white light were formed in the east, and rapidly passed westward, a little south of the zenith. These streams or clouds were entirely separate from each other, and the more northern band, and possessed a real motion. The time occupied in passing was about one second. Not less than twenty flashes passed over. They were formed about 30° above the eastern horizon, and disappeared about 60° above the western. After 10 o'clock the white light in the north became more brilliant, and tinged with red, extended very far towards the south. We traced the red tinge on the east to within 40° of the

south point, and on the west to within 50° of the same. At 11 P. M. the aurora was still bright in the north.

Aug. 29th, 2 A. M. Very brilliant streams of white and red light filled the northern hemisphere. These streams were perpendicular to the horizon in the north, and were inclined at regular decreasing angles towards the east and west to about 70°. Brilliant flashes passed across the heavens, originating in the northeast, and passing in a southerly direction vanished in the southeast.

At $2^h\ 45^m$ A. M. Heavy bank of red light ten degrees north of east. Patches of white light in north with occasional streams.

At 3 A. M. the whole northern hemisphere was filled with streams of white light with the same inclination as at 2 A. M. At $3^h\ 15^m$ A. M. the auroral storm was at its height. Flashes of red and white light each instant flew across the northern hemisphere. At $3^h\ 30^m$ A. M., a bright band of white light covered the hemisphere except low down in the north; and the incessant flashes in the northeast and east still continued. Long streams of light flashed across the entire hemisphere. The lower part of the band passed through the heavens at an elevation of 40°. In the zenith was displayed a brilliant red bank. In the east, red and white flashes were very brilliant—better defined but not so rapid in transition as at $3^h\ 15^m$ A. M.

At $3^h\ 45^m$ A. M. Magnificent corona in the zenith. Central portion spiral, red and white, changing instantly to a beautiful rose color, with spiral streams shooting forth into all parts of the heavens; the most brilliant streams flowing east and west. The heavens were completely covered with these streams of light.

At 4 A. M. the white light in the north was still very bright, but the dawn obscured the eastern bank.

12. *Observations at Philadelphia,* (lat. 39° 57′), *by* CHARLES J. ALLEN.

Soon after half past 8, Aug. 28th, the southern margin of the luminous auroral curtain was well defined, and its position between two fixed objects carefully noted. It was afterwards ascertained by actual measurement that this gave an elevation of about 22½ degrees above the southern horizon.

13. *Observations at Sandy Spring, Md.,* (lat. 39° 9′), *by* Prof. BENJAMIN HALLOWELL.

The elevation of the southern margin of the luminous auroral curtain above the southern horizon, Aug. 28th, about 9 P. M., I thought, and mentioned to those with me, was about the meridian altitude of the equator, say 51 degrees.

14. *Observations at Stockton, California,* (lat. 38° 10′), *from the San Joaquin Republican.*

The aurora of Aug. 28th first appeared about 9 P. M., when a faint white light commenced about north and extended to about east by north. About 9¼ P. M. great streams of red and blue shot up all along the northeastern horizon, but they appeared to shoot highest about mid-way of the light. These streams would faint and brighten in such a wonderful manner, that we imagined some painter in the skies drew his great brush from the horizon up to 40 or 50 degrees, dipped with vermillion, then with sky blue, and then with white and flesh color.

15. *Observations at Sacramento, California,* (lat. 38° 34′), *by* THOMAS M. LOGAN, M.D.

I have observed the aurora only at five different times during a residence of nine years at Sacramento, viz., Dec. 16, 1857, Oct. 27, 1858, Aug. 28, 1859, Sept. 1, 1859, and Oct. 18, 1859. I know of but three other well authenticated instances of the phenomenon having been witnessed in California; one by Geo. H. Goddard at Sonora, Jan. 19th, 1852; and two by Henry Gibbons, M.D., at San Francisco, Jan. 19th, 1852, and Feb. 19th, 1852. This shows the infrequency of its appearance in this State.

The aurora of Aug. 28th, 1859, commenced at 9 P. M. and ended about 3 A. M. next morning. The appearances exhibited during this extended period were so various as to render it impossible to note the particular hours of the different changes. In its perpetual movements and fantastic changes were recognized all of the characteristic features that mark this phenomenon, from its close resemblance to the aspect of the sky before sunrise, to the formation of the luminous arc, darting forth palpitating rays towards the zenith, of white, pale red, and deep blood color. This last mentioned feature was seen in its greatest glory at about midnight; and lambent streamers about this time were noticed to shift gradually from west to east, and vice versa. The summit of the arc was not more than six or eight degrees above the horizon, and appeared to coincide with the magnetic meridian. The lower segment of the arc was not as obscure as in the other auroras observed by us. The most remarkable feature during the whole display, was the long continued gleaming of a dark rose or carmine illumination, particularly at the western extremity of the arc; this rosy light passing occasionally along the belt with a fluctuating movement towards the opposite end. The whole northern sky at one time seemed to be a cupola on fire, supported by columns of divers colors, relieved and intensified by dark shadows or rather streaks. The sky remained almost entirely clear the whole night. This au-

rora was observed in all parts of the State, and very generally throughout the whole north Pacific region.

The aurora of Sept. 1, 1859, was first observed about 10 P. M. There was seen first a warm glow in the northwest, and two white silvery clouds in the north. Soon the light extended in all directions, until the entire firmament was suffused with a ruddy light so bright at times that the hour could be distinguished on the dial of a watch. At midnight a splendid glowing corona was seen extending from the eastern to the western horizon, and the whole southern hemisphere appeared to be in one continuous blaze. These ever changing phenomena continued to manifest themselves until lost in the dawning day.

16. *Observations at St. Louis, Mo.* (lat. 38° 37′), *from a St. Louis Journal.*

On the night of Sept. 1st we had a most beautiful exhibition of the aurora. The view did not approach its highest grandeur until after 11 o'clock. At first there was a hazy appearance, embellished here and there by faint streaks and tremulous touches of light. Then the wavy pencillings grew stronger and broader, and the light spread until it had crept up to the zenith, when half of the world seemed enveloped in a sheet of mellow flame.

17. *Observations at Louisville, Ky.* (lat. 38° 3′), *from the Louisville Journal.*

One of the most magnificent auroras ever witnessed in this latitude was seen about 9 o'clock, Aug. 28th. The whole heavens, from the northern horizon to the zenith, were brilliantly illuminated with a rose-colored light, and the flashes were very vivid. The northwestern sky was the portion most brilliantly illuminated, but in the northeast, the rosy flush was exquisitely beautiful.

18. *Observations at Charleston, S. C.* (lat. 32° 46′), *from the Charleston Mercury.*

Aug. 28. The northern heavens were brilliantly lighted until about 2 o'clock Monday morning, when the aurora faded entirely away. On the morning of Sept. 2d the auroral exhibition far surpassed any former instance observed in this city, for the general extent and diffusion of the lights.

19. *Observations at Bermuda* (lat. 32° 34′), *from the Bermuda Royal Gazette.*

The aurora of Aug. 28th appeared to ascend from a few degrees above the northern horizon a great distance upwards towards the zenith, assuming a variety of shades and beautiful

colors; sometimes in tremulous sheets of pale yellow, changing gradually into a deep crimson, or shooting upwards in streams of light resembling those frequently observed from the setting sun. It covered at times the entire space between N.W. and N.E., leaving the sky from the horizon to its apparent base, perfectly clear. Towards 10 o'clock its brilliancy gradually died away, but it continued more or less visible till the dawn of day.

Sept. 2d, between 2 and 3 A. M., the aurora displayed itself in greater splendor than it did on the 28th. Many persons were awakened from their slumbers by the intense light which entered their chambers.

20. *Observations at Savannah, Ga.* (lat. 32° 5′), *from the Savannah Republican.*

On the evening of Aug. 28th we had a brilliant display of the aurora borealis. The northern sky, for an extent of some forty-five degrees, was luminous with a mass of red light, from whence shot up towards the zenith the usual streaks, at times vivid and beautiful.

Sept. 2d, about 1 A. M. the aurora again appeared and was of a very intense and beautiful color, being a mixture of pink, gold and purple. After it had reached an elevation of about 45°, it seemed to dissolve in the centre, and spread out both east and west. About 2 o'clock it formed a complete arch overhead, from N.E. to S.W. About 3 A. M. it gathered in the zenith, and sent out bright fiery flashes in every direction. It was far more magnificent than the aurora of Aug. 28th.

21. *Observations at Mobile, Ala.* (lat. 30° 41′), *from the Mobile Daily Register.*

The aurora showed itself a little east of north about 7¼ P. M. Aug. 28th, and kept up the exhibition until about 9½ P. M., when its paling light died out. It was of a reddish hue, inclining to yellow, and its flickering light assumed a kind of pyramidal form, shooting up into the heavens, nearly to the zenith. Then the centre seemed to grow dim, and a division took place, its right wing moving to the extreme north, where the left wing after a short time joined it.

Between 12 and 3 o'clock on the morning of Sept. 2d, the aurora was repeated upon a scale of beauty and grandeur never before witnessed in the south. A bright pink colored light shot up from the northern horizon, and darted off into beautiful rays, flickering and brightening until they reached the zenith, and soon encircled the hemisphere like a belt from east to west. After about three-quarters of an hour, during which time the aurora occasionally furnished light enough to read by, the bright and beautiful light suddenly clothed the entire firmament.

22. *Observations at New Orleans, La.* (lat. 29° 57′), *from the New Orleans Daily Delta.*

A grand auroral display appeared between 8 and 9 o'clock, Aug. 28th, in the northern horizon. A column of light first shot up into the sky, which soon spread up towards the zenith, and around the horizon, and made one of the most magnificent appearances that the sky has ever exhibited.

About 11 P. M., Sept. 1st, the aurora reappeared and continued until 3 or 4 o'clock in the morning. Nearly the whole visible northern hemisphere was covered with a fiery, blood-reddish, though transparent vapor. The deepest color was on the east and west, a space around the polar centre seeming to be the only non-illumined portion of the northern heavens. Over this crimson ground, spears and pencils of pale flickering light shot up at intervals from the horizon, converging at a point near the zenith. The whole sky along those lines was at once luminous and tremulous. In a moment vast segments of arches would rise, and then suddenly disappear.

23. *Observations at Galveston, Texas* (lat. 29° 17′), *by* Prof. C. G. FORSHEY.

Aug. 28th, as early as twilight closed, the northern sky was slightly lurid, and at times lighter than other portions of the heavens. At $7^h\ 30^m$ a few streamers showed themselves. Soon the whole sky, from Ursa Major to the zodiac in the east, was occupied by the streams or spiral columns that rose from the horizon. Spread over the same extent, was an exquisite roseate tint which faded and returned. Stately columns of light reaching up about 45° from the horizon, moved westward about one degree for every ninety seconds of time. There were frequent flashes of lightning, apparently from distant clouds, along the whole extent of the aurora; but no clouds were visible, except a single streak near the horizon. At 9 P. M. the whole of the streaking had faded, leaving only a sort of twilight over the northern sky, and we ceased our observations.

At 3 A. M. Aug. 29th, I awoke and perceiving that it was very light outside, rose, and found the whole northern heavens again on fire. Such a display I have never seen equalled since the aurora of Sept. 1, 1839. The whole distance before-named was tinted with the roseate hue; darker, nearly crimsoned at the two flanks. In the centre, near the meridian, stood a stupendous pyramid of white light with its apex near the zenith. On either side at some twenty degrees, stood a pyramid of rosy light, each about sixty degrees in height and in exactly symmetrical positions. Scarcely had I sketched the outline of this noble spectacle when the columns drifted westward and faded.

Another fine display of the aurora commenced about 10½ P. M. Sept. 1st, and lasted until near daylight the next morning. A dusky red, like the reflection of an immense conflagration, overspread almost the entire heavens, beyond the zenith, far down towards the southern horizon.

24. *Observations at Sea* (lat. 28° 30′, long. 79° 30′), *Barque Pride of the Sea.*

Sept. 2d, at 12^h 35^m A. M., a bright spot or cloud appeared in the N.W. which shot up rays resembling the aurora, and in thirty or forty minutes formed an arch across the horizon from N.W. to N.E., which became lighter as it arose, and at 1^h 15^m A. M. it was light enough to read the smallest print without a light. At the time the horizon was cloudy, but overhead was clear, the larger stars being just seen. At 2^h 15^m the arch passed over to the southward, when it became dark again.

25. *Observations at Key West* (lat. 24° 32′), *from a Journal.*

A brilliant exhibition of the aurora was witnessed at this place Aug. 28th, and a still more brilliant one on the morning of Sept. 2d. The whole northern half of the heavens was tinged with crimson, red as blood. Occasional flashes of blue and white light shot up towards the zenith and then slowly melted away.

26. *Observations at Havanna, Cuba* (lat. 23° 9′), *by* M. ANDREAS POEY.

In his former communication (Am. Jour., vol. xxviii, p. 406) Mr. Poey stated that during the auroras of Aug. 28th and Sept. 1st he was unable to obtain any indications of atmospheric electricity. In a later communication he states that neither at the time of these auroras, nor on the preceding or following days, was there the smallest interruption or disturbance experienced on the electro-magnetic telegraph lines of Cuba.

27. *Observations at Inagua, Bahama Islands* (lat. 21° 18′), *from the New York Journal of Commerce.*

The aurora of Aug. 28th was distinctly seen from this place, and was supposed to have been a large fire in the neighborhood. It was remarkably brilliant, but was not attended by that flashing appearance which is sometimes noticed in higher latitudes.

28. *Observations at Cohe, Cuba* (lat. 20°), *by* GEORGE F. ALLEN.

On the night of Sept. 1st a Spanish mechanic who worked for me called me out of bed to see the great light in the northern sky. He was much struck with it, and said the people in St. Jago de Cuba would think the end of the world was at hand.

I found a display which would have been considered more than ordinary even in the latitude of New York. It resembled the auroral displays occasionally seen in New York when more than usually brilliant. The same rosy light, on a darker horizon, fading off into yellower and whiter as it spread upwards, variegated occasionally with white streamers. It extended horizontally, according to my rough estimate, about one-third or two-fifths of the horizon, and upwards about two-fifths of the arch of the visible heavens. It was a very brilliant display, and surprised me much by its brilliancy in that latitude.

29. *Observations at Kingston, Jamaica* (lat. 17° 58′), *from the New York Herald.*

An extraordinary light appeared in the north on the night of Sept. 1st and the morning of Sept. 2d. It appeared as if there was a colossal fire on earth which reflected its flames on the heavens. The whole island was illuminated. The light was seen at Montego Bay (lat. 18° 21′) at 10 P. M., but it was not observed at Kingston until 1 A. M. Sept. 2. It continued until 5 A. M., when it gradually disappeared. It looked as if Cuba was on fire, and many believe that a portion of this island had been destroyed by a conflagration. Other persons were of opinion that the light was that of an aurora, but the aurora has never before been seen in this latitude. A similar fire was observed on the north side of Jamaica Aug. 28th.

30. *Observations at Guadeloupe, West Indies* (lat. 16° 12′), *from L'Institut.*

On the 2d of September, from $1\frac{1}{2}^{h}$ till daylight, an Aurora Borealis was seen at Guadeloupe to the great astonishment of the population. Its ruddy light was noticeable in the interior of the houses. At the centre of this vast conflagration were noticed two rays of whitish light which rose parallel to each other, passing a little to the left of the pole star. The aurora attained its maximum of brightness at 3 A. M.

31. *Observations at La Union, San Salvador* (lat. 13° 18′), *from the Gaceta del Estado.*

On the night of Sept. 2d, a most extraordinary phenomenon was witnessed. About 10 o'clock, a red light illuminated all the space from north to west, to an elevation of about 30° above the horizon. The light was equal to that of day-break, but was not sufficient to eclipse the light of the stars. The sea reflected the color, and appeared as if of blood. This lasted until three in the morning, when a dense black cloud arose in the east, and commenced to spread over the colored portion of the heavens,

presenting a most curious spectacle; for in the parts where the cloud was not dense enough, the red light shone through, and formed a thousand fantastic figures, as if painted with fire on a black ground.

In the city of Salvador (lat. 13° 44′) the same phenomenon was visible, occupying the same space in the heavens, and the red light was so vivid that the roofs of the houses and the leaves of the trees appeared as if covered with blood.

February, 1860.

ART. XXV.—*Correspondence of Mr. Jerome Nicklès, dated Nancy, November 10th*, 1859.

Biography.—Cagniard-Latour.—We have already given some notice of this physicist, so lately lost to science; the following details are taken from an autobiography, which gives a very interesting account of the circumstances which led him to some of his discoveries. His researches may be classed under four heads; acoustics, mechanics, chemistry, and general physics having successively occupied his attention. His first invention (1809) was a pneumatic Archimidean screw, which is now in common use for conveying gases under liquids, and has received the name of the *Cagniardelle.* The ingenious inventor simply inverted the action of the ordinary screw of Archimedes, making it revolve from right to left. As Arago remarked in the chamber of deputies in 1844, during a discussion of the law of patents, although the *Cagniardelle* is nothing more than Archimedes' screw reversed, it is not less true that 2000 years had passed before any one conceived the idea of making this simple change and rendering it available in mechanics as a pneumatic machine.

The *Siren* (1819) as is well known, is an instrument for measuring the vibrations of the air which constitute sound. If, as had been supposed by physicists, the sounds produced by musical instruments are due to the regular succession of impulses given to the air by their vibrations, it was evident that a mechanism which would enable us to strike the air with the same rapidity and regularity should in like manner produce sounds. Reasoning in this manner, Latour was led to the invention of this well-known and beautiful instrument.

In 1822 he published his experiments on the combined action of heat and pressure upon certain liquids, such as water, alcohol, ether and naphtha. He imagined that the dilation of a volatile liquid must have a limit beyond which, notwithstanding the compression, it would pass to the state of vapor, provided that the capacity of the vessel permitted the liquid to expand beyond its maximum of dilation. The remarkable results to which he was led by this reasoning are well known.

In 1837 he published with Mr. Demonferrand the description of an acoustic pyrometer, by which the authors proposed to render the measurement of all temperatures appreciable through the medium of sound. In the same year he examined the pressure to which the air in the trachea is exposed during the act of sounding. He had previously been employed

in investigating the pressure to which the air in the lungs is exposed when employed in sounding certain reed instruments, and had found it for the clarionette equal to the pressure of the atmosphere, *plus* a column of water of 30 centimeters. In order to extend these inquiries to the human larynx, it was necessary to find a person having an opening in the trachea, and yet able to produce vocal sounds at will. After long search, Cagniard-Latour found such a man, who was for his purpose as precious as the subject with the permanent gastric fistula became for the well-known experiments of Dr. Beaumont. In the same year he made known his chronometric balance, designed to measure the dynamic effects of machines in motion.

Next appeared a memoir on the alcoholic fermentation, of which these were the principal results:

1st. The yeast of beer is made up of little globular bodies, apparently vegetable, and capable of reproduction in two different manners. These bodies seem to act upon a solution of sugar only when in a state of life, and he hence conceived it probable that it is by a vital process that they transform the sugar into carbonic acid and alcohol. This investigation was suggested by a question long before proposed as the subject of a prize by the French Academy of Sciences, in the seventh year of the Republic, viz. What are the characters which distinguish among animal and vegetable matters, those which serve as ferments and those which are subject to fermentation?

Cagniard-Latour now resumed his researches upon vibrating bodies, and succeeded in producing a sound by causing a glass rod to oscillate easily between two metallic columns. The peculiarity in the sound thus obtained was that the number of vibrations indicated by it corresponded only to one-half the synchronous number of simple oscillations of the rod, although the apparatus was arranged in such a manner that each movement backwards and forwards should produce two strokes of equal intensity, by the alternate blows upon the two columns. The experiments made with this instrument enabled him to give the theory of the production of sounds by vibrating cords. During the same year (1840) he studied the production of grave tones, like those of the human voice, and made various researches to discover the mechanism of the human voice.

In 1851 he laid before the Academy a memoir upon the *moulinet à battemens*, demonstrating some new acoustic phenomena. In two previous papers published in 1830 and 1831, upon the sounds produced by solid bodies turning with great velocity, he had shown certain facts relative to the musical tones produced by the friction of the axle of a wheel against its supports. Subsequently he conceived the idea that a solid of revolution, a cylinder for example, arranged so as to turn vertically around its axis upon two center-holes, might give rise to pulsating sounds (beatings), although moving with a feeble velocity, provided it received in the lower center-hole the friction of the revolving axis of a winch turning in an opposite direction to the cylinder. It was to the instruments constructed according to these ideas that he gave the name of the *moulinet à battemens.*

We have also from Cagniard-Latour an investigation on the action of heat on different kinds of wood enclosed in hermetically sealed glass

tubes. Sir James Hall, in his experiments upon the saw-dust of pine wood and of horn in sealed gun-barrels, had observed that the mixture underwent fusion and was cemented into a sort of coal. Similar results were obtained by Latour with thick glass tubes.

These are not the only researches which we owe to this lamented physicist; in our previous letter we mentioned that while connected with the Government powder works in 1814, he made some useful improvements in that department, especially in the glazing of powder. We also spoke of his very light, portable, and efficient flour-mill, which consisted of a steel rasp moving vertically with an alternating motion between two fixed rasps, also of steel. During the severe winter of 1816, the streams being all frozen and the mills stopped, Cagniard-Latour was directed to have an immense number of these hand-mills constructed, and thus in a few days the public were saved from the fears of a famine. We have also seen the part which he took in the establishment of gas-lighting in Paris. He was besides the constructer of an aqueduct, a model of its kind, suspended between two rocks, and formed of a single span 200 metres in length. When we consider these varied achievements, we learn with surprise that it was only in 1851 that he became a member of the Academy of Sciences.

The aurora borealis and its theory.—The late brilliant auroras have called attention to De la Rive's theory, of which we have formerly spoken, and which is explained at length in his *Traite d'Electricité.* Great perturbations were observed along the telegraphic lines over the European continent, similar to those remarked some years since by Matteucci in Tuscany, and Highton in England. The most remarkable fact in these electrical disturbances is that they were produced by a continuous current, while those of a thunder storm are instantaneous, and only mark points upon the paper in Morse's apparatus; the aurora of the 29th August traced continuous lines of greater or less length. These effects lasted for several days after the aurora.*

It is fortunate that the aurora of the 29th was carefully studied by a man so competent as Coulvier-Gravier. This observer, who has studied the heavens for nearly 60 years, and has so much advanced the sciences of Cosmography and Meteorology, was found that night as usual at his post in the observatory which the government prepared for him twenty years since at the Luxembourg palace. The phenomenon was in all its splendor at $2^h\ 45^m$ A. M.; its extent included more than 100°, and M. Coulvier-Gravier declares that he had never seen it more beautiful during the long period of his observations.

The observations during the late auroras support the theory of De la Rive, which he has thus defined. The vapors constantly rising from the sea, and especially from the equatorial regions, carry with them to the higher regions of the air a great quantity of positive electricity, to which they serve as the vehicle, leaving the surface of the globe negatively electric. Borne to the poles by the currents which always prevail in the higher regions of the atmosphere, these vapors carry with them their electricity, and thus give to the whole atmosphere a positive electric condition, which diminishes from above downwards. This positive electricity tends unceasingly to combine with the negative electricity of the earth,

* See p. 92, this volume.

directly through the stratum of air, but more especially at the two poles, where the currents of vapor carried by winds converge and are condensed.

According to Mr. De la Rive the aurora of the 29th August was a natural consequence of the great drought which had prevailed over the continent. The dryness of the air had prevented the positive electricity from neutralizing itself directly with the negative of the earth. From this accumulation of electricity there finally took place towards the polar regions a discharge much more intense and much more rapid than usual, which constituted the brilliant aurora in question. A fact which tends to show that the aurora is an electrical and not a magnetical phenomenon, is furnished by the ozonometrical observations made by Mr. Perigny, at Versailles. From the 28th of August to the 2d of September, he found the air to contain a quantity of ozone, relatively large, and more abundant by night than by day.

Human Remains in the Drift.—For the last twenty years it has been known that axes of flint, evidently wrought by human skill, were found in beds of the drift at Amiens, associated with the bones of extinct species of animals. This discovery, made by a learned antiquary, Mr. Boucher de Perther, had been regarded as doubtful, and it was supposed that sufficient precaution had not been observed in conducting the explorations. Recent discoveries made in a cavern at Brixham, near Torquay, in England, however, recalled attention to the observations of Mr. Perther, and Mr. Prestwich, with several other geologists, accordingly visited Amiens in order to make the excavations necessary to decide this important question. Every precaution was of course taken to prevent errors or deception, and Mr. Prestwich could find nothing at Abbeville, but at Amiens he was more fortunate; one of his companions, Mr. Flower, in examining a bed of gravel at six meters from the surface, and evidently undisturbed, extracted with his own hands a fine axe, more than five inches long. After this Mr. Prestwich having been informed that a similar discovery had been made in 1737, at Haxne, in Suffolk, visited the place, and learned that some years since wrought flints were still found in abundance, although rare at present. He however succeeded in finding two axes, similar to those of Amiens, but of less perfect finish. An analogous fact has just been verified by French geologists, who have found these axes in Picardy, associated with remains of *Elephas primogenius*, *Rhinoceros tichorhinus*, *Equus fossilus*, and an extinct species of *Bos*. The conclusion from all these facts is that man was cotemporaneous with these several species of large animals now lost, and known to us only by their fossil remains.

Curare in the treatment of Tetanus.—This important question has elicited much discussion at the Academy, and many contradictions and differences of opinion. In 1850, Mr. Claude Bernard showed that the curare, or arrow poison, acts by paralyzing the system of motor nerves; following up this observation, an Italian physician, Villa, of Turin, made in 1854 a series of experiments, in the course of which he showed that curare exerts upon the nervous system an action so completely antagonistic to that of strychnine, that the two poisons may be neutralized by each other. Dr. Villa having been attached to the French hospitals during the late war, was induced to apply the curare in the treatment of three cases of trau-

matic tetanus one of which recovered. It was the case of a soldier wounded by a ball in the right foot. The curare dissolved in water was applied to the wound, with the effect of diminishing the pain and suspending temporarily the tetanic spasms, which, however, returned. After fifteen days of this treatment the patient left the hospital completely cured. The experiments of Mr. Villa have been repeated in the hospitals of Paris; but as yet only a single case of cure has been reported out of several failures; the experiments, however, continue. According to a letter from Sir Benj. Brodie, of London, to the Academy of Sciences, the application of curare as a specific against tetanus, was unsuccessfully made upon horses at London in 1815, by Dr. Sewell, professor at the Veterinary College. Such are the principal facts in the question as far as yet made known.*

The new alloys of Platinum.—We recall the interesting researches of Messrs. Deville and Debray on this subject only to mention their industrial applications. Hitherto it had been supposed that the presence of iridium impaired the quality of platinum, but the labors of Deville and Debray have shown that on the contrary alloys of these two metals may be prepared which are greatly superior to pure platinum, presenting greater strength and rigidity, and resisting better both heat and acids. Thus the alloy containing 21·3 of iridium is highly malleable and scarcely attacked by aqua regia. As the quantity of iridium is less, the alloy becomes softer, and one containing 10 or 15 per cent. is peculiarly fitted for chemical vessels. These alloys are now largely wrought in Paris; retorts for the manufacturers of sulphuric acid have been made, having the strength and rigidity of rolled iron.

Messrs. Deville and Debray are at present making some trials at the French mint, for the Russian government, to determine the fitness of the new alloys for coinage. They have found that those containing 20, 10, 7½ and 4 per cent of iridium, take the impression of the dies with great perfection. The same is true of the natural alloy, which is obtained by directly fusing crude platinum, and retains only the iridium and rhodium in combination with the platinum, the other metals having been removed by volatilization or oxydation. The platinum workers of Paris are now manufacturing and selling the new alloys, and, contrary to the wishes of the discoverers, are exacting higher prices than for pure platinum.

Rifled Cannon.—The invention of these guns appears to be due to a former captain of artillery, Mr. Tamisier, who was in 1842 charged with the course of instruction in musketry at Vincennes, where he applied himself with great assiduity to the study of various questions connected with his profession, with results which have contributed very much to the improvement of the system of musketry instruction in the army and in the arms of precision. After studying the effects of elongated projectiles in rifles and muskets, Mr. Tamisier was led to construct a rifled mortar, with cylindro-conical shells. The duke of Montpensier, then colonel of artillery, at once saw the importance of this new project, and after causing many experiments to be made, ordered at his own expense, in 1847, the construction by Capt. Tamisier of elongated balls and shells. These however were not tried until 1850, from which time up to 1853, Mr.

* See experiments on two new varieties of South American arrow poison by Drs. Hammond and Mitchell, Am. Jour. Med. Sci., July, 1859, (this Jour. [2], xxviii, 303).

Tamisier continued his experiments at Vincennes and at La Fère. The first experiments were made at Vincennes, on the 15th of July, 1850, when it was shown that rifled six-pounders with elongated projectiles, carried much farther and with greater exactness than ordinary guns of smooth bore. On the 14th August, 1851, Capt. Tamisier repeated his experiments at the polygon at Vincennes, before the President and the Minister of War. A six-pounder rifled with three grooves projected balls of five kilogrammes to a distance of 1500 meters with a charge of only 700 grammes of powder. The government then ordered further experiments to be made, which were conducted at La Fère, a fortified place in the Department of the Aisne, where greater secrecy could be secured than near Paris. The trials were there made by Col. Trenille and by Col. Virlet, now directors of the School of Artillery at Metz, and led to a complete solution of the problem, so that the army of Italy was able to bring into the field more than 200 rifled guns of a calibre of 84 millimeters, requiring for service and transport only two-thirds the men and horses hitherto necessary, and carrying balls of four kilogrammes 3500 meters with such precision that at this distance they would all fall in a rectangle of 80 meters in length by 40 meters in breadth.

Acclimatation.—In our last letter we spoke of a number of Arabian camels, which the Society of Acclimatation had procured in Algeria, to be introduced into Brazil. We learn that they were safely landed there after a voyage of 28 days.

Photo-Chemical Researches: Persistent Activity of Light.—In this Journal for March, 1859, p. 257, we have described the beautiful experiments of Niepce de St. Victor, upon the persistence of the effects of insolation. According to Mr. Laborde, the curious results obtained by Mr. Niepce are due not to radiation, but to a veritable emanation from the card-board, which has been impregnated with tartaric acid, and exposed to the sun-light. The principle subsequently evolved from this card-board, which acts upon sensitive paper prepared with a silver salt, is, according to Mr. Laborde, no other than formic acid. This is well known to be a product of the oxydation of tartaric acid, especially when in contact with peroxyd of lead. He found that a concentrated solution of tartaric acid mixed with peroxyd of lead, and placed in a dark place, evolved vapors which reddened litmus and discolored prepared paper after a very short time. There is, then, in the case of insolated tartaric acid, an emanation and not a radiation; but Mr. Niepce obtained similar results with nitrate of uranium spread upon porcelain and insolated, in which case it is not easy to conceive of an emanation of vapor. However this may be, the facts announced by Mr. Niepce remain, and the only difference of opinion is as to their interpretation.

Maritime Canals.—Besides the canals proposed at Panama and Suez, the question of one between the Caspian and the Black Sea is now being discussed in Russia. The construction of such a canal was ordered by Peter the Great, after having been commenced a century before under the Sultan Selim II, upon the suggestion of his Grand Vizier, Mohammed Sokolli, who was for Turkey what Colbert was for France. The canal was commenced in 1569, and during two years there were employed upon it 5000 janissaries and 20,000 prisoners.

The canal now projected will unite the Don and the Volga, the first of these falling into the sea of Azof, and the second into the Caspian, near Astrachan, at a point designated on the maps as Tzaritsin. The two rivers are separated by a distance of only 55 kilometers. The importance of a canal which will connect the Caspian with the Mediterranean will readily be seen, and it must at the same time be confessed that it will be much more easily executed than the tunnel under the channel proposed by Mr. Thornè de Gamond, since the canal, being exclusively on Russian soil, will be a question for engineers, with which politicians will have no concern.

For the last thirty years, it has been proposed to make a sea-port of Paris by a ship-canal leading from the Channel; the introduction of railways caused the project to be for a time abandoned, but it is now again discussed. Among the plans proposed is one which appears in the *Ann. des Sciences* of the 8th October, 1859. It is intended to excavate at the mouth of the Seine, near Havre, a harbor 1000 meters long and 200 wide, with a depth of 12 meters. The canal, 150 kilometers in length, will follow the course of the Seine, and will have the same depth as the harbor, which will be furnished with gates to preserve the water at a proper height. A double line of railway will accompany the canal, one line serving for passengers and freight, and the other for towing vessels, which will thus make the voyage in four hours.

The position and direction of the canal will be such that the west winds, which are the most frequent at Paris, will help vessels in coming up, while the water of the Seine, to be let in by sluices, will aid them, by their current, in descending. The great difficulty in this enterprise will arise from the tunnels required, the total length of which will be from twenty to twenty-five kilometers. These will be vaulted, and with a height of 30 meters by at least 50 meters in breadth. Mr. Piorry estimates that the expense of this work may amount to a billion (1,000,000,000) of francs.

Engineers are also occupied with a plan for joining the English Channel with the Mediterranean, by taking advantage of the river Rhone, the Saône, the Yonne, and the Seine. The details of its execution having only a local interest, we spare our readers their recital.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On two new series of Organic Acids.*—Hentz has studied the action of methylate of soda and similar substances upon chloracetic acid, and has obtained interesting new acids in which hydrogen may be considered as replaced by the deutoxyds of methyl, ethyl, etc. The acid resulting from the action of methylate of soda on chloracetic acid has the formula $C_6H_6O_6$, which is that of lactic and paralactic acids, but is not identical with either of these. To this acid the author gives the name of methoxacetic acid; it is monobasic, and gives beautifully crystallized salts. The acid itself is easily prepared by decomposing the zinc salt with sulphu-

retted hydrogen, and distilling the liquid after separating the sulphid of zinc. The boiling point rises gradually till it becomes constant at 198° C., when the acid passes over as a colorless liquid, with a sour smell, resembling that of acetic acid.

When monochloracetate of soda is boiled continuously, an acid liquid passes over, which, when saturated with baryta, yields on evaporation a crystallizable salt having the formula of glycolate of baryta. The author proposes for the acid contained in this salt the name of oxacetic acid; it is isomeric, but not identical with the glycolic acid. Its formula is $C_4H_4O_6$.

Ethylate of soda acts violently on monochloracetic acid; the products of the reaction are chlorid of sodium, and the soda salt of a new organic acid, homologous with the two last described, and which the author terms ethoxacetic acid. This acid is volatile without decomposition and appears to boil at a lower temperature than the corresponding methyl compound. Its formula is $C_8H_8O_6$.

Amylate of soda acts in a similar manner on monochloracetic acid. The new acid formed in the reaction is an oily liquid which has the formula $C_{14}H_{14}O_6$.

Phenylate of soda, under similar circumstances, yields phenoxacetic acid, as an oily liquid which crystallizes at a low temperature, and distils over without decomposition. The analyses appeared, however, to show that the phenyl alcohol employed contained benzalcohol, and that the product examined was therefore a mixture of the homologous acids $C_{16}H_8O_6$ and $C_{18}H_{10}O_6$.

When the soda salts of organic acids are heated with monochloracetic acid, chlorid of sodium is formed, and new organic bodies which the author proposes to study. It is easy to see that all the acids belonging to the formic series will yield similar new acids with the peroxyds of the alcohol radicals homologous with hydrogen, and that in this manner a very great number of new compounds may be obtained.—*Journal für prakt. Chemie*, 78, *p.* 174.

Note.—According to Kolbe's view, lactic acid is to be regarded as propionic acid, in which one equivalent of hydrogen is replaced by one of HO_2, so that its rational formula is $\left.\begin{matrix} C_6H_4(HO_2)O_2 \\ H \end{matrix}\right\} O_2$. Upon this view the oxacetic acid of Heintz must be identical with glycolic acid, which appears not to be the case. The question may doubtless be decided directly by examining the products of the action of water upon chloropropionic acid, which ought thus to yield lactic acid directly, since we should have the reaction

$$C_6H_4ClO_5HO + HO_2H = C_6H_4(HO_2)O_5HO + HCl.$$

Heintz's acids may be more simply regarded as derived from the formic series by simple replacement of hydrogen by the peroxyds HO_2, $C_2H_3O_2$, $C_4H_5O_2$, &c. Dichloracetic and trichloracetic acids ought to yield analogous products in which two or three equivalents of hydrogen are replaced by two or three equivalents of peroxyds which need not be of the same radical. The number of possible acids would thus almost or quite equal that of the ammonias or ammoniums. W. G.

2. *On the chemical constitution of Isethionic acid and Taurin.*—By the distillation of isethionate of potash with perchlorid of phosphorus, Kolbe has obtained a new acid which has the empirical formula $C_4H_5ClS_2O_5$, and which he terms chlorethyl-sulphuric acid. This acid yields taurin by the substitution of NH_2 for Cl, and ethyl-sulphuric acid $C_4H_6S_2O_6$ when the chlorine is simply replaced by hydrogen. Kolbe draws a parallel between the derivatives of carbonic and sulphuric acids, which is best illustrated by the following tabular view:

$C_2O_2 . O_2$	$S_2O_4 . O_2$
Carbonic acid.	Sulphuric acid.
$HO . (C_4H_5)(C_2O_2) . O$	$HO . (C_4H_5)(S_2O_4), O$
Propionic acid.	Ethyl-sulphuric acid.
$HO . (C_4H_4Cl)(C_2O_2), O$	$HO . (C_4H_4Cl)(S_2O_4), O$
Chloropropionic acid.	Chlorethyl-sulphuric acid.
$(C_4H_4Cl)(C_2O_2), Cl$	$(C_4H_4Cl)(S_2O_4), Cl$
Chlorethyl-carbon-chlorid.	Chlorethyl-sulfochlorid.
$HO . (C_4H_4NH_2)(C_2O_2), O$	$HO . C_4H_4NH_2)(S_2O_4), O$
Amidoethyl-carbonic acid.	Amido-ethyl-sulphuric acid.
(Alanin)	(Taurin).
$HO . (C_4H_4(HO_2)),(C_2O_2),O$	$HO . (C_4H_4(HO_2))S_2O_4, O$
Oxethyl-carbonic acid	Oxethyl-sulphuric acid.
Lactic acid.	Isethionic acid.

The author promises a more detailed account of the compounds and reactions referred to in the above brief preliminary notice.—*Ann. der Chemie und Pharm.*, cxii, 241.

3. *Researches on the atomic weight of Graphite.*—Brodie has communicated an exceedingly interesting and suggestive memoir on the atomic weight of graphite, considered as an allotropic form of carbon, the fundamental idea being that the different modifications of the same substance may exhibit a difference in equivalents, as well as in their ordinary chemical and physical properties. The author finds that graphite, when heated with nitric acid and chlorate of potash, increases in weight, and ultimately yields a light yellow crystalline substance. The details of the process are as follows: a portion of graphite is intimately mixed with three times its weight of chlorate of potash, and the mixture placed in a retort. A sufficient quantity of the strongest fuming nitric acid is added to render the whole fluid. The retort is placed in a water-bath, and kept for three or four days at a temperature of 60° C. until yellow vapors cease to be evolved. The substance is then thrown into a large quantity of water and washed by decantation nearly free from acid and salts. It is then dried in a water-bath, and the oxydizing operation repeated with the same proportion of nitric acid and chlorate of potash, until no farther change is observed. This is usually after the fourth time of oxydation. The substance is then to be dried, first in vacuo, and then at 100°. By placing the mixture in a flask exposed to sunlight, the change takes place more rapidly and without the application of heat.

The formula of the body thus obtained is $C_{22}H_4O_{10}$, or, as the author writes it, $C_{11}H_4O_5$.

Its crystals belong either to the right or oblique prismatic system. It is insoluble in water, containing acids or salts, and very slightly soluble in pure water. It unites with alkalies, and the crystals have an acid reaction: ammonia converts it into a transparent jelly, but the substance is not dissolved. Acids separate it from this combination, as a gelatinous mass resembling silica. Treated with deoxydizing agents, it is readily decomposed. When a solution of sulphate of ammonium or of potassium is poured upon the dry substance, a crackling sound is heard, and a body is formed resembling graphite.

The crystals are decomposed with ignition on the application of heat, gases being evolved, and a black residue left, which resembles finely divided carbon. This substance the author proposes to term graphic acid.

When graphic acid is heated in naphtha to about 270°, water and carbonic acid are given off, while the naphtha takes a deep red color. The residual substance resembles graphite and has the formula $C_{44}H_2O_8$, or with the author's equivalents, $C_{22}H_2O_4$. When this substance is heated in a current of nitrogen to a temperature of 250°, water is given off with a little carbonic acid; the substance remaining is found to have the formula $C_{132}H_4O_{22}$ or $C_{66}H_4O_{11}$. This body may be exposed for several hours to a red heat in a current of nitrogen without losing all its oxygen and hydrogen.

The author compares graphic acid with a remarkable compound of silicon discovered by Buff and Wöhler, which has the formula $Si_4H_4O_5$, and which was obtained from the graphitoid form of that element. The properties of the two substances agree very closely, whence it may be inferred that the graphite compound is the same term in the system of carbon as the silicon compound in the system of silicon. The total weight of graphite which in the compound is combined with atoms of hydrogen and of oxygen is 132. If we assume that this weight is like the corresponding weight, 84 of silicon, to be divided into four parts, we arrive at the number 33 as the atomic weight of graphite. Representing this weight by the letters Gr, the formulas of the substances $C_{11}H_4O_5$, $C_{22}H_2O_4$, and $C_{66}H_4O_{11}$ become $Gr_4H_4O_5$, $Gr_8H_2O_4$ and $Gr_{24}H_4O_{11}$, where $O=16$.

According to the law of Dulong and Petit, the specific heats of the elements are inversely as their equivalents. The elements are divided into two classes, one in which the product of the specific heat into the equivalent is about 3·3—the other in which this product is 6·6. The specific heat of carbon in the form of graphite—0·20187—presents a remarkable exception to the law, if we take its equivalent as 6· or 12, but if we assume the atomic weight of graphite as 33, we have for the product of the specific heat into the atomic weight, the number 6·6 which is according to the law of Dulong and Petit.

The relation which exists between the atomic weights of boron, silicon and zircon, and that form of carbon for which a place may be claimed as a distinct element, *graphon*, is precisely the kind of numerical relation which is found to exist between the weights of analogous elements. We have

Boron	11
Silicon	21
Graphon	33
Zircon	66

These considerations lead to the inference that graphite functions as a distinct element, forming distinct combinations with a distinct equivalent, viz: 33. How far this inference may be extended to the allotropic forms of other elements, experiment alone can decide.—*Quart. Journal of Chem. Soc.*, vol. xii, p. 261.

[*Note.*—With respect to the numerical relations between the eqs. of boron, silicon, graphon and zirconium which Brodie points out, it may be remarked that boron—at least with the equivalent 11—is triatomic, as shown by the density of the vapor of BCl_3 and other considerations. It cannot, therefore, with this equivalent, belong to the same natural group with silicon and zircon, which are diatomic, as shown by recent investigations. Marignac has established the isomorphism of SnF_2+KF with SiF_2+RF, while Troost and Deville have shown from the vapor-density of chlorid of zirconium that its true formula is ZCl_2=2 vols. or Z_2Cl_4 if we assume that all compounds correspond to 4 vols. in a gaseous state. The vapor density of $SiCl_2$ also agrees with the supposition that silicon is diatomic, supposing it to represent 2 vols. The true equivalents of silicon and zirconium become therefore respectively 14 and 44 or 28 and 88, if we admit the 4-volume theory. The equivalents of carbon, silicon and zirconium are then to each other as 6, 14 and 44, or as 12, 28 and 88, the common difference being 8 or 16 nearly. The formula $Si_4H_4O_{10}$ was deduced by Buff and Wöhler upon the supposition that the equivalent of silicon is 21, the element being triatomic as assumed by Berzelius. But if we take 14 as the true equivalent, the formula for the same compound becomes $Si_6H_4O_{10}$, and comparing with this the formula $C_{22}H_4O_{10}$ we have 132 parts by weight of carbon, representing 6 eqs. of graphon instead of 4, as assumed by Brodie. This gives 22 as the equivalent of graphon, instead of 33. If now we multiply the spec. heat of graphite as found by Regnault, namely 0·201, by 22 we have 4·4 so that the spec. heat of an atom of graphon does not obey the laws of Dulong and Petit, as the product should be either 3·3 or 6·6. It may, however, be remarked that the spec. heat of graphitoid silicon has not yet been determined, and that there may be other classes of elements whose atoms have the intermediate spec. heats 4·4 and 5·5. The formulas of Brodie's compounds become, if we take the equivalent of graphon as 22—$Gr_6H_4O_{10}$, $Gr_{12}H_2O_8$, $Gr_{18}H_4O_{22}$, (taking O=8 and not with Brodie as 16). No probable relation can be pointed out between the numerical values of the equivalents of graphon and of other elements, until we know to what natural group graphon belongs, since it is not certain or even very probable that the allotropic modifications of the same element belong to the same group.—W. G.] W. G.

4. *On the Cause of Color and the Theory of Light;* by Mr. JOHN SMITH, M.A. (Read by his brother, Dr. R. A. Smith).—The author, in attempting to explain certain natural phenomena, could not satisfy himself by applying the principles of either theory of light, and said that many natural phenomena indicated beats or vibrations in the luminous ether very

different from what science taught. That is, that there were greater intervals between them than Newton had demonstrated and scientific men believed. He therefore endeavored to contrive experiments by which he would be able to make as many revolutions or beats in a second as he considered the effective vibrations of light were repeated in a second of time, and argued that by certain contrivances to produce light and shade in alternate vibrations he should produce color. A series of experiments was subsequently undertaken, which led to the conclusion that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color. That is, supposing white light to consist of the motion of an ether, blackness to consist of an entire absence of motion, then a certain color, blue, red, or yellow, will be produced by the alternate action of the light and the shadow. The author used shadow in the positive sense as the sensation was positive.

On pursuing the inquiry, he first caused a small parallelogram cut in card board to revolve over a black surface with a rapidity which he considered equal to the vibration of light. By this motion he obtained a distinct blue, while at another time in different weather he obtained a purple. He then made a disc with several concentric rings, which he painted respectively $\frac{1}{3}$, $\frac{2}{3}$, $\frac{3}{4}$, and $\frac{1}{2}$ black, leaving the remainder white, and on making this disc revolve the rings became completely colored. There was no appearance of any black or white. In a bright day with white clouds in the sky, the rings were colored respectively a light yellowish green, two different shades of purple, and a pink. By using discs of a great variety of shapes and different proportions of white and black, the author said that he produced successively or together all the colors of the rainbow, although he had not yet arrived at the exact arithmetical determination of the amount of light and shade needful for each color.

These experiments were made before the Society by the light of a paraffin oil lamp with a reflector. The author said that they were much more brilliant by sunlight.

There was another set of experiments which the author considered as very effective, and especially as being easily made and described, but requiring strong sunshine to show them. These were made by casting a shadow of a particular figure on a white wall or on a sheet of paper, so as to produce alternate beats of light and shadow when put in revolution. The figure became colored of different shades, and because these could be seen on the wall, like the spectrum from the prism, he called them spectra by reflection.

He mentioned also that the colors may be produced by making a black disc, with figures cut out of it, revolve before a white cloud or white screen.

There were many others which he had no time to enumerate, much less to describe, but he described some of the figures which produce the phenomena which are perceived when looking through transparent solids.

The author considered that his theory gave an entirely new and simple explanation of the phenomena of refraction through the prism, and summed up as follows:—

The experiments prove the homogeneity of the ether.

They prove the undulatory hypothesis, but oppose the undulatory theory.

They enable us to dispense with the different refrangibilities of the rays of light, as taught by Newton.

They help to explain many of the phenomena of what is called the polarization of light.

They give a new explanation of prismatic refraction, and explain in a plain and simple manner many very interesting natural phenomena.

Startling, he said, as these conclusions are to those who are conversant with the subject of light, he thought he was perfectly warranted in drawing them from his experiments. The general process of reasoning could not, however, be given in a short abstract.—*Ordinary Meeting, Oct. 4th, 1859, Manchester Literary and Phil. Society.*

TECHNICAL CHEMISTRY.

1. *Vegetable Parchment.—Papyrine.*—The interesting substance obtained in 1846 by Poumarède and L. Figuier (Comptes Rendus, xxiii, 918; see also this Journal xxviii, 431,) by immersing bibulous paper in partially diluted sulphuric acid—called *papyrine** by its discoverers—which with the exception of a few comparatively unimportant applications in France, where it was used for the shelves on which silk-worms are reared, &c., had excited scarcely any interest other than that naturally attaching to it as a chemical curiosity, until patented (Dec. 6, 1853) in England, by Gaine, (see *Rep. of Pat. Inv.* [E. S.] xxiv, 151) and manufactured by the well known house of De LaRue & Co., of London, has recently been investigated by Prof. A. W. Hofmann, (Ann. Ch. u. Pharm., Nov. 1859, cxii, 243; from a report to Messrs. Thos. De LaRue & Co.) In its prominent properties it resembles ordinary parchment very closely: indeed the two can hardly be distinguished from each other except on close inspection. Both exhibit the same peculiar pale, yellowish tint, the same degree of translucency, the same half fibrous, horn-like texture. Like animal parchment, the artificial product is not easily torn: it may be repeatedly bent or folded without exhibiting any special appearance of breaking in the creases formed. Like ordinary parchment it is extremely hygroscopic, and becomes more pliable by absorbing moisture. When wet with water it comports itself like untanned skins, swelling up to a slippery mass through which water cannot pass except by endosmose: the coherence of the substance is not at all impaired by thus soaking.

Vegetable parchment is best prepared by immersing unsized paper during a few seconds in oil-of-vitriol which has been diluted with half its volume of water, and immediately afterwards washing it in a dilute solution of ammonia; a thorough washing with pure water completing the process. Hofmann has ascertained by direct experiment that not less than one-fourth volume, or more than one-half volume, of water must be used with one volume of monohydrated sulphuric acid, in preparing the acid bath. The paper must not be immersed too long, nor should the temperature of the bath be higher than about 15° (C.)=[59° F.] A considerable amount of practice is moreover requisite before one can obtain a perfectly satisfactory product. When paper is transformed into vegetable parchment it undergoes no appreciable increase in weight. The action

* Should not this term, which has an undoubted right of priority, be preserved as the scientific name of the substance?—[F. H. S.]

of the sulphuric acid is purely molecular, the ultimate chemical composition of the paper—*cellulose*—remaining unchanged. [As already stated by Poumarède and Figuier *loc. cit.* and by J. Barlow, Proc. of the Royal Inst. 1857, ii. 411]. The result of the momentary action of sulphuric acid in this instance is comparable with that which a longer action of this acid upon woody fibre produces, viz.: formation of dextrine, a substance well known to be isomeric with cellulose. Indeed, the vegetable parchment may be regarded as a middle term between dextrine and cellulose.

The samples of parchment-paper examined by Hofmann [and by Barlow] contained no trace of free sulphuric acid; small portions of sulphate of lime and of sulphate of ammonia being the only soluble impurities present.

There is no apparent reason why the parchment-paper should not endure for an indefinite length of time. It is evident that if its destruction were dependent in any way upon the chemicals used in preparing it, decomposition would set in at once. Nothing of the kind occurs, however. Specimens of the factitious parchment which have been in Hofmann's possession during four years being undistinguishable from those recently prepared.

From experiments made in order to ascertain the strength of parchment-paper, as compared with that of true parchment and of unsized paper, it appeared that while strips of unsized paper broke when subjected to a weight of 15 or 16 pounds, similar strips of vegetable parchment supported 74 lbs., and those of ordinary parchment 75 lbs., before breaking. The cohesive force of unsized paper is thus increased five-fold by the treatment with sulphuric acid. It was also proved by experiment that for equal *weights* of the two substances parchment-paper exhibited about three-fourths the cohesive power of animal parchment. It also appeared that while the strength of strips of parchment-paper taken from different sheets was nearly constant, that of strips of animal parchment, even when cut from a single piece, was extremely variable, owing to the differences in thickness to which it is liable.

Parchment-paper although not quite so strong as ordinary parchment, is nevertheless more capable than the latter of withstanding the action of chemical agents, and especially of resisting the action of water; it may be left in this liquid for days, or even boiled in it, without undergoing any change, other than the increase of volume already alluded to, its original cohesion, and indeed all its properties being regained on drying. As is well known, animal parchment is soon converted into glue when boiled with water.

Since the parchment-paper contains no nitrogen, it is much less liable than ordinary parchment to putrefy when exposed to moisture, and will probably be less subject to the attacks of insects. Not only may the new parchment be substituted for that ordinarily employed for legal documents, &c.; but from its cheapness it will probably soon be used for ledgers and other important records—possibly for bank-notes—instead of the more perishable paper now employed. ["It will take the place of ordinary paper in school books, and other books exposed to constant wear." "It also promises to be of value for photographic purposes, and for artistic uses, in consequence of the manner in which it bears both oil and water-

color."—*Barlow.*] Its strength and power of resisting the action of moisture seem also specially to adapt it for the use of architects and engineers —particularly for working-plans liable to receive rough usage; also for the envelopes of letters and for cartridges. In thin leaves it affords an admirable tracing paper. As a material for binding books it will without doubt be extensively used. The ease with which it receives both printers' and ordinary writing ink is remarkable. For chemical laboratories it affords a most convenient material for fitting together retorts, condensers, and the like; while its power of resisting the fluids used in galvanic batteries suggests that it may be useful for diaphragms, &c. It is already used by tons, instead of bladder, as a covering for jars containing preserves, marmelades, etc.

Parchment-paper has been successfully manufactured on the great scale for a year or more by the firm of De LaRue, the numerous difficulties which presented themselves having been fully overcome by the perseverance of one of its members—the distinguished chemist Warren De La-Rue. [Specimen sheets of the parchment-paper accompany Hofmann's memoir.—F. H. S.].

2. *Weighing of Moist Precipitates;* by FERDINAND F. MAYER.—Mr. Ch. Mène, of Creusot,* gives a mode of weighing which does away to a great extent with the tediousness and difficulties attending the drying of many precipitates, especially in volumetric analysis. He washes the precipitate thoroughly by decantation, and then introduces it carefully into a bottle, the exact weight of which, when filled with distilled water at a certain temperature, is known. Since the precipitate is heavier than water, the bottle when filled again will weigh more than without the precipitate, and the difference between the two weights furnishes the means of calculating the weight of the precipitate.

In case the precipitate settles but slowly it may be collected on a filter, and together with the filter, after washing, be introduced into the bottle, in which case the weight of the filter and its specific gravity, supposing any difference should exist between its own and that of water, is to be taken in account. Precipitates soluble in or affected by water may be weighed in some other liquid.

This method, of which the above are the outlines, is spoken of in the Jahresbericht der Chemie for 1858† in rather disparaging terms, and I consider it not more than justice to the method, if not also to Mr. Mène, to prove its correctness, the more so as I have applied the principle on a large scale as far back as 1855.

I engaged in that year in the manufacture of carbonate of lead from refuse sulphate of lead, by treating the latter in a pulpy condition with carbonate of soda. The sulphate of lead I used contained very varying proportions of water and soluble impurities, from which latter it had first to be freed by washing. It was then in the state of a thin pulp, and the difficulty was to find the amount of dry sulphate of lead, as it was a matter of importance to use as little carbonate of soda, and to obtain as pure a carbonate of lead and sulphate of soda as possible. This could only be done by weighing it as a whole, or in portions; but as the

* Journal de Pharmacie et de Chemie, Oct. 1858.

† Jahresbericht der Chemie, by Will and Kopp, for 1858, p. 6.

drying of a tubful of sulphate of lead (from 500 to 1200 pounds) was impracticable, and sampling not less so, since the upper strata contained a much larger proportion of water than the lead at the bottom: I contrived the following method, which enabled me to leave the management of the process in the hands of a workman.

I took a strong oaken pail, weighing eight pounds when empty, and caused a black mark to be burnt in horizontally around the inside of the pail, two inches below the rim, up to which mark it held twenty pounds of water. I reasoned as follows: The specific gravity of sulphate of lead being 6·3, the pail if filled up to the mark would hold 126 pounds of pure sulphate of lead. The specific gravity of water being 5·3 less than that of sulphate of lead, it followed that if there was one pound of water in the pailfull of moist sulphate, the pail would weigh 5·3 pounds less than 126 (+8, the tare of the pail) =120·7 (+8); if there were two pounds of water present, the weight would be 115·4 (+8), and so on. This enabled me to calculate a table, giving in one column the actual weight of the pail when filled with moist sulphate, and opposite in a second column, the amount of dry sulphate corresponding to the gross weight. The weight of dry sulphate was thus found as accurately as could be desired, although the amounts varied in practice from 30 to 105 pounds.

It is nothing but an application of the Archimedean theorem, that, when a solid body is immersed in a liquid it loses a portion of its weight, equal to the weight of the fluid which it displaces, or to the weight of its own bulk of the liquid.

This, as I suppose, is precisely the principle applied by Mr. Mène. *The precipitate he obtains by a certain chemical manipulation is a substance of known composition and specific gravity.* Supposing it to be sulphate of lead, and the bottle, when filled with water at the normal temperature, to weigh 70 grammes =50 grammes of water, and 20 for tare. After introducing the precipitate and filling again with water it weighed 71·06 grammes. Now, as the specific gravity of sulphate of lead is 6·3, or as the weight of a cubic measure of sulphate of lead is 6·3 times that of a cubic measure of water, and as the space of one part by weight of water is taken up by 6·3 parts by weight of sulphate of lead, it follows that the quantity of sulphate of lead in the bottle, which has taken up the space of one part by weight of water, increases the original weight of the bottle (filled with pure water) by 5·3. To find the amount of water displaced it is only necessary to divide the overweight (1·06 grammes) by 5·3=0·2, which, added to the overweight 1·06+0·2 gives 1·26 grammes as the weight of the precipitate.

Hence the rule, which is of great convenience in volumetric analysis, that *to find the weight of a moist precipitate, which is a compound of known specific gravity, weigh it in a specific gravity bottle or some other vessel of known weight when filled with water, or any other liquid, at the normal temperature, again fill it with the water or other liquid, divide the excess of the new weight by the specific gravity of the substance, less that of the water or other liquid (that of water being =1) and add the quotient to the overweight, which gives the weight of the precipitate.*

The editor of the Jahresbericht appears to have overlooked the fact that the precipitates weighed in this manner are definite compounds, the specific gravity of which is well ascertained.

The principle I have exemplified above may not be novel; but as I have never met with it, chemists, as well as manufacturers (especially of colors), will probably also find it of interest, and certainly highly practicable and easy of execution.

36 Beekman street, New York, Feb. 3d, 1860.

3. *New Chemical Journal.*—The Chemical News (with which is incorporated the Chemical Gazette), edited by WILLIAM CROOKES. London: Weekly. Price 3*d*., stamped 4*d*. 8vo. 12 p. each number. This new Journal commenced on the 10th of December last, and eight numbers have already reached us. The contents are divided under Scientific and Analytical Chemistry, Technical Chemistry, Pharmacy, Toxicology, &c., Proceedings of Societies, Notices of Patents, Correspondence, Scientific Notes and Queries, Laboratory Memoranda, Miscellanies, and Answers to Correspondents. Mr. Crookes is favorably known by several valuable researches, and thus far has shown good judgment and spirit as an editor. His verbatim reports of the late lectures of Dr. Faraday (the Holiday lectures) at the Royal Institution, attest his appreciation of the true sources of vitality for such a journal.

4. *American Druggists' Circular and Chemical Gazette;* N. Y. Feb. 1860. 4to.—Although chiefly special and wholly technical in its objects, this Journal (which has now reached its 4th volume, whole number 38) is conducted by Mr. Mayer and others, in a manner to entitle it to rank as a valuable coadjutor in technical chemistry.

II. GEOLOGY.

1. *On some of the Igneous Rocks of Canada;* by T. STERRY HUNT, F.R.S. (In a letter to one of the editors, dated Jan. 1860.)—There occurs in the district of Montreal a series of isolated hills running nearly east and west for a distance of ninety miles along the line of an undulation which has disturbed the lower Silurian strata. These hills, which often cover considerable areas, consist of igneous rocks which have apparently been solidified under a considerable pressure, and have subsequently been exposed by the denuding action which has removed from around them the soft and unaltered palæozoic strata. The names of these mountains counting from the west are Rigaud, Mount Royal, Montarville, Belœil, Rougemont, Yamaska, Shefford and Brome, to which we may add Monnoir a similar mass lying somewhat to the south of Belœil.

I am now engaged in the study of the various rocks composing these mountains, which offer great diversities in lithological character and composition. Prominent among them we may mention the trachytes, which in their various types of compact, granular, porphyritic and granitoid are abundant. The mountains of Brome and Shefford appear to be made up entirely of a granitoid trachyte, which consists of crystalline orthoclase, without quartz, and with small portions of hornblende or mica, sphene and magnetite. The orthoclase in a great number of these rocks which I have analyzed contains like sanidin a large proportion of soda. The other varieties of trachyte which occur in veins and dykes often contain a portion of carbonates amounting to from 6·0 to 18·0 p. c. and consisting chiefly of carbonate of lime with some magnesia and

iron. Some of these rocks pass into phonolites through the admixture of a silicate which gelatinizes with acids and has the composition of natrolite. This mineral in one case amounted to more than 40·0 p. c. of the rock, the remainder being orthoclase with a small amount of carbonates.

A large part of the mountain of Yamaska consists of a coarsely crystalline diorite the feldspar of which approaches anorthite in composition, while an apparently similar diorite, which makes up the mass of Monnoir, contains oligoclase in large crystals. Other diorites from this series contain labradorite; mica and sphene, in small quantities, are often present.

Dolerites are also abundant, and sometimes pass, owing to a scarcity of feldspar, into an augite rock, generally with ilmenite and magnetite. A fine-grained dolerite from Rougemont contains abundance of crystallized olivine, and a large part of Montarvilles consists of a remarkable granitoid rock, made up of a crystalline feldspar, in some parts at least labradorite, with sparsely disseminated crystals of black augite, a little brown mica, and a great abundance of crystals of honey-yellow olivine, which amount to more than 45·0 p. c. of the mass. The composition of this olivine I have found to be silica 37·17, magnesia 39·68, protoxyd of iron 22·54 = 99·39.

Many of these diorites and dolerites, except in their lithological structure, closely resemble the stratified rocks made up of anorthic feldspar, with hornblende and pyroxene, and containing magnetite and ilmenite, which are so abundant in the Laurentian system, suggesting the notion that the intrusive masses may be nothing more than these stratified rocks displaced and injected among the Palæozoic strata. Durocher has already pointed out a similar resemblance between the intrusive rocks of some parts of Scandinavia, and the subjacent gneiss.—(*Bul. Soc. Geol. France*, [2] vi, 33.)

The granitoid trachytes as well as the dolerites, diorites and peridotite (olivenite rock) make up mountain masses, while the earthy and porphyritic trachytes and the phonolites are generally found cutting the above, and the adjacent strata. The absence of quartz or of any excess of silica from all these rocks is a remarkable feature. Farther to the east, however, intrusive granites are very abundant; these penetrate the Devonian strata but are older than the carboniferous. Quartziferous plutonic rocks are also abundant in the county of Grenville, where they penetrate the Laurentian series. These plutonic rocks consist of dolerites, syenites and eurites, which are in their turn cut by dykes of very beautiful porphyries. The base of these is jasper-like, black, red or green in color, and encloses crystals of red orthoclase and occasional grains of quartz. The analysis of the base shows it to consist of the elements of orthoclase with an excess of silica and a little oxyd of iron. The syenites are cut by large veins of chert, and in the vicinity of these have been changed into a sort of kaolin from a decomposition of the feldspar, which may have been the source of the silicious accumulations. This group of igneous rocks, which is overlaid by the Potsdam sandstone is very unlike those which we find penetrating the palæozoic strata.

The details of my investigations on these rocks, so far as completed, will be found in the Report of the Geological survey of Canada, for the last year, now in press; a first portion has already appeared in the Report for 1853–56, p. 485. It is by the systematic study of different series of igneous rocks that we may hope to arrive at just notions as to their origin, their mode of formation and their relations to metamorphic sedimentary rocks.

2. *Notes on the Dolomites of the Paris Basin, etc.;* by T. Sterry Hunt, F.R.S. (In a letter to one of the Editors, dated Montreal, Feb. 3, 1860.)—The gypsums of the Paris basin are evidently not of epigenic origin but regularly stratified and alternating with marls and limestones. In September, 1855, I visited with Elie de Beaumont and several members of the Geological Society of France, the gypsum quarries at the hill of Chaumont, and there insisted upon the views which I have since urged in this Journal (vol. xxviii, p. 365) upon the different origins of gypsum. For a report of my remarks on that occasion see the *Bulletin de la Société Geologique de France*, [2], xii, 1306.

I have subsequently shown in the memoir just cited in the last volume of this Journal that the formation of these stratified gypsums by the double decomposition of bicarbonate of lime and sulphate of magnesia involves the production of carbonate of magnesia, which unless carried away or decomposed by an irruption of sea water will be found to overlie the gypsums forming the dolomite which is their common associate. The presence of carbonate of magnesia in the gypsiferous series of the Paris basin has hitherto been unnoticed, and having at the time above mentioned collected specimens from the quarries at Chaumont I was recently induced to examine the so-called *white marls* which overlie the gypsiferous series, and find them to be magnesian. The analyses of two specimens, one penetrated by seams of gypsum, gave each about 60·0 per cent of dolomite mingled with clay. The Paris gypsums then offer no exception to the general rule.

Beneath the gypsiferous series and in the lacustrine group known as the lower travertine, or St. Owen limestone, occur beds of a whitish, very fissile, shaly matter, enclosing concretions of menilite (opal), and consisting of a hydrated silicate of magnesia, identical with meerschaum or quincite in composition, intermingled with small portions of earthy carbonates. I have examined a specimen of this mineral which I collected near Paris and find it to be the same with that described by Dufrénoy and Berthier as occurring in similar positions in various other localities. This appearance of beds of a silicate of magnesia approaching talc in composition, in the midst of unaltered deposits, is interesting inasmuch as it seems to show that such silicates may be formed in basins at the earth's surface by the reactions between magnesian solutions and dissolved silica. I have many years since described the existence of similar silicates among the deposits during the artificial evaporations of natural alkaline waters, and farther inquiries in this direction may show us to what extent certain rocks consisting of calcareous and magnesian silicates may be directly formed in the moist way.

I propose to send you very soon a supplement to my paper on Gypsums and Magnesian rocks, describing some recent experiments which

confirm what I have already announced that no dolomite is formed in the experiment of Von Morlot and Haidinger, recently resuscitated by Charles Deville, and appealed to by Prof. Phillips in his last Annual Address to the Geological Society of London, as resolving the problem of the origin of dolomite. This double salt is however readily formed when a mixture of the moist amorphous carbonates (such as is obtained by precipitating in the cold by an excess of carbonate of soda a solution of the chlorids of calcium and magnesium in equivalent proportions), is gradually heated under pressure.

3. *New Palæozic Fossils;* by J. H. McChesney. Chicago, 1859. 8vo. pp. 64. In this publication the following species from the Carboniferous rocks of the Western States are noticed as newly described:

Crinoidea.—*Platycrinus ornogranulus, P. inornatus, Scaphiocrinus longidactylus, Zeacrinus bifurcatus, Z. mucrospinus, Actinocrinus asterius, A. tenuisculptus, A. subæqualis, A. Fosteri, A. subventricosus, A. urnæformis, A. Hurdianus, A. æquibrachiatus, A. Andrewsianus, A. Hageri, Forbesiocrinus Pratteni.*

Brachiopoda.—*Orthis, Kaskaskiensis, O. Lasallensis, O. Pratteni, O. Richmonda, Productus asperus, P. symmetricus, P. Wilberanus, P. tubulospinus, P. fasciculatus, P. inflatus, P. pileiformis, Ambocelia gemmula, Spirifer transversa, S. subelliptica, S. perplexea, S. subventricosa, Retzia subglobosa, Athyris spiriferoides, A. orbicularis, A. differentius, Terebratula inornata, Rhynchonella Eatoniæformis, R. explanata, R. carbonaria, R. Algeri, Trematospira Mathewsoni, Discina caputiformia.*

Lamellibranchiata.—*Leda Oweni, L. gibbosa, L. polita, Nucula parva, N. cylindricus, N. rectangula, Astartella varica, Edmondia concentrica, Allorisma clavata, A. sinuata, Myalina Swallovi, Nuculites Vaseyana, Pinna Adamsi, Syringopora multattenuata, Cyathoxona prolifera.*

Gasteropoda.—*Bellerophon ellipticus, B. vittatus, B. Blanyana, B. Stevensiana, Pleurotomaria Beckwithana, P. nodomarginata, Natica Shumardi, Platyceras crytolites, Platyostoma Peoriensis, Bucania Chicagoensis.*

Cephalopoda.—*Nautilus Forbesianus, N. Illinoiensis, N. quadrangulus, N. nodocarinatus, Gonitites Hathawana, Cyrtoceras (Lituites?) giganteum, Trochoceras Desplaniensis, Orthoceras Rushensis, O. Knoxensis.*

The typography of this brochure is very good, and the descriptions of the species show that the author has a wide acquaintance with the Carboniferous fauna of the West. We think, however, that the science would present to the student a much less formidable array of difficulties, if Palæontologists would on all occasions give accurate measurements of the species they describe. Throughout this book, for instance, the size of the individual is recognized as a specific character, and yet as no dimensions are given it must be impossible for even the most experienced practical naturalist to decide whether *O. Lasallensis, O. Richmonda* and *O. Pratteni* are small or large forms; whether two lines or two inches wide. The first mentioned of these species is also said to have the "*surface marked by sharp rugose radiating striæ increased by implantation,*" but so have a great many other *Orthides*, and until some standard for comparison is furnished, all the students of this book, except those who may have access to the original specimens, must remain in doubt upon the question as to whether there are ten or twenty striæ in the width

of one line. We find it stated further that in *O. Richmonda* the striæ are "finer and less rugose" than they are in *O. Lasallensis* while in *O. Pratteni* they "are not so distant and not so rough," as in either of the other two, but as we have no means of ascertaining their size in the first named species, which is here made the standard, all that relates to the surface characters of the second and third might have been left out without detracting anything from the value of the description. No doubt all this appeared to be sufficiently clear to the author while he was engaged with the specimens before him, but we think upon a little reflection he will agree with us, that without some clue to the size and surface characters of these species, they cannot be identified. Where a shell is said to be smaller or larger than some other species which has been described in some other book, the difficulty would be somewhat less, provided that access could be had to that book, and even then, such questions as "how much smaller?" or "how much larger?" cannot be decided. By adding two or three lines to their descriptions, palæontologists may add a vast deal to the value of their labors, and save others engaged in the same pursuits much perplexing, and too often fruitless, intellectual toil. It is not always possible to give figures of new species, but it is easy to furnish measurements. The absence of these is the great defect in the work before us, and it is a defect that may be observed in books of much greater pretensions. We notice a new genus, (AMBOCELIA) among the Brachiopoda, which is said to have been "recently established by Prof. James Hall," in the "Regents' Report of the State of New York for the present year," (meaning 1859). Has this report been published? If not, then the genus is not established. No author can establish a genus before he has published his description, and, even then, he may not succeed in shewing that it is new.

The practice of antedating genera and species should be discountenanced, as it cannot be beneficial to science. Where specific names are derived from names of persons or places, the initial should be a capital letter, a rule which has been of late much disregarded.

4. *Explorations in Nebraska.*—Dr. F. V. HAYDEN, in a letter to Prof. DANA, dated Deer Creek, Nebraska, Dec. 1, 1859, states, after speaking of the interest of the region, that as soon as the grass is sufficiently high this coming Spring, and the swollen streams permit the passage of pack trains (which cannot be sooner than the first of June), Capt. Reynolds proposes to divide his party into two divisions—one party in charge of Lieutenant Maynadier, Asst., will pass up the Wind River Valley along the western side of the Big Horn Mountains. The other division under Capt. Reynolds will proceed up the Platte to the South Pass, exploring the Wind River Mountains—the two parties to spend the 4th of July at Clark's Pass. Here Lt. M.'s party will proceed down the Yellowstone to Fort Union, and Capt. R.'s party will reach the same point by crossing at the head waters of the Missouri and descending that stream, and thence to the States as soon as possible. Such is the programme of explorations; it may be slightly varied, but in any case will traverse a country very interesting for geology.

Dr. Hayden, in addition to his geological labors, has since 1852 interested himself in ethnographical studies, and will have by the end of this trip material enough for a large volume on this subject.

5. *Geological Surveys of South Carolina and Kentucky.—The geological survey of South Carolina* has unfortunately been stopped by the unwise action of the legislature of that State at its last session. Dr. O. M. Lieber, late the State Geologist, is occupied in preparing his final report, which will embrace Anderson and Abbeville district, and a part of Edgerfield. Dorn's gold mine is described in it.

The geological survey of Kentucky is continued with unabated zeal.* We have received the Synoptical Report for the past year—a brochure of 50 pages—by Dr. D. D. Owen, principal, aided by S. S. Lyon and Jos. Lesley, Jr., Topographical Assistants, Leo Lesquereux, Palæontological Assistant, and Dr. Robert Peter, Chemist. Dr. Owen here expresses the opinion that "the report of Mr. Lesquereux of the last season's work (now completed), is by far the most practically useful geological report on this subject (coal), which has ever appeared, not only in the United States but in any part of Europe."

In his synopsis of this forthcoming report, Mr. Lesquereux says, "The third section of the report contains a short comparison of the distribution, geologically and geographically, of the coal strata in Kentucky, Ohio and Pennsylvania. This comparison is of high scientific interest, as it fixes the general distribution of the coal strata in the whole extent of the coal basins of the United States, and cannot but give to the geological reports of Kentucky a great value as containing the key of the general distribution of the coal. Henceforth all the reports treating of the distribution of coal strata will naturally take their guide and standard of comparison from the section in the Kentucky coal-fields."

6. *First report of Progress of the Geological and Agricultural Survey of Texas;* by B. F. Shumard, M.D., State Geologist. Austin, Texas, 1859. pp. 17.—We learn from this brochure that satisfactory progress has been made in the preliminary reconnaissance of the vast territory (237,500 square miles) included within the State of Texas. Dr. Shumard states that a more complete series of the geological formations exists in Texas than in any State of the Union, ranging from the Potsdam sandstone to the latest Tertiary. He has made no less than *seven* lines of section extending in various directions a collective distance of 1220 miles, determining his levels by the barometer. He has found time also for minute and final surveys of eleven counties and partial surveys of several others. The coal measures cover an area not less than four thousand or five thousand square miles, with a thickness of eight or nine feet of coal in about 300 feet of coal rocks. The coal is good in quality. Extensive beds of brown coal also occur in the Tertiary rocks in the eastern and middle portions of the State.

The fossil forms of the various strata in Texas are very abundant, and Dr. Shumard informs us that his collection is already very rich.

From the head waters of the Brazos river he has been fortunate enough to obtain a fine mass of meteoric iron weighing about 320 pounds, and a smaller mass of the same kind from Denton county, specimens of which have reached us.

* Notwithstanding the cramped appropriation of money for the two years past —two-thirds of the whole sum being consumed by expenses of publication, which is an absurdity unworthy of the spirited state of Kentucky.

7. *Post-pleiocene Fossils of South Carolina;* by FRANCIS S. HOLMES, A.M., &c. Nos. 6 to 10 inclusive, containing plates 11 to 20 inclusive. Quarto. Charleston, S. C., 1859; Russell & Ivens.—This beautiful monograph, previously noticed,* is continued in the same excellent manner in the numbers now received. The description and figures of the mollusca end with the part containing Nos. 6 and 7, and with Nos. 8, 9, and 10 commences a "Description of Vertebrate Fossils, by Prof. Jos. LEIDY." The student will refer to this memoir with particular interest at the present moment, when so much attention is being given to the occurrence of human reliquiæ with the remains of animals heretofore judged to be extinct before the human epoch. The Eocene and Post-pleiocene beds on the Ashley River are exposed to the wash of the water, and "the fossils washed from them form part of the shingle on the shore, and here become mingled with the remains of recent indigenous and domestic animals, together with objects of human art." Of those vertebrate remains actually obtained in excavations of the Post-pleiocene and Eocene formations, more confidence is felt in determining the actual age to which they belong. Both the collections submitted to Dr. Leidy by Prof. Holmes and Capt. Bowman, contain remains of the horse, ox, sheep, hog and dog, which we feel strongly persuaded, with the exception of many of those of the first mentioned genus, are of recent date, and have become intermingled with the true fossils of the Post-pleiocene and Eocene periods on the Ashley River and its tributaries. In regard to the remains of the horse from the facts related (in this memoir) we think it must be conceded that several species of this animal inhabited the country of the United States during the Post-pleiocene period, contemporaneously with the mastodon, the giant sloth, and the great broad-footed bison."—pp. 99–100.

8. *Assiniboine and Saskatchewan Exploring Expedition;* by HENRY YOULE HIND, M.A., Prof. of Chemistry and Geology in Univ. of Trinity Coll., Toronto. Toronto, 1859. 4to, pp. 202, with many maps, sections and plates.—This valuable Report comes to hand at too late an hour to enable us to do more than give its title, deferring to our next an analysis of its contents.

9. *Geology for Teachers, Classes, and Private Students;* by SANBORN TENNEY, A.M., Lecturer on Physical Geography and Natural History in the Mass. Teachers' Institutes. 12mo, pp. 311. Philadelphia, 1860.—As the scope of this little work comprises the whole range of geological phenomena, it gives necessarily a very concise account of the various departments included. The matter however seems in most cases well selected, and shows the author to be acquainted with his subject. The book is illustrated by some 200 good wood-cuts, and has one excellent feature;—that the illustrations are taken, whenever possible, from *American* objects, which cannot be said of most geological text-books previously published in this country, the authors of which have generally gone to Europe for their examples. American geology should be the main subject of American text-books, and it is pleasant to see a step taken in the right direction. Our author however is not quite up to date in some instances, as in ignoring our Permian and western Jurassic beds; in including the Lias in the Oölitic system, etc. W. S.

* This Journal, xxvii. 156.

III. ZOOLOGY.

1. *On Botanical and Zoological Nomenclature;* by Wm. Stimpson.—A more careful attention to the subject of nomenclature is urgently demanded of the followers of all branches of Natural History. It is a subject to which too little attention has been paid in an abstract or general sense, and too much perhaps in particular cases. A comprehensive code of rules, recognised by the authority of the greater lights of science, has been always needed. This was attempted during the last century by Linnæus and Illiger, and in 1842 "Rules of Nomenclature" were drawn up by the British Association, and ratified by the American Association in 1845. These are excellent as far as they go, but need much extension and many additions, as any one may observe who attempts to decide by them all questions which occur in his experience.

On the other hand, in particular cases of species and genera, the discussion of questions of nomenclature has reached such a pitch that it is no uncommon thing to see the greater part of a new zoological work devoted to synonymy. One author, after six pages of historical and synonymical matter, evincing great critical acumen and much bibliographical research, will arrive at what appears to him to be a certain and final conclusion that the true *Orthonymus aliquis* is such and such a species. The next writer who succeeds him in the same field will triumphantly prove in *ten* pages that it is not that species at all, but the *O. neminis.* And so on to the end of the chapter, if it ever will have an end, which is doubtful unless some decided action is soon taken by naturalists for the purging of their favorite science from this opprobrium. After all the pages which have been written upon some of these cases we seem no nearer to a settlement than at first. The difficulty increases rather than diminishes,—each succeeding author putting forth views differing from those of his predecessors. All this discussion, let us bear in mind, is merely preliminary, and for the purpose of indicating with certainty an object about which the author has perhaps not a dozen words to say.

Now it may appear at the first glance that the application of the law of priority is exceedingly simple. The name given by the first describer of a genus or species is to be respected, and applied to that genus or species throughout all time. But as soon as we come to apply this rule, we find cases without number in which complications occur, rendering limitations of the law necessary. Genera are to be subdivided, and are subdivided with different limits by different authors; the species of one are found by another to include two or three distinct forms, and so on. Some of the limitations of the law of priority have been laid down in the "Rules" of the British Association, but not enough to enable us to decide half the cases which may arise,—leaving the remainder subject to the whims or dependent upon the extent of the knowledge of the author who would follow them.

In applying the great law, the most difficult question of all immediately arises,—What constitutes a description? or, When has an author so designated his species that his name for it should hold? On this subject we have every variety of opinion, from that of the German ornithologists, who consider that a simple published name, referring to a specimen

in a museum, is sufficient, to that of the lamented Edward Forbes, who once insisted that no name proposed should be accepted unless accompanied by a *Latin* description or an illustrative figure. The first opinion we believe to be scouted by nine-tenths of living naturalists;—the second appears to be too stringent, as an author can of course write better in his own language than in any other, though we doubt if a description appearing in Chinese would gain the least notice from modern naturalists.

The question, "What constitutes a description," can never be decidedly answered. No rule can be proposed which is universally applicable. With regard to its *length;*—we may say that two words are not sufficient, an hundred are; but where shall we draw the line? The two sentences of one author may be better than the two pages of another. One writer will describe an object well except in one point, in which from defective observation, a character is represented in exact opposition to the true state of the case. Some descriptions are sufficient to enable the naturalists of one country, from their collateral knowledge, to determine a species, while those of another country or continent would be left entirely in the dark. An author may publish descriptions in a work for private distribution, which will be inaccessible to the great body of naturalists. We might fill many pages with such cases as these, and yet, were rules made out applicable to each, there would still be cases constantly arising which could be decided by none of them. How then can the matter be settled in these latter instances? We will suggest a method further on.

It will be observed that it is among the more common and earliest-described species that the synonymic heap is greatest. This is exceedingly embarrassing to the student, who in general has occasion to use these very species, being those most easily accessible, in the course of his studies. He may find in a dozen different books the characters, anatomical or otherwise, of what appear to him a dozen different objects, since the names used may be different, and elementary works cannot be expected to go into synonymical details. At the present day, thanks to the advance of knowledge and precision, and the international exchange of scientific works, the name of an entirely new genus or species may escape the burden to which that of older species is subjected. It is with those published in the last century that the greatest trouble occurs. Investigators among antique and forgotten books are constantly finding some obscure work or paper, perhaps scarcely known out of its immediate vicinity even at the time it was published, in which names occur which must be adopted, in the opinion of some, to the exclusion of the familiar titles which have been used for half a century. The disinterment of Klein's name *Cyclas* is an instance of this. How strange it must seem to a conchologist of the present day to be obliged to designate the common marine *Lucina* by a name which has been in use seventy years for a freshwater bivalve, while this freshwater bivalve becomes *Sphærium;* and to use *Cyclostoma* for *Delphinula*, *Terebellum* for *Turritella*, etc. The restoration by G. R. Gray of Boddaert's names in ornithology is another instance. By the discovery of a meagre pamphlet of the eighteenth century, only two or three copies of which now exist, we find ourselves forced to change the generic names of common birds, familiar as they are by long and constant usage.

In the discussion of these questions all personal considerations should be entirely rejected. The smallest interest or convenience to the science in general, followed as it is by a republic of thousands, is of more importance than any compliment to the feelings of a living, or the memory of a deceased naturalist. In fact our mere recognition of an author's names is not of such vast importance to his reputation. His fame must rest upon a securer foundation than this. For the custom of placing the name of an author after a species described by him is not (or should not be) done for that author's personal advantage, but simply to assist us in the recognition of that species. It is a short method of referring to the place where the description of the species may be found, or enables us to distinguish it from some other to which the same name has been by mistake applied; as, *Pleurotoma violacea*, Hinds, *non* Mighels. In this view, how ludicrous it appears, to hear, as we often do, naturalists complain that if the custom of placing after a species the name of that author who first placed it in its proper genus is adhered to, more than one-half of Linné's species will be wrested from him. Does the fame of the great Linnæus depend upon the number of species he described?

We will now mention a few points concerning which great difference of opinion exists in the minds of naturalists, and which for the good of science should be immediately settled in one way or the other. The first is: shall the same generic name be allowed to occur in different departments of zoology or botany, or even in both these, or, we may add, in other sciences. Many are of the opinion that they may be used, and should not be changed, if so occurring;—in view of the great difficulty now experienced in selecting a name which is not preoccupied, and shall be at the same time descriptive or suggestive of the object intended. But what is the object of a name? Surely, the *main* object is to enable us to distinguish one thing from another, and from all others, that when it is used we may know what is intended, and not be forced to decide by other aids. Is it not of vastly more importance that a name should serve this purpose, than that it should remotely indicate (which is the most generally possible) some character of the object, which it may after all hold in common with an hundred others? Greek compounds are by no means exhausted yet, and if they were, we might fall back upon euphonic names, which serve the purpose however barbarous they may appear in the eyes of some. The custom of using the same name for many diverse objects is productive of serious inconveniences. If we have stars, countries, minerals, plants, vertebrates, articulates, mollusks and radiates, all named alike, some singular anomalies might occur, since we can of course reduplicate *specific* appellations as often as we please in different genera. For instance, suppose a travelling naturalist "making his researches in Arizona, observed specimens of the *Arizona pœtula* (hermit-crab) inhabiting the shell of *Arizona pœtula* (univalve), creeping among the roots of *Arizona pœtula* (shrub); and upon examining it anatomically, found great numbers of the *Arizona pœtula* (infusorium) living in its gills. The *Arizona pœtula* (bird) was feeding upon these crabs with great voracity," etc.

Another point. A genus may contain a vast number of species, and yet from want of profound investigations no one may see the propriety

of dividing it up. As occurs very commonly, in the course of time some new species belonging to it are described under names, which, being preoccupied in that genus, are very properly changed. The new designations become established and may be used for years. At last it becomes necessary to divide the genus, and the species whose names have been referred to are found to belong to different genera. Shall the old reduplicated specific name, or the substituted one be now adhered to? Naturalists are about equally divided in opinion upon this point.

The propriety of using small initial letters to proper specific names, nouns or adjectives, has been made the subject of discussion. Whatever method be followed here, it would seem that uniformity is desirable; if any of these proper names are to have small initials, why not all? Most zoologists and botanists seem in this matter to follow the usage of their own language rather than that of the Latin, or any uniform system. The Germans will have all nouns begin with a capital, and all adjectives with a small letter, as *Ocypode Cursor*, *Chiton emersonianus*, whereas the English write common nouns with a small initial, and all *proper* appellations, whether nouns or adjectives, with a capital, as *Ocypode cursor*, *Chiton Emersonianus*. The truly convenient system will be to write all specific names without exception, with a small initial letter, as is done by one of the most eminent zoologists of this country, and by many of those of Europe. We shall then have no difficulty in distinguishing specific from generic names, and may discuss the relations of species without the necessity of repeating the generic name or its initial every time they are mentioned. A proper name, modified for use as a specific appellation, becomes a part of a new title, and involves a different idea.

We will not detain the reader by discussing other mooted points, as whether ante-Linnæan names shall be accepted, if binomial; whether names of faulty etymology shall be corrected, etc. The above are only mentioned as instances of the necessity of establishing many rules to produce uniformity of usage among naturalists. In pointing out how this may be satisfactorily done, we proceed to our promised suggestion.

We have somewhere read, that when any orthographical or other difficulty occurs in the use of the French language, the case is immediately referred,—in accordance with the admirable *system* for which the nation are remarkable both in science and literature,—to the Academy, who decide upon it, arbitrarily it may be, but finally. The action of this tribunal is respected, and no farther uncertainty or diversity in the use or spelling of the word can occur to embarrass French authors. Now why may not a similar mode of action be of use in science, and enable us at last to settle all our difficulties. Science is cosmopolitan, not national. Let a convention meet at Paris or some other central point, composed of delegates from all the scientific societies of the earth, and representing at least all the departments of zoology and botany. Here they may hold sessions of the entire body, for a sufficient length of time to establish all the general rules of nomenclature which can be conveniently applied. But as we have before observed, there are some particular cases for which no rule will serve, and which must in some way be decided separately and *arbitrarily*. For the settlement of these cases let the convention divide itself into as many sections or committees as there are classes of

plants and animals; a committee of ornithologists for the birds, of entomologists for the insects, etc. These committees being composed of experts in the various branches, will not find it difficult to discuss and pass judgment by vote upon the name of each contested genus or species. Let these decisions be respected, and let the names of those who will not abide by them be placed upon a new edition of that black-book which LINNÆUS kept of old,—the list of *Damnati!*

2. *Les genres Loriope et Peltogaster*, H. Rathke; par W. LILJEBORG, Professeur de Zoologie à Upsala en Svède. pp. 35, 4to, pl. 3. (Extr. des Nova Acta Reg. Soc. Sc. Upsal, Ser. 3, Vol. iii.)—M. Liljeborg has unraveled a singular history in relation to the curious sac-like parasites found on the abdomens of Decapod crustaceans. They were first observed by Cavolini, and in more recent times by Rathke, who considered them to be worms, probably Entozoa, and instituted the genus *Peltogaster* for their reception, describing two species, *P. paguri* and *P. carcini*. Diesing placed them among his Myzelmintha, and formed a new genus, *Pachybdella*, for *P. carcini*. These forms were made the subject of discussion by Kroyer, Steenstrup, O. Schmidt, and Lindstrom, who agreed with Cavolini in referring them to the Crustacea, but could only conjecture their more intimate relations, although suggesting those with the Entomostraca, the Lerneidæ, or the Bopyridæ. From a study of their larvæ M. Liljeborg now ascertains their true place to be among the Cirripedes, and describes two new species.

Within the body of *Peltogaster paguri*, Rathke found a minute Tetradecapod, scarcely a line in length, which he considered to have been swallowed as food by that animal, and described it as an Amphipod under the name of *Loriope*. It was afterward referred to the Tanaidæ by Dana, who described a new allied genus *Cryptothir*. Its history however remained very obscure, although it was demonstrated that, being always found alive, it was not the food of the *Peltogaster*, and some naturalists even suspected it to be the male of that parasite. By a fortunate discovery M. Liljeborg has now cleared up the difficulty. In examining a *Peltogaster* taken from the abdomen of *Pagurus pubescens*, he found attached to it another sac-like body filled with Loriopes, which might well have been taken for the egg-pouch of the *Peltogaster* itself, but which after careful study proved to be a distinct animal,—a parasite upon a parasite! It proved, in fact, to be the female of the *Loriope*, grown monstrous by a process of degradation similar to that observed in the female of the *Bopyridæ*, to which family indeed the *Loriope* must now be referred. The occurrence of the young Loriopes in the digestive cavity of *Peltogaster* is, then, simply adventitious.

3. *Neue Wirbellose Thiere, beobachtet und gesammelt auf einer Reise um die Erde*, 1853 bis 1857, von LUDWIG K. SCHMARDA; 1st Band, 1st Hälfte, 4to, pp. 66, and 15 colored plates. Leipzig, 1859. (New York, B. Westermann & Co.)—A quarto volume, handsomely bound and splendidly illustrated, containing descriptions of the Turbellaria and Rotatoria collected and observed by Dr. Schmarda during a voyage round the world. The descriptions of the animals belonging to the former order are made additionally clear by woodcuts showing the shape of the head and the distribution of the ocelli. A review is given of the genera of

each order. The Dendrocœla are divided into two divisions, *Acarena* and *Carenota*, the former having no distinct head, which those of the latter group possess. We should judge this to be a character of much less importance than those derived from anatomical characters, which forbid such a division. Following it, the author separates *Planaria gonocephala* from the other freshwater Planariæ and places it with such forms as *Cephalolepta* and *Planaria oceanica*, Darwin! There is also a large number of genera hitherto considered sufficiently well established, which are entirely ignored by M. Schmarda, as *Prosthiostomum*, *Dendrocœlum*, *Procerodes*, *Fovia*, *Bdelloura*, *Geoplana*, and *Rhynchodemus*. The new genera of Dendrocœla are *Dicelis*, *Prosthecerœus*, *Homalocerœus*, *Goniocarena*, *Carenocerœus*, and *Sphyrocephalus*. Of these several have been previously established under different names. *Prosthecerœus* may be adopted, as Quatrefages' name *Proceros* is preoccupied. *Goniocarena* is *Dugesia* Girard, *Carenocerœus* is *Nautiloplana* Stm., and *Sphyrocephalus* is *Bipalium*.

The Nemertinea are subdivided upon more certain grounds than the preceding order, but we are at a loss to understand why the proboscis should not be considered as the mouth, as it is certainly the aperture through which food is introduced into the body. Oken's name *Borlasia* is adopted (although it is exactly synonymous with *Lineus*, Sow.) and made to include *Amphiporus*, *Acrostomum* and *Baseodiscus* of Diesing. A new genus, *Loxorrhochma*, is established for the *Polia coronata* of Quatrefages. The figures are excellent, showing well the colors, which among these animals form the most reliable specific characters, and notwithstanding errors of nomenclature and arrangement, we have to thank M. Schmarda for an exceedingly interesting and beautiful work. W. S.

4. *A Supplement to the "Terrestrial Air-breathing Mollusks of the United States and the Adjacent Territories of North America;"* by W. G. Binney. Boston, 1859. 8vo, pp. 207, and 6 colored plates. (Extracted from the Journal of the Boston Society of Natural History.)—This much needed supplement to Dr. Binney's great work contains the additions and corrections which have accumulated in the rapid progress of the science during the seven years which have elapsed since the publication of that work. Several doubtful species which were omitted in that work have since been investigated, and are now included or referred to their proper place in the synonymy. Besides these, we have figures and descriptions of all the recently discovered species, of which the number is large, particularly among those of the Pacific coast. These are placed together in the first part of the volume. The writings of foreign authors upon our land-snails are properly discussed, and their descriptions reprinted in full wherever there was any doubt as to the species to which they should be referred. Thus the whole subject is thoroughly posted up to the date of Jan. 1st, 1859, and the work forms a most acceptable contribution to American conchology. W. S.

5. *Catalogue of the Recent Marine Shells found on the Coasts of North and South Carolina;* by J. D. Kurtz. 8vo, pp. 10. Portland, Me., 1860.—This catalogue shows the results of the author's researches in the conchological fauna of our southern coast made in the years 1848–52. The number of species given is 204, an increase of 78 over that given

by Prof. Gibbes in his catalogue published in Tuomey's "Geology of South Carolina," published in 1848. Several species are mentioned as occurring on the marl-bottom off the N. C. coast which have not been hitherto observed north of Florida. Four new species are briefly described, viz., *Venus trapezoidalis*, (which is perhaps the same as *V. pygmœa*,) *Arca Holmesii*, *Scalaria rupicola* and *Chemnitzia textilis;* and several new ones are mentioned by name only. Capt. Kurtz has also contributed very largely to our knowledge of the marine animals of the same coasts, in other departments, as our zoological archives abundantly show. W. S.

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 281, Resolutions on the death of Thomas Nuttall.—A new Unio from the Isthmus of Darien; *I. Lea.*—Additions to the Coleopterous Fauna of Northern California and Oregon; *J. L. LeConte.*—p. 294, Notes and Descriptions of Foreign Reptiles; *E. D. Cope.*—p. 297, A new Myalina and Posidonia from the Carboniferous of Texas; *W. M. Gabb.*—p. 297, New birds from Cape St. Lucas; *J. Xantus.*—p. 299, Notes on Birds collected at Cape St. Lucas by Mr. John Xantus; *S. F. Baird.*—p. 306, Mineralogical Notices; *W. J. Taylor.*—p. 310, New Histeridæ; *J. Leconte.*—p. 317, Contributions to American Lepidopterology, No. 2; *B. Clemens.*—p. 329, On the soft parts of certain Unionidæ; *I. Lea.*—p. 331, Descriptions of three new species of exotic Uniones; *I. Lea.*—Notice of Shells collected by Xantus at Cape St. Lucas: *P. P. Carpenter.*—p. 332, Catalogue of the Venomous Serpents in the Museum of the Academy, with notes on the families, genera, and species (new genus, Teleuraspis); *E. D. Cope.*—Index to vol. for 1859.

Transactions of the Am. Philos. Society (Philadelphia). Vol. XII (new series), Part II, contains:—Art. XI. Della Correlazione delle Forze Chimiche Colla Rifrangibitita delle Irradiazione; Di Zantedeschi.—Esperimenti Eeguiti Col Calorico Solave.—Art. XII. Geological sketch of the estuary and freshwater deposit of the Bad Lands of the Judith, with some remarks upon the surrounding formations; by F. V. Hayden, M.D.—Art. XIII. Extinct Vertebrata from the Judith River and Great Lignite Formations of Nebraska; by Joseph Leidy, M.D.—Art. XIV. A sketch of the Botany of the Basin of the great Salt Lake of Utah; by E. Durand.—Art. XV. Observations on the Magnetic Dip in the United States; by Prof. Elias Loomis.

Report of the Twenty-eighth Meeting of the British Association for the Advancement of Science, held at Leeds, Sept. 1858. London, 1859.—Contains the following zoological papers of more general interest:—On the Anatomy of the Araneidea, particularly of their Spinning organs (2 plates); *R. H. Meade.*—Various reports of Dredging committees.—On the reproductive organs of Sertularia tamarisca; *Allman.*—On the Migration of Birds; *Collingwood.*—Anatomy of the Brain in some small quadrupeds; *Garner.*—On the formation of the Cells of Bees; *Tegetmeier.*

Det Kongelige Danske Videnskabernes Selskabs Skrifter, 5te Raekke, Naturv. og Math. Afdeling, 4de Binds. Copenhagen, 1856-59.—Contains the following zoological papers:—Coroctoca and Spirachtha, Staphylines which bring forth living young and are domesticated with a Termite (with 2 plates); *J. C. Shiödte.*—On the Hectocotyle forms of the Octopod genera Argonauta and Tremoctopus (with 2 plates); *J. J. Sm. Steenstrup.*—Attempt at a monographic exposition of Sergestes, a genus of Shrimps, with remarks upon the organs of hearing in the Decapod Crustacea (5 plates); *H. Kroyer.*—On Mephitis Westermanni, a new "Stinkdyr" from Brazil; *J. Reinhardt.*—5te Binds, 1ste Hefte, 1859: Additamenta ad historiam Ophiuridarum,—descriptions of new or little known species of Serpent-stars; 1st and 2d parts, 7 plates, (new genera, Ophiocten, Ophionereis, Ophiactis, Ophiostigma, and Ophioblenna); *Chr. Fr. Lütken.* W. S.

6. *New Zoological Journal.*—Dr. H. F. Weinland, already well known to American zoologists, has commenced, Oct. 1, 1859, at Frankfort on the Maine, a monthly journal (8vo, 16 pp.) entitled *Der Zoologische Carten. Organ für die Zoologische Gesellschaft.*

IV. ASTRONOMY AND METEOROLOGY.

1. *Supposed intra-Mercurial planet.*—The announcement of M. Le Verrier that the existence of one or more planets within the orbit of Mercury is rendered highly probable by his computations on the movement of the perihelion of Mercury, has called out former observations of the transits of bodies over the sun's disc, in addition to those mentioned in vol. xxviii, pp. 445 and 446 of this Journal.

(1.) Dr. Lescarbault at Orgères (Dept. Eure et-Loir), France, observed with a telescope, March 26th, 1859, a small black circular spot moving across the upper limb of the sun, at a rate which would occupy $4^h\ 26^m\ 48^s$ to traverse the entire disc. Its apparent diameter was less than a quarter that of Mercury in transit. M. LeVerrier thinks the observation worthy of credit, and computes that on the supposition of a circular orbit the time of the planet's revolution around the sun is $19^{d}\cdot7$, and the inclination of its orbit 12° 10′. Its greatest elongation from the sun would not exceed eight degrees, and its light be less than that of Mercury. This planet seems however insufficient to account for all the movement of the perihelion of Mercury.—*Comptes Rend. Acad. Sci., Jan.* 2, 1860.

(2.) Mr. Benj. Scott, of London, states that about midsummer in 1847, he chanced to turn a telescope towards the sun then near its setting, and saw on the sun's disc a well defined black spot, which was not to be seen there at sunrise the next morning. Its angular diameter appeared as large as that of Venus. Mr. Scott mentions that a similar body, or spot, was seen by Mr. Lloft, January 6, 1818.

2. *Mr. Alvan Clark's New Micrometer for measuring large Distances,* (Extracted from the Monthly Notices of the Royal Astronomical Society for July 1859).—At the monthly meeting of the Society in June, Mr. Alvan Clark, of Boston U. S., exhibited a micrometer invented by himself, which is capable of measuring with accuracy any distance up to about one degree. It is also furnished with a position-circle. Its character is essentially the same as that of the parallel-wire micrometer; but it has some peculiarities not, it is believed, previously introduced, and on which its wide range depends.

The most remarkable of these peculiarities consists in its being furnished with *two eye-pieces*, composed of small single lenses, mounted in separate frames, which slide in a grove, and can be separated to the required distance. A frame carrying two parallel spider-lines, each mounted separately with its own micrometer-screw, slides in a dove tailed grove in front of the eye-pieces; and, by a free motion in this frame, each web can be brought opposite to its own eye-lens.

In using this micrometer, the first step is, to set the position-vernier to the approximate position of the objects to be measured. Then the eye-lenses are separated till each is opposite to its own object. The frame containing the webs and their micrometer-screws is then slid into its place; and, the webs having been separated nearly to their proper distance by their free motion in the frame, they are placed precisely on the objects by their fine screws, the observer's eye being carried rapidly from one eye-lens to the other a few times, till he is satisfied of the bisection of each of the objects by its own web. The frame is then removed for

reading off the measure by means of an achromatic microscope, on the stage of which it is placed. One of the webs is brought to the intersection cross-wires in the eye-piece of the microscope; and by turning a screw (the revolutions of which are counted), the frame travels before the microscope, and the other web is brought to the intersection of the cross-wires. The *parts* of a revolution are read off by a vernier from a large divided circle attached to the screw.

The advantages arising from the peculiar construction of this micrometer are the following:—

1. Distances can be observed with great accuracy up to about *one degree*, and the angles of position also.

2. The webs, being in the same plane, are free from parallax, and are both equally distinct, however high the magnifying power may be.

3. The webs are also free from distortion and from color.

4. A different magnifying power may be used on each of the objects; which may be advantageous in comparing a faint comet with a star.

3. *New Double Stars discovered by Mr. Alvan Clark;* communicated by the Rev. W. R. Dawes. (From Monthly Notices of the Royal Astronomical Society, xx., p. 55. Second series.)

Curr't No.	Designation.	R.A. 1860. h m s	N.P.D. 1860. ° ′	Mag.	Dist. ″	Date of discovery.
13	Lalande 1980	1 0 51	45 32·4	8, 8	0·45	1858, Nov. 29
14	Lalande 2634	1 20 7	47 56·6	8, 9	0·8	1858, Oct. 13
15	99 Herculis	18 1 43	59 27·4	5½, 10½	1·7	1859, July 10
16	* Vulpeculæ	19 52	63 7	7½, 8	0·4	1858, Aug. 30
17	{ Cygni 153 B / B.A.C. 6959 }	20 8 3·8	38 57·4	6, 11½	3·8	1859, July 19
18	44 Cygni	20 25 41	53 32·0	6½, 11½	2·8	1859, July 6
19	A.Z. xxiv. 11	21 10 50	26 10·2	7½, 7½	+0·9	1859, July 8
20	75 Cygni	21 34 41	47 21·6	5½, 11	2·9	1859, July 19

All the double stars in this series may be considered as good test-objects for telescopes of from 6 to 8 or 9 inches aperture. But it is not merely as such that they are interesting: they become especially so from the fact that they are all situated in the northern hemisphere, and all but one at more than 30° from the equator. They consequently attain a good altitude at Pulkova; notwithstanding which they have escaped the acute search of M. Otto Struve with the 15-inch refractor, in addition to the previous one by his father with the Dorpat telescope of 9·6 inches aperture. Either, therefore, they have recently undergone considerable change; or, if not, it appears that objects of great difficulty and delicacy may be detected with very perfect telescopes of smaller size, which have escaped the most diligent scrutiny with far larger instruments. The whole of these objects were discovered by Mr. Clark with his own object-glasses of 8 and 8¼ inches aperture, and five of them in my own observatory, during his visit to me last summer; since which I have met with *seven others*, of similar character and situation; and it should be mentioned that none of the objects recorded are below Struve's eighth magnitude for the larger star; all smaller, of which many have been found, having been systematically rejected. It appears, therefore, that there is still much oc-

cupation for telescopes of moderate dimensions, even in this department of astronomy, which might reasonably have been supposed to have been long since exhausted. The *distances* stated in the list are from my own recent measurements.

Haddenham, Thame, November, 1859.

4. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—In the last No. of this Journal, p. 137, I gave a brief notice of this meteor, but from want of space was compelled to limit myself to a brief summary of results. I have received a large amount of documents relating to this meteor, most of which however are too indefinite to be of much value. I now proceed to present a brief summary of what appear to me the most reliable observations.

At New Haven, Ct., Judge W. W. Boardman saw the meteor descend at an angle of 25° to 35° with the vertical, and it passed from his view at the edge of the dome of a steeple in azimuth S. 35° 34′ W. Continuing the meteor's path down to an altitude of 3° or 2°, we have S. 37° W. for the azimuth of the place where the meteor would have disappeared to him had his view been unobstructed.

At New York city Mr. Tatham was riding in the Bowery, and saw the meteor descend at an angle of 20° with the vertical, and in a range with the middle of the street opposite the Old Bowery theatre. According to the map of the city, this direction was S. 27½° W. The diameter of the meteor appeared to be about one-third that of the full moon.

A correspondent of the Evening Post, walking down Broadway, saw the meteor disappear in azimuth S. 25° W.

Mr. Gould, also in Broadway, saw the meteor disappear behind a building in azimuth S. 23½° W.

Mr. Pirsson, also in Broadway, saw the meteor disappear behind a high building in azimuth S. 21° W.

Mr. Bradley, also in Broadway, reports that the meteor disappeared in azimuth S. 16° W.

Several other observers agree as to the general direction of the meteor, but their statements are less precise than those of the preceding. As Mr. Bradley's observation differs materially from the others, I reject it, presuming that his memory must have been in fault, either in respect to his point of observation, or that of the meteor's disappearance. The mean of the other four estimates is S. 24¼° W., or allowing for the effect of the high buildings which obstructed the view of three of the observers, the mean would be about S. 26° W. This result differs five degrees from my former estimate; a difference which is explained by my having obtained two new observations, and by my rejection of Mr. Bradley's observation.

At Washington, the apparent path of the meteor was vertical, and its point of disappearance was estimated at four degrees north of east.

A gentleman four miles west of Dover, Del., was riding towards Dover. His wife saw the meteor; he only saw the smoky trail which he describes as a nearly vertical column, with its base 20°, and its top 40° from the horizon; direction due east.

Mr. Parsons, at Salisbury, Somerset county, Md., saw the meteor descend in a slanting direction to the earth, when it exploded with a dull sound. Its direction was from the N.E.

At Lewistown, Del., the meteor was seen to fall in the N.E. The report was heard five minutes later—loud but distant.

If we mark upon a map all the preceding directions, we find that the lines do not intersect at one point, but they indicate the most probable point of the meteor's disappearance to have been near lat. 39° 10′ and long. 75° 5′.

At New Haven, the path of the meteor was estimated to make an angle of from 25° to 35° with the vertical. Mr. Wilder Smith, near Waterbury, Ct., estimated the inclination to the vertical at about 30°.

At New York, Mr. Tatham estimated the angle with the vertical at 20°, Mr. Gould 10°, Mr. Pirsson 35°, and Mr. Bradley 45°. The mean of these four estimates is 27°.

At Washington, the path was pronounced exactly vertical. The actual path of the meteor was therefore such as, if continued, must undoubtedly have struck the earth. It must have passed vertically over the extreme southern part of New Jersey, and must have struck the earth in Delaware Bay, or near its shore.

That this conclusion is a near approximation to the truth, is confirmed by observations from the southern part of New Jersey.

Mr. Mills was surveying in the forest four miles west of Stephens Creek in Atlantic county, and heard a noise nearly overhead. He looked up and saw a cloud of a rounded form like a puff of smoke about 15 degrees south of the zenith.

At Millville, Cumberland county, a strange rumbling noise was heard somewhat resembling thunder, and one or more clouds of smoke were seen in a southeast direction at an elevation very roughly estimated at 45°.

At Newport, Cumberland county, a rumbling noise, which lasted two minutes, was heard in an east or southeast direction.

At Maurice River Cove, Cumberland county, the captains of the oyster boats saw a flash and smoke in an easterly direction.

At Dias Creek, Cape May county, Mr. Smith states that the noise was great and lasted two or three minutes. The flash was brilliant, and the smoke was seen in a northeast direction at an elevation of 75° or 80° above the horizon.

At Goshen, Cape May county, a noise was heard in a northeast direction, and a cloud of a rounded form was seen in the northeast.

At Dennisville, Cape May county, the noise appeared directly overhead. There was a small cloud or belt of white smoke left in the train of the meteor, about five degrees northwest of the zenith, the atmosphere being perfectly clear at the time. The detonations lasted somewhat over a minute.

The directions indicated in the preceding notices have a decided convergence towards a point near lat. 39° 13′, and long. 74° 52′. This result accords so nearly with that derived from observations made at a distance of a hundred miles and upwards, as to show that the observations are in the main reliable, but subject to that uncertainty which attends all estimates made without instruments, and not reduced immediately to writing. We must then conclude that this meteor passed vertically over the southern part of New Jersey, nearly on the parallel of 39° 13′, and that it struck the earth near the eastern shore of Delaware Bay, probably between Dennis Creek and Maurice River.

I assume that this meteor was a solid body. We are acquainted with two classes of meteors quite distinct from each other and differing greatly in density. Ordinary shooting stars have never been known to reach the earth's surface, or to produce any audible noise. Another class of meteors like that of Agram in 1751 is composed chiefly of iron, and another like that of Weston, Ct. (1807), partly of iron and partly of silicates. They frequently strike the earth, or at least let fall fragments to the earth, and are attended not only by a brilliant flash of light, but by a tremendous noise. The New Jersey meteor bore a striking resemblance to the Weston meteor, not only in the brilliancy of its light, but in the noise which attended it. We cannot doubt that it was a body of considerable density; and the direction of its motion was such that the entire mass must have struck the earth. It may have sunk into Delaware Bay and not a single fragment have fallen upon dry land; but there is reason to hope that at least some fragments of it may yet be discovered. Such fragments, if they exist, are probably scattered along an east and west line coinciding nearly with the parallel of 39° 13′, and the entire mass probably lies near the meridian of 75°.

5. *Sandwich Island Meteor of Nov.* 14, 1859.—A meteor of remarkable size and brilliancy was seen from the slope of Mauna Kea north of the great volcanoe of Kilauea, S. I., soon after dark of the 14th November last, shaped like a cross, having the light of the moon at full, moving vertically south from a point a little below the zenith and disappearing near the crater.—*Pacific Com. Advertiser, Dec.* 15.

6. *Der Meteoreisenfall von Hraschina bei Agram am* 26 *Mai* 1751; von W. Haidinger. Wien, 1859.—Prof. Haidinger has here revised all the contemporary evidence respecting the fall of this remarkable meteoric iron mass, the details of which are fortunately well authenticated. This history is of great interest at the present moment when the late meteors of August and November last have called up anew the discussion of this subject.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Monthly varying level of Lake Ontario, measured, in inches, from a fixed point above the surface downwards, for fourteen years, at Charlotte, mouth of Genesee River, N. Y.:*

Year.	Jan.	Feb	Mar	Apr	May	Jun.	July	Aug	Sep	Oct.	Nov	Dec	Mean	Range.	
1846	51	54	48	45	42	39	39	42	45	45	48	45	45·2	15	Low.
1847	48	42	36	36	29	25	25	25	36	39	43	46	35·8	23	Higher.
1848	29	34	43	38	38	37	38	39	44	49	54	53	41·3	25	Lower.
1849	50	50	52	46	36	33	44	39	45	38	38	41	42·7	19	do.
1850	45	40	40	40	32	29	34	46	47	52	55	43	41·9	26	do.
1851	44	54	48	47	44	38	35	38	42	47	53	51	45·0	19	Low.
1852	50	51	48	44	26	26	22	24	30	23	38	34	34·7	29	Higher.
1853	35	32	32	25	20	14	27	20	24	28	38	39	27·8	25	Mean.
1854	39	39	38	38	27	24	25	27	36	44	48	50	36·2	26	Lower.
1855	52	53	36	40	40	36	34	36	36	34	33	33	38·6	20	do.
1856	35	35	33	31	23	18	23	30	37	46	53	53	34·7	35	Higher.
1857	54	56	46	44	35	24	19	12	14	9	24	22	30·0	47	Mean.
1858	19	13	13	18	4	6	4	2	8	12	14	16	10·9	17	Very high.
1859	20	24	16	10	6	2	8	11	17	22	28	25	15·7	26	Lower.
Mean,	41	41	38	36	29	25	27	28	33	35	40	39	34·3	25	

1. As more water falls usually in the warmer months, the Lake is higher in those months generally than in the colder months.

2. The range has been only 54 inches, the lowest being in February, 1857, and the highest in August, 1858, and in June, 1359; the mean of the two is 27 inches.

3. In 1846 and 1857 the mean level *lowest*, and in 1858 and 1859 *highest.*

4. In 1853 the Lake was near the *mean level*, and in 1857 only a little less, though the first half of the year gave low water and the last half high.

5. The Lake was near the highest, or within four inches of it, in May, June, July and August, 1858, and in May and June, 1859, and of course the average of both years was high.

6. The Lake down to 50 inches or more in January and February, 1846; in November and December, 1848; in January, February and March, 1849; in October and November, 1850; in February, November and December, 1851; in January and February, 1852; in December, 1854; in January and February, 1855; in November and December, 1856; and in January and February, 1857.

These statements show that the changes of the level must be owing to the ordinary causes of supply or diminution of water over this great watershed, and disprove any notion of *periodic* rise and fall under any but meteoric laws. As the water was high in the Lake through 1858, it was suggested that the average fall of water must continue it high in 1859, as the measures now prove. In November, 1859, the water fell to the mean, and rose afterwards from the great autumnal rains at the west which had flowed into Lake Ontario. C. D.

2. *Eruption of Mauna Loa, Sandwich Islands*, (in a letter to Prof. DANA from Prof. R. C. HASKELL, Oahu College, dated Honolulu, Nov. 5, 1859).—Since my last dates (June 22d)* the lava continues to flow from the place of the recent eruption. With scarcely any cessation since the middle of June it has been flowing into the sea. Hawaii has been increased in area by many acres at least, by several hundred acres it is said.

After writing you from Kona in June, I visited Kilauea, which I found *very quiet.* There has, however, been considerable action since you were there in 1840, for the crater is now filled up even with the "black ledge" of which Wilkes speaks.

From Kilauea, passing through Hilo, I went to Waimeu, intending to ascend Mauna Kea, but the weather proved so rainy and foggy that I was unable to do so. From Waimeu I went direct to Kona, crossing the lava stream without difficulty on a mule, between the three mountains. The stream was fully three miles wide where I crossed, and at some points above appeared to be five or six miles wide. At this time the lava was flowing into the sea, and of course running under me as I crossed, yet the lava on the surface was in no place so hot as to burn the hoofs of the mule, or even to be noticed by myself, unless I touched my hand to it.

After arriving at Kona I went by canoe to visit the place where the lava was then and is still flowing into the sea. Without attempting to give an adequate description of the sight presented as I passed, by night, a few rods in front of the stream, which was more than a mile wide, I will only mention one fact.

* Vol. xxviii, [2], 284.

The lava was at a light-red heat, and flowed into the sea with a velocity of two or three miles per hour. And yet this point is forty miles from the source of the stream, and at least twenty-five miles from the lowest point to which the "fissure" in Mauna Loa can possibly extend. Therefore the lava flows twenty-five miles at least, without receiving any heat from the interior of the earth, and yet is still of a light-red heat. It will be remembered, of course, that the stream is covered over with solid lava all the way from the source to within a few feet of the sea, with the exception of a small opening here and there, once in a mile perhaps.

Rev. T. COAN adds, under date of Hilo, Hawaii, Nov. 25, 1859.—"The old lake of fusion in Kilauea is slowly enlarging, and the area around it is subsiding. Probably it may in time resume its old size of half a mile in diameter. Recent visitors have found it active. On one occasion it was thought to throw up jets to the height of 70 feet.

The present eruption has now been in progress ten months, and our last advices report it still active. Several streams have fallen into the ocean along the coast of Kona. These are of different widths, and some of them are separated miles from each other. A small village, Kibele, has been covered of late with the lava, and a large and valuable fish-pond filled up. The people in Kibele pulled down their houses, and also the church, on the approach of the lava stream, and carried off the materials. Just above the church the fiery stream parted, flowed along on each side of the ground where the church had stood, reunited below it, and continued in one stream to the sea. This fact struck the Hawaiians as marvellous, and they regretted having removed their house of worship.

During the early stages of this eruption there were many splendid exhibitions along the line of flow. Canals, cataracts, lakes, fountains and jets of fusion were seen along the slope of the mountain. Forests were consumed, rocks were rent, loud and startling detonations were heard, and the heavens were shrouded with a pall of darkness. Now, and for a long season past, little or no fire is seen, except where the red lava pours into the sea. Here a broken line of fusion is seen coming out from under its self-made counterpane of hardened lava, and pouring down the face of a low and cragged precipice into the ocean, keeping up a constant boiling and sending up clouds of vapor into the air.

The central parts of Kilauea are more quiet than any other part of the crater. We have occasional earthquakes. Two shocks occurred in February, one in July, and two in November of the current year."

BOOK NOTICES.—

1. *Trübner's Bibliographical Guide to American Literature:* a classed list of books, published in the United States of America during the last forty years. With Bibliographical Introduction, Notes and Alphabetical Index. Compiled and edited by NICHOLAS TRÜBNER. London, 1859. 8vo, 554 pp.—This work is beyond all question the best guide which we have to recent American literature and science. Not only is it better than all other bibliographical works of a similar scope but it is excellent in itself. Our limits permit us to mention only one of its most valuable features. Special attention has been bestowed on works in natural science, not only those which appear with an author's name, and are accordingly easy to trace, but more particularly on serial works, such as scientific journals, the transactions of learned societies, and reports of the

state and national legislatures, which are often very difficult to discover by the ordinary apparatus of the trade. Mr. Trübner not only mentions what constitute complete sets of such works; but he enumerates the contents of the several volumes,—so that by means of his excellent index a multitude of articles and essays, often overlooked, are brought to the knowledge of every student. This book should be owned by every book-buyer. D. C. G.

2. *Manual of Public Libraries, Institutions and Societies in the United States and British Provinces of North America;* by WILLIAM J. RHEES, chief clerk of the Smithsonian Institution. 8vo, pp. 687. Lippincott. Philadelphia, 1859.—This volume contains a great amount of useful information on Public Libraries, and gives evidence of much labor in its compilation. The list of libraries in the various States extends to over three thousand titles. In a second edition the Author will be able to supply some obvious deficiencies which are inseparable from the first cast of such a work.

3. *The New American Cyclopedia: a popular Dictionary of general Knowledge;* edited by GEO. RIPLEY and CHAS. A. DANA. Vol. I—VIII. 8vo. New York and London: D. Appleton & Co.—Since our former notice of this Cyclopedia it has advanced rapidly, until now we have before us eight volumes of eight hundred pages each, the last article being on the too famous HAYNAU. Such promptness in issuing so large a mass of elaborately prepared matter speaks well not only for the energy of the publishers and the industry of its editors, but also of the public appreciation of the work. Like its predecessor, the "*Encyclopedia Americana*," 1829–47, by Dr. LIEBER and others, it gives a satisfactory response to almost all questions coming within the range of its plan. The *New Cyclopedia*, however, besides its greater range of topics, has the advantage derived from a vast progress in many departments of knowledge, developed by a numerous corps of contributors skilled each in his own speciality.

In looking over its articles with a peculiar reference to our own departments, we are often tempted to linger among its miscellaneous topics, so rich in various and interesting information. The fine arts, religion, law, politics and war, share our attention with history and biography, ancient and modern, foreign and American, including persons still living, with sketches of events within their respective eras; geography, with the physical and picturesque features and the mineral treasures of particular countries; common and useful arts, agriculture, mechanics, and their various productions; gas lighting, gun-powder, its history, manufacture and uses; caoutchouc, gutta percha, and kindred topics of technical chemistry, with their diversified applications, and a multitude of other subjects, more or less practical and interesting to society at large.

In the American Cyclopedia the articles on science are numerous and valuable, and elevate the work to the character of a compendium of modern science. These articles are in most cases written with decided ability, and evidently by persons who are familiar with the topics which they discuss. While many of the less important subjects are presented with luminous brevity, others are more fully expanded. Among these are many topics of natural history; Chemistry is presented with its equivalents and laws of combination illustrated by many of its modern discoveries and practical applications; of the latter an example is found in the full account of the manufacture of gelatine, of beer and bricks, and

in the ample history of gas lighting. Geology, voltaic electricity, magnetism, and other departments of pure or applied science, are treated with reasonable fulness. As a literary work the Cyclopedia is written in a pure and chaste style, and exhibits the candor and fairness which should ever adorn a record of universal knowledge. B. S.

An Introduction to Practical Pharmacy: designed as a text-book for the Student, &c.; by EDWARD PARRISH. Philadelphia. 2d edition. 246 illustrations. 8vo, pp. 720. Blanchard & Lea, 1859.—A well arranged and carefully prepared treatise, adapted to the state of this art in the United States.

Elements of Inorganic Chemistry, including the applications of the Science in the Arts; by THOS. GRAHAM, F.R.S., L. and E. Edited by HENRY WATTS, B.A., and ROBT. BRIDGES, M.D. 2d American edition in one volume. 233 woodcuts, 8vo, pp. 852. Philadelphia: Blanchard & Lea. 1858.—The American publishers issued the 430 first pages of this volume in 1852 under the editorship of Dr. Bridges. The remainder is reproduced without alteration from the English edition.

Nautical Monographs, No. I. (Washington Observatory) Oct. 1859. The winds at sea, their mean direction, and annual average duration from each of the four quarters. With four plates of diagrams of winds and calms. 4to, pp. 8. By Lt. M. F. MAURY.

Caloric: its Mechanical, Chemical and Vital Agencies in the Phenomena of Nature; by SAMUEL J. METCALF, M.D. 2 vols. 8vo, pp. 630 and 481. Philadelphia: J. B. Lippincott & Co.

Announced.

A Dictionary of English Surnames; by JOHN HENRY ALEXANDER, Esq., of Baltimore, Maryland.—The work will be comprehended in about one thousand pages, and it will be sent to press directly after the necessary commercial and technical arrangements can be made.

Little, Brown & Company, of Boston, propose to publish by subscription a series of photo-lithographic plates of the Fossil Footprints found on the Connecticut River Sandstone, prepared by the late Dr. JAMES DEANE, of Greenfield, in one volume, 4to. Price $5.00.—The work will be issued under the superintendence of T. T. BOUVE, Esq., A. A. GOULD, M.D., and HENRY I. BOWDITCH, M.D., and for the benefit of the family of Dr. Deane, and will be published in the best style, similar to Prof. Agassiz's "Contributions to the Natural History of the United States." Two hundred subscribers are required.

OBITUARY.—

Mr. GUSTAVUS WURDEMANN died on the 29th of Sept. 1859 at Swedesboro, N. J., aged 41 years. Mr. Wurdemann was employed in the U. S. Coast Survey since 1837, in the last twelve years of his life principally as a tidal and meteorological observer in Florida and the Gulf of Mexico. The observations made by him are of great value for their completeness and faithfulness. The short intervals of time left to him by the confining nature of his duties he employed with much success in collecting objects of natural history, and as he was mostly stationed on parts of our coast seldom visited by naturalists, he succeeded in obtaining several species new to science and still more which were new to the fauna of the United States. His collections are in the museums of the Smithsonian Institution and of Prof. Agassiz in Cambridge. Most of his zoological acquisitions have been already published to the world. The largest of our North American herons, *Ardea Wurdemanni*, was discovered by him.

JAMES P. ESPY, one of the most successful meteorologists of our time, died in Cincinnati, on the 24th of January, after a short illness, in the 75th year of his age. We expect to present a notice of his life in our May number.

JEAN-FRÉD.-LUDW. HAUSMANN, the eminent mineralogist, died at Göttingen, Dec. 26, 1859, aged 77 years 10 months.

THE

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JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVI.—*On the Origination and Distribution of Vegetable Species:—Introductory Essay to the Flora of Tasmania;* by Dr. JOSEPH D. HOOKER.

(Continued from p. 25.)

§ 4. *On the General Phenomena of the Distribution of Plants in Time.*

A THIRD class of facts relates to the antiquity of vegetable forms and types on the globe, as evidenced by fossil plants. The chief facts relating to these are the following:—

31. The earliest Flora of which we know much scientifically, is that of the Carboniferous formation. We have indeed plants that belonged to an earlier vegetation, but they do not differ in any important respects from those of the carboniferous formation.

Now the ascertained features of the coal vegetation may be summed up very briefly. There existed at that time,—

Filices, in the main entirely resembling their modern representatives, and some of which may even be generically, though not specifically, identical with them.

Lycopodiaceæ; the same in the main characters as those now existing, and, though of higher specialization of stem, of greater stature, of different species, and perhaps also genera, from modern *Lycopodiaceæ*, yet identical with these in the structure of their reproductive organs and their contents, and in the minute anatomy of their tissues.

Coniferæ. The evidence of this order is derived chiefly from the anatomical characters of the Dicotyledonous wood so abundantly found in the coal, and which seems to be identical in all important respects with the wood of modern genera of that order, to which must be added the probability of *Trigonocarpon* and *Nœggerathia* being Gymnospermous, and allied to *Salisburia.** On the other hand, it must not be overlooked that no Coniferous strobili have been hitherto detected in the Carboniferous formation.

Cycadeæ. Some fragments of wood, presenting a striking similarity in anatomical characters to that of *Cycadeæ*, have been found in the carboniferous series.

In the absence of the fructification of *Calamites*, *Calamodendron*, *Halonia*, *Anabathra*, etc., there are no materials for any safe conclusions as to their immediate affinities, beyond that they all seem to be allied to Ferns or *Lycopodiaceæ*. But the same can hardly be said of the affinities of *Volkmannia*,† *Antholites*, and others, which have been referred, with more or less probability, to Angiospermous Dicotyledons.

The Permian Flora is for the most part specifically distinct from the Carboniferous, but many of its genera are the same. The prevalent types are Gymnospermous Dicotyledons, especially *Cycadeæ*, and a great abundance of Tree-ferns.

The New Red Sandstone, or Trias group, presents plants more analogous to those of the Oolite than to those of the Carboniferous epoch, but they have also much in common with the latter. *Voltzia*, a remarkable genus of Conifers, appears to be peculiar to this period.

In the Lias numerous species of *Cycadeæ* have been found, with various Conifers and many Ferns. No other Dicotyledonous or any Monocotyledonous plants have as yet been discovered, but it is difficult to believe that none such should have existed at a period when wood-boring and herb-devouring insects, belonging to modern genera, were extremely abundant, as has been proved by the researches of Mr. Brodie and Mr. Westwood.‡

The Oolite contains numerous *Cycadeæ*, *Coniferæ*, and Ferns, and more herbivorous genera of insects; and here Monocotyledonous vegetables are recognizable in *Podocarya* and other Pandaneous plants. A cone of *Pinus* has been discovered in the Purbeck, and one of *Araucaria* in the inferior Oolite of Somersetshire.

* Phil. Trans., 1855, p. 149.

† See Quarterly Journal of the Geological Society, May, 1854.

‡ These insects include species of the existing common European genera, *Elater*, *Gryllus*, *Hemerobius*, *Ephemera*, *Libellula*, *Panorpa*, and *Carabus*. Of all conspicuous tribes of plants the *Cycadeæ*, *Filices*, *Coniferæ*, and *Lycopodiaceæ* perhaps support the fewest insects, and the association of the above-named insects with a vegetation consisting solely or mainly of plants of these orders is quite inconceivable.

In the Cretaceous group, Dicotyledons of a very high type appear. A good many species are enumerated* by Dr. Debey, of Aix-la-Chapelle, including a species of *Juglans*, a genus belonging to an order of highly-developed floral structure and complex affinities.†

Characeæ appear for the first time at this epoch, and are apparently wholly similar in structure to those of the present day.

The Tertiary strata present large assemblages of plants of so many existing genera and orders, that it can hardly be doubted but that even the earliest Flora of that period was almost as complex and varied as that of our own. In the lowest Eocene beds are found *Anonaceæ*, *Nipa*, *Acacia*, and *Cucurbitaceæ*.‡ In the Bagshot sands some silicified wood has been found, which may confidently be referred to *Banksia*, and which is, in fact, scarcely distinguishable from recent and fossil Australian Banksia wood.§

In the brown coal of the Eocene and Miocene periods, Fan-palms, Conifers, and various existing genera of *Myriceæ*, *Laurineæ*, and *Plataneæ* are believed to have been identified. Wesel and Weber describe from the brown coal of the Rhine a rich and varied Flora, representing numerous families never now seen associated, and including some of the peculiar and characteristic genera of the Australian, South African, American, Indian, and European floras.‖

* Quart. Journ. Geol. Soc., vii, pt. 1, misc., p. 110.

† Professor Oswald Heer, of Zurich, in an interesting little paper (Quelques Mots sur les Noyers), in Bibl. Univ. Genev., Sep. 1858, argues from the fact of the early appearance of *Juglans* in the geological series, that this genus must be a low type of the Dicotyledonous class to which it belongs. The position of *Juglans* is unsettled in the present state of our classification of Dicotyledonous orders, as it has equal claims to be ranked with *Terebinthaceæ*, which are very high in the series, and with *Cupuliferæ*, which are placed very low; and were the grounds for our thus ranking these orders based on characters of ascertained relative value, such an argument might be admissible; but the system which sunders these orders is a purely artificial one, and *Juglans* with its allies would prove it so, if other proofs were wanting; for it absolutely combines *Terebinthaceæ* and *Cupuliferæ* into one natural group, in which (as in so many others) there is a gradual passage from great complexity of floral organs to great simplicity.

‡ I am far from considering the identification of these and the other genera which I have enumerated in various strata as satisfactory, but I conclude that they may be taken as evidence of as highly developed and varied plants having then existed as are now represented by these genera.

§ I am indebted to the late Robert Brown for this fact, and for the means of comparing the specimens, which are beautifully opalized. I ascertained that he was satisfied with the evidence of this wood having really been dug up near Staines, though it is so perfectly similar in every respect to the opalized Banksia-wood of Tasmania as to suggest to his mind and my own the most serious doubts as to its English origin.

‖ See Quart. Journ. Geol. Soc., xv, misc. 3, where an abstract is given, with some excellent cautions, by C. J. F. Bunbury, Esq. The Australian genera include *Eucalyptus*, *Casuarina*, *Leptomeria*, *Templetonia*, *Banksia*, *Dryandra*, and *Hakea*. I am not prepared to assert that these identifications, or the Australian ones of the Mollasse, are all so unsatisfactory that the evidence of Australian types in the brown coal and Mollasse should be altogether set aside; but I do consider that not one of the above-named genera is identified at all satisfactorily, and that many of them are not even problematically decided.

In the Mollasse and certain Miocene formations at Œningen and elsewhere in Germany, Switzerland, and Tuscany,* 900 species of Dicotyledons† have been observed, all apparently different from existing ones. They have been referred, with more or less probability, to Fan-palms, Poplars (three species), evergreen *Laurineæ*, *Ceratonia*, *Acacia*, *Tamarindus*, *Banksia*, *Embothrium*, *Grevillea*, *Cupressus*, several species of *Juglans* (one near the North American *J. acuminata*, another near the common walnut of Europe and Asia, *J. regia*, and a third near the North American *J. cinerea*); also a Hickory, near the *Carya alba* (a genus now wholly American), and a *Pterocarya* closely allied to *P. Caucasica*.

The rise of the Alps was subsequent to this period; and in the European deposits immediately succeeding that event, in Switzerland (at Durnten and Utznach) are found evidences of the following existing species,—Spruce, Larch, Scotch Fir, Birch, a Hazel (different from that now existing), *Scirpus lacustris*, *Phragmites communis*, and *Menyanthes trifoliata*.

The glacial epoch followed, during and since which there has probably been little generic change in the vegetation of the globe.

32. So much for the main facts hitherto regarded as established in Vegetable Palæontology. They are of little value as compared with those afforded by the Animal Kingdom, even granting that they are all well made out, which is by no means the case. In applying them theoretically to the solution of the question of creation and distribution, the first point which strikes us is the impossibility of establishing a parallel between the successive appearances of vegetable forms in time, and their complexity of structure or specialization of organs, as represented by the successively higher groups in the natural method of classification. Secondly, that the earliest recognizable Cryptogams

* During the printing of this sheet I have received from my friend M. DeCandolle a very interesting memoir on the tertiary fossil plants of Tuscany, by M. C. Gaudin and the Marquis C. Strozzi, in which some of the genera here alluded to are described. The age of these Tuscan beds is referred by Prof. O. Heer to a period intermediate between those of Utznach and Œningen. The most important plants described are, Coniferæ 6 sp., Salix 2, Liquidambar 1, Alnus 1, Carpinus 1, Populus 2, Fagus 1, Quercus 5, Ulmus 2, Planera 1, Ficus 1, Platanus 1, Oreodaphne 1, Laurus 2, Persea 1, Acer 2, Vitis 1, Juglans 4, Carya 1, Pterocarya 1. There are 49 extinct species in all, of which 46 are referred, without even a mark of doubt or caution, to existing genera, and this in almost all cases from imperfect leaves alone! Without questioning the good faith or ability of the authors of this really valuable and interesting memoir, I cannot withhold my protest against this practice of making what are at best little better than surmises, appear under the guise of scientifically established identifications. What confidence can be placed in the positive reference of supposed fossil Fungi to *Sphæria*, or of pinnated leaves to *Sapindus*, and other fragments of foliage to existing genera of *Laurineæ*, *Ficus*, and *Vitis*?

† O. Heer, Sur les Charbons feuilletés de Durnten et Utznach, in Mem. Soc. Helvet. Sc. Nat. 1857; Bibl. Univers. Genev., August, 1858.

should not only be the highest now existing, but have more highly differentiated vegetative organs than any subsequently appearing; and that the dicotyledonous embryo and perfect exogenous wood with the highest specialized tissue known (the coniferous, with glandular tissue*), should have preceded the monocotyledonous embryo and endogenous wood in date of appearance on the globe, are facts wholly opposed to the doctrine of progression, and they can only be set aside on the supposition that they are fragmentary evidence of a time further removed from that of the origin of vegetation than from the present day; to which must be added the supposition that types of *Lycopodiaceæ*, and a number of other orders and genera, as low as those now living, existed at that time also.

33. Another point is the evidence,† said to be established, of genera now respectively considered peculiar to the five continents having existed cotemporaneously at a comparatively recent geological epoch in Europe, and the very close affinity, if not identity, of some of these with existing species. The changes in the level and contour of the different parts of the earth's surface which have occurred since the period of the chalk, or even since that preceding the rise of the Alps, imply a very great amount of difference between the past and present relations of sea and land and climate; and it is no doubt owing to these changes that the *Araucariæ*, which once inhabited England, are no longer found in the northern hemisphere, and that the Australian genera which inhabited Europe at a period preceding the rise of the Alps have since been expelled.

34. Such facts, standing at the threshold of our knowledge of vegetable palæontology, should lead us to expect that the problem of distribution is an infinitely complicated one, and suggest the idea that the mutations of the surface of our planet, which replace continents by oceans, and plains by mountains, may be insignificant measures of time when compared with the duration of some existing genera and perhaps species of plants, for some of these appear to have outlived the slow submersion of continents.

* The vexed question of the true position of Gymnospermous plants in the Natural System assumes a somewhat different aspect under the view of species being created by progressive evolution. In the haste to press the recent important discoveries in vegetable impregnation and embryogeny into the service of classification, the long-established facts regarding the development of the stem, flower, and reproductive organs themselves of Gymnospermous plants have been relatively underrated or wholly lost sight of; and if an examination of the doctrines of progression and variation lead to a better general estimation of the comparative value of the characters presented by these organs, the acceptance or rejection of the doctrines themselves is, in the present state of science, a matter of secondary importance.

† See fifth foot-note of p. 307 (‖): what I have there said of the supposed identifications of the Australian genera applies to many of those of the other enumerated quarters of the globe.

35. From the sum then of our theories, as arranged in accordance with ascertained facts, we may make the following assumptions:—That the principal recognized families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have as families survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, etc., and the increase of individuals of others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a variety possesses of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions. An appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age,* and thus survive many generations of other species, of which generations some present characters foreign to their parents.

36. In the above line of argument I have not alluded to the question of the origin of those families of plants which appear in the earliest geological formations, nor to that of vegetable life in the abstract, conceiving these to be subjects upon which, in the present state of science, botany throws no light whatever. Regarded from the classificatory point of view, the geological history of plants is not altogether favorable to the theory of progressive development, both because the earliest ascertained types are of such high and complex organization,† and because there are no known fossil plants which we can certainly assume to be-

* In considering the relative amount and rate at which different plants vary, it should be remembered that we habitually estimate them not only loosely but falsely. We assume annuals to be more variable than perennials, but we probably greatly overrate the amount to which they really are so, because a brief personal experience enables us to study many generations of an annual under many combinations of physical conditions; whereas the same experience embraces but a fractional period of the duration of (comparatively) very few perennials. It has also been well shown by Bentham (in his paper on the British Flora, read [1858] before the Linnæan Society) that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, etc., which propagate the individual; and in the case of *Rubi*, which comparatively seldom propagate by seed, a large tract of ground may be peopled by parts of a single individual.

† I have elsewhere stated that I consider the evidence of *Algæ* having existed at a period preceding vascular Cryptogams to be of very little value. (Lond. Journ. Bot., viii, p. 254.)

long to a non-existing class or even family, or that are ascertained to be intermediate in affinity between recent classes or families.*

The progress of investigation may ultimately reveal the true history of the unrecognized vegetable remains with which our collections abound, and may discover to us amongst them new and unexpected organisms, suggesting or proving a progressive development; but in the meantime the fact remains that the prominent phenomena of vegetable palæontology do not advance us one step towards a satisfactory conception of the first origin of existing natural orders of plants.

Taking the Conifers for an example, whatever rank is given to them by the systematist, that they should have preceded Monocotyledons and many Dicotyledons in date of appearance on the globe, is a fact quite incompatible with progressive development in the scientific acceptation of the term, whilst to argue from their apparently early appearance that they are low in a classificatory system is begging the question.

Another fact to be borne in mind is, that we have no accurate idea of what systematic progression is in botany. We know little of high and low in the Vegetable Kingdom further than is expressed by the sequence of the three classes, Dicotyledons, Monocotyledons, and Acotyledons; and amongst Acotyledons, of Thallogens being lower than Acrogens, and of these that the Mosses, etc., are lower than Filices and their allies. It is true that we technically consider multiplication and complexity of floral whorls in phænogamic plants as indications of superior organization; but very many of the genera and orders most deficient in these respects are so manifestly reduced members of others, which are indisputably the most complex in organization in the whole Vegetable Kingdom, that no good classification even has been founded on these considerations alone.†

* It must not be supposed that in saying this I am even expressing a doubt as to there having been plants intermediate in affinity between existing orders and classes. Analogy with the animal kingdom suggests that some at any rate of the plants of the coal epoch do hold such a relationship; but should they not do so, I consider this fact to be of little value in the present inquiry, for I incline to believe that the ascertained geological history of plants embraces a mere fraction of their whole history.

† The subject of the retrogression of types has never yet been investigated in botany, nor its importance estimated in inquiries of this nature. To whatever order we may grant the dignity of great superiority or complexity, we find that order containing groups of species of very simple organization; these are moreover often of great size and importance, and of wide geographical distribution. Such groups, if regarded *per se*, appear to be far lower in organization than other groups which are many degrees below them in the classified series; and our only clue to their real position is their evident affinity with their complex co-ordinates;—destroy the latter by a geological or other event, and all clue to the real position of the former may be lost. Are such groups of simply-constructed species created by retrogressive variation of the higher, or did the higher proceed from them by progressive variation? If the latter, did the simpler forms precede in origin the highest forms of all other groups which rank below them in the classified series?

37. Again, it is argued by both Mr. Darwin and Mr. Wallace that the general effects of variation by selection must be to establish a general progressive development of the whole animal kingdom. But here again in botany we are checked by the question, What is the standard of progression? Is it physiological or morphological? Is it evidenced by the power of overcoming physical obstacles to dispersion or propagation, or by a nice adaptation of structure or constitution to very restricted or complex conditions? Are cosmopolites to be regarded as superior to plants of restricted range, hermaphrodite plants to unisexual, parasites to self-sustainers, albuminous-seeded to exalbuminous, gymnosperms to angiosperms, water plants to land, trees to herbs, perennials to annuals, insular plants to continental? and, in fine, what is the significance of the multitudinous differences in point of structure and complexity, and powers of endurance, presented by the members of the Vegetable Kingdom, and which have no recognized physiological end and interpretation, nor importance in a classificatory point of view? It is extremely easy to answer any of these questions, and to support the opinion by a host of arguments, morphological, physiological, and teleological; but any one gifted with a quick perception of relations, and whose mind is stored with a sufficiency of facts, will turn every argument to equal advantage for both sides of the question.

To my mind, however, the doctrine of progression, if considered in connection with the hypothesis of the origin of species being by variation, is by far the most profound of all that have ever agitated the schools of Natural History, and I do not think that it has yet been treated in the unprejudiced spirit it demands. The elements for its study are the vastest and most complicated which the naturalist can contemplate, and reside in the comprehension of the reciprocal action of the so-called inorganic on the organic world. Granting that multiplication and specialization of organs is the evidence and measure of progression, that variation explains the *rationale* of the operation which results in this progression, the question arises, What are the limits to the combinations of physical causes which determine this progression, and how can the specializing power of Nature stop short of causing every race or variety ultimately to represent a species? While the psychological philosophers persuade us that we see the tendency to specialize pervading every attribute of organic life, mental and physical; and the physicists teach that there are limits to the amount and duration of heat, light, and every other manifestation of physical force which our senses present or our intellects perceive, and which are all in process of consumption; the reflecting botanist, knowing that his ultimate results must accord with these facts, is perplexed at feeling

that he has failed to establish on independent evidence the doctrines of variation and progressive specialization, or to co-ordinate his attempts to do so with the successive discoveries in physical science.

38. Before dismissing this subject, I may revert once more to the opposite doctrine, which regards species as immutable creations, and this principally to observe that the arguments in its favor have neither gained nor lost by increased facilities for investigation, or by additional means for observation. The facts are unassailable that we have no direct knowledge of the origin of any wild species; that many are separated by numerous structural peculiarities from all other plants; that some of them invariably propagate their like; and that a few have retained their characters unchanged under very different conditions and through geological epochs. Recent discoveries have not weakened the force of these facts, nor have successive thinkers derived new arguments from them; and if we hence conclude from them that species are really independent creations and immutable, though so often illimitable, then is all further inquiry a waste of time, and the question of their origin; and that of their classification in Genera and orders, can, in the present state of science, never be answered; and the only known avenues to all means of investigation must be considered as closed till the origin of life itself is brought to light.

39. Of these facts the most important, and indeed the only one that affords a tangible argument, is that of genetic resemblance. To the tyro in Natural History all similar plants may have had one parent, but all dissimilar plants must have had dissimilar parents. Daily experience demonstrates the first position, but it takes years of observation to prove that the second is not always true. There are, further, certain circumstances connected with the pursuit of the sciences of observation which tend to narrow the observer's views of the attributes of species; he begins by examining a few individuals of many extremely different kinds or species, which are to him fixed ideas, and the relationships of which he only discovers by patient investigation; he then distributes them into genera, orders, and classes, the process usually being that of reducing a great number of dissimilar ideas under a few successively higher general conceptions; whilst with the history of the ideas themselves, that is, of species, he seldom concerns himself. In a study so vast as botany, it takes a long time for a naturalist to arrive at an accurate knowledge of the relations of genera and orders if he aim at being a good systematist, or to acquire an intimate knowledge of species if he aim at a proficiency in local floras, and in both these pursuits the abstract consideration of the species itself is generally lost sight of; the systematist seldom returns to it, and

the local botanist, who finds the minutest differences to be hereditary in a limited area, applies the argument derived from genetic resemblance to every hereditarily distinct form.

40. It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not of consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species,* of which some generations have proved invariable within our extremely limited experience. These theories must not, however, be judged of solely by the force of the very few absolute facts on which they are based. There are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage. For according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation, and their extinction by natural causes, and regarding the *rationale* of classification, is swallowed up in the gigantic conception of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation† and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the "spontaneous generation" of living beings of every degree of complexity in structure and refinement of organization.

* See paragraph 4, where I have stated that the grand total of unstable species probably exceeds that of the stable.

† It is a curious fact (illustrative of a well-known tendency of the mind), that the few writers who have in imagination endeavored to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtain; thus in both cases arguing *ad ignotum ab ignoto.*

On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expanding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties. And if, as I have endeavored to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelops the history of species,—holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations, founded on genetic resemblance, now appears to be.

The arguments deduced from genetic resemblance being (in the present state of science), as far as I can discover, exhausted, I have felt it my duty to re-examine the phenomena of variation in reference to the origin of existing species. These phenomena I have long studied independently of this question; and when treating either of whole floras or of species, I have made it my constant aim to demonstrate how much more important and prevalent this element of variability is than is usually admitted, as also how deep it lies beneath the foundations of all our facts and reasonings concerning classification and distribution. I have hitherto endeavored to keep my ideas upon variation in subjection to the hypothesis of species being immutable; both because a due regard to that theory checks any tendency to careless observation of minute facts, and because the opposite one is apt to lead to a precipitate conclusion that slight differences have no significance; whereas, though not of specific importance, they may be of high structural and physiological value, and hence reveal affinities that might otherwise escape us. I have already stated how greatly I am indebted to Mr. Darwin's* *rationale* of the phenomena of variation and natural selection in the production of species; and though it does not positively establish the doctrine of creation by variation, I expect that every additional fact and observation relating to species will gain great additional value from being viewed in reference to it, and that it will materially assist in developing the principles of classification and distribution.

* In this Essay I refer to the brief abstract only (Linn. Journ.) of my friend's views, not to his work now in the press, a deliberate study of which may modify my opinion on some points whereon we differ. Matured conclusions on these subjects are very slowly developed.

[This extensive Essay proceeds to consider the Flora of Australia under § 1. General Remarks. 2. Estimate of the numbers, distribution, and affinity of the Classes, Orders, &c. 3. The Australian distribution of Natural Orders. 4. The Genera of the Australian Flora. 5. The Tropical Australian Flora. 6. The Flora of Extra-tropical Australia. 7. The Flora of Countries around Spencer's Gulf. 8. The Tasmanian Flora, an analysis of its elements and the geographical distribution of the species and their allies. 9. The New Zealand and Polynesian features of the Australian vegetation. 10. The Antarctic plants of Australia. 11. The South African features of Australian vegetation. 12. The European features of the Australian Flora. 13. On the Fossil Flora of Australia and its Geology in relation to the existing Flora. 14. On some of the naturalized Plants of Australia. 15. A List of some of the Esculent Plants of Australia. 16. Outlines of the progress of Botanical Discovery in Australia, etc.—From the large amount of matter suitable to our pages, we have space for only two of the shorter of the sections.]

General Remarks on the Flora of Australia.

The Flora of Australia has been justly regarded as the most remarkable that is known, owing to the number of peculiar forms of vegetation which that continent presents. So numerous indeed are the peculiarities of this Flora, that it has been considered as differing fundamentally, or in almost all its attributes, from those of other lands; and speculations have been entertained that its origin is either referable to another period of the world's history from that in which the existing plants of other continents have been produced, or to a separate creative effort from that which contemporaneously peopled the rest of the globe with its existing vegetation; whilst others again have supposed that the climate or some other attribute of Australia has exerted an influence on its vegetation, differing both in kind and degree from that of other climates. One of my objects in undertaking a general survey of the Australian Flora, has been to test the value of the facts which have given rise to these speculations, and to determine the extent and comparative value of a different and larger class of facts which are opposed to them, and which might also give some clue to the origin of the flora, and thus account for its peculiarities. This I pursued under the impression that it is the same with the study of whole floras as of single species or their organs, viz., that it is much easier to see peculiarities than to appreciate resemblances; and that important general characters which pervade all the members of a family or flora, are too often overlooked or undervalued, when associated with more conspicuous differences which enable us to dismember them. The result has proved, as I anticipated, that, the great difficulty being surmounted of collecting all the materials and so classifying them as to allow of their being generalized upon, the peculiarities of the flora, great

though they be, are found to be more apparent than real, and to be due to a multitude of specialities affecting the species, and to a certain extent the genera, but not extending to the more important characteristics of the vegetation, which is not fundamentally different from that of other parts of the globe.

Before proceeding to the discussion of the elements of the Australian Flora, I shall shortly describe its general character, viewed in the double light of a peculiar vegetation and as a part of the existing flora of the globe. Its chief peculiarities are:—

That it contains more genera and species peculiar to its own area, and fewer plants belonging to other parts of the world, than any other country of equal extent. About two-fifths of its genera, and upwards of seven-eighths of its species are entirely confined to Australia.

Many of the plants have a very peculiar habit or physiognomy, giving in some cases a character to the forest scenery (as *Eucalypti*, *Acaciæ*, *Proteaceæ*, *Casuarinæ*, *Coniferæ*), or are themselves of anomalous or grotesque appearance (as *Xanthorrhœa*, *Kingia*, *Delabechea*, *Casuarina*, *Banksia*, *Dryandra*, etc.).

A great many of the species have anomalous organs, as the pitchers of *Cephalotus*, the deciduous bark and remarkable vertical leaves of the *Eucalypti*, the phyllodia of *Acacia*, the fleshy peduncle of *Exocarpus*, the inflorescence and ragged foliage of many *Proteaceæ*.

Many genera and species display singular structural peculiarities, as the ovules of *Banksia*, calyptra of *Eucalyptus*, stigma of *Goodeniaceæ*, staminal column of *Stylidium*, irritable labellum of various *Orchideæ*, flowers sunk in the wood of some *Leptospermeæ*, pericarp of *Casuarina*, receptacle and inner staminodia of *Eupomatia*, stomata of *Proteaceæ*.

On the other hand, if, disregarding the peculiarities of the flora, I compare its elements with those of the floras of similarly situated large areas of land, or with that of the whole globe, I find that there is so great an agreement between these, that it is impossible to regard Australian vegetation in any other light than as forming a peculiar, but not an aberrant or anomalous, botanical province of the existing Vegetable Kingdom. I find:—

That the relative proportions of the great classes of Monocotyledons to Dicotyledons, of genera to orders, and of species to genera, are the same as those which prevail in other floras of equal extent.

That the subclasses distinguished by a greater or less complexity of the floral envelops, or their absence, as *Thalamifloræ*, *Calyciflorœ*, *Corolliflorœ*, etc., are also in the same relative proportions as prevail in other floras.

That the proportion of Gymnospermous plants to other Dicotyledons is not increased.

That all the Australian natural orders, with only two small exceptions, are also found in other countries; that most of those most widely diffused in Australia are such as are also the most widely distributed over the globe; and that Australia wants no known order of general distribution.

That the only two absolutely peculiar natural orders contain together only three genera, and very few species; they are, further comparatively local in Australia, and are rather aberrant forms of existing natural families than well-marked isolated groups: *Brunoniaceæ* being intermediate between *Goodeniaceæ* and *Compositæ*, and *Tremandreæ* between *Polygaleæ* and *Buettneriaceæ*.

That the large natural orders and genera, which, though not absolutely restricted to Australia, are there very abundant in species and rare elsewhere, and for which I shall hence adopt the term Australian, stand in very close relationship to groups of plants which are widely spread over the globe (as *Epacrideæ* to *Ericeæ*, *Goodeniaceæ* to *Campanulaceæ*, *Stylideæ* to *Lobeliaceæ*, *Casuarineæ* to *Myriceæ*).

That these Australian orders are exceedingly unequally distributed in Australia; that there is a greater specific difference between two quarters of Australia (southeastern and southwestern) than between Australia and the rest of the globe; and that the most marked characteristics of the flora are concentrated at that point which is geographically most remote from any other region of the globe.

That most of those Australian orders and genera which are found in other countries around Australia, have their maximum development in Australia at points approximating in geographical position towards those neighboring countries. Thus the peculiarly Indian features of the flora are most developed in northwestern Australia, the Polynesian and Malayan in northeastern, the New Zealand and South American in southeastern, and the South African in southwestern Australia.

That of the nine largest natural orders, which together include a moiety of the Australian species of flowering plants, no fewer than six belong to the nine largest natural orders of the whole world, and five belong to the largest in India also.

That in Australia itself, in advancing from the tropics to the coldest latitudes, or from the driest to the most humid districts, or from the interior to the seashore, or in ascending the mountains, the changes in vegetation are in every aspect analogous to what occur in other parts of the globe.

That the relations between the epochs of the flowering and the fruiting of plants, and the seasons of the year, are the same

in Australia as elsewhere, and most remarkably so; the *Orchidæ* being spring flowers, the *Leguminosæ* summer, the *Compositæ* autumn, and the *Cryptogamia* winter.

That the peculiarities of the Australian flora in no way disturb the principles of natural arrangement derived from the study of the flora of the globe apart from that of Australia. For, after having attempted to consider the Australian vegetation in a classificatory point of view, shutting out of my view, as far as I could, that of other countries, I have been led to the conclusion that the authors of the Natural System—Ray, Linnæus,* and the Jussieus—might have developed the same Natural System had they worked upon Australian plants instead of upon European.

I find further, that the classes, orders, genera, and species, may be about as well (or as ill) fixed or limited by a study of their Australian members as by those of any other country similarly circumstanced; and that there is the same vagueness as to the exact limits of natural groups, a similar inequality amongst them in numerical value and botanical characters, and an analogous difficulty in forming subclasses intermediate between classes and orders, as other floras present. The Australian flora, in short, neither breaks down nor improves the Natural System of plants as a whole, though it throws great light on its parts; the Australian genera fall into their places in that system well enough, though that system was developed before Australia was known botanically, and was chiefly founded upon a study of the vegetation of its antipodes.

Thus, whether the Australian flora is viewed under the aspect of its morphology and structure, as exhibited by its natural classification, or its numerical proportions or geographical distribution, it presents essentially the same primary features as do those of the other great continents: and it hence appears to me rash to assume that its origin belongs to another epoch of the earth's

* The real merits of Linnæus as a founder of the Natural System have never been appreciated. In the well deserved admiration of the genius and labors of the Jussieus, it is forgotten that the powers displayed by Linnæus in constructing the Genera Plantarum was not less (perhaps greater) than that exercised in grouping these into those genera of a higher value, which are now called Jussieuan Orders. The history of our Natural System presents but four salient points:—I. Ray's division of all plants into Phænogams and Cryptogams, and of the former into Monocotyledons and Dicotyledons. II. Linnæus's forming natural groups called genera, and rendering a knowledge of them acccessible to scientific minds by means of a binomial nomenclature and a mixed natural and artificial system of classes and orders. III. The Jussieus' combining most of the genera of Linnæus into truly natural orders, under Ray's classes, which classes they divided into subclasses as artificial as many of Linnæus's classes were. IV. The separation of Gymnosperms, by Brown, which is the first step towards a natural classification of the Jussieuan orders of Dicotyledons. (See Lond. Journ. of Bot. and Kew Gard. Misc., ix, 314, *note.*)

history than that of other floras, when the proportions of its classes, etc., are identically the same with these; or that it should be attributed to a distinct creative effort, if this is manifested only in effecting morphological differences requisite to constitute species and genera in our classification, without disturbing the proportions of these; or that the local influence of the Australian climate should be essentially different from that of other countries, and yet effect no physiological change in the periods of flowering and fruiting, or produce any other functional disturbances of the vegetable organisms, or affect the agency of humidity, temperature, soil, and elevation, on plants.

On the Fossil Flora of Australia, and its Geology in relation to the Existing Flora.

The fossiliferous rocks of Australia do not throw much light upon the antiquity of its existing flora, because of the hiatus which geologists seem to consider exists between the palæozoic and tertiary strata of that country. Mr. Jukes* has called attention to the curious fact that this deficient series in Australia is largely developed in Europe, and there presents such Australian forms of life as marsupiate quadrupeds, *Trigonia* and other fossil shells, together with Cycadeous plants. To the latter no importance can be attached, as this order is far more characteristic of tropical America, of India, and even of southeast Africa, than of Australia; but on the other hand the *Araucaria* of the English oolite, and other fossils alluded to at p. 308, would seem to tend to confirm Mr. Jukes's observation.

The so-called Palæozoic rocks of Australia contain fossil plants of which so little, botanically, is known, that it would be rash to speculate on their affinities, even if we knew the age of the beds they are found in, as compared with the European, which we do not. Their fossils comprise ferns of several genera, including the genus *Glossopteris*, which is found in the oolite beds of England, and in India;† *Phyllotheca*, a plant somewhat similar to *Casuarina*, but of extremely doubtful affinity; *Vertebraria*, also an Indian fossil, as to the affinities of which no plausible guess has been made; *Sphenopteris* and *Zygophyllites*, of which little more can be said. To these the Rev. W. B. Clarke‡ adds the following well-known British coal fossils,—*Lepidodendron*, *Halonia*, *Sigillaria*, *Ulodendron*, *Calamites*, and *Stigmaria*.

Many of the tertiary fossil plants of Australia would seem to be very closely allied to existing ones; these include the *Casuarina* cones of Flinders Island, the *Banksia* and *Araucaria* wood

* J. B. Jukes, 'Physical Structure of Australia,' p, 89, etc.
† M'Coy in Ann. Nat. Hist., vol. xx, p. 152.
‡ Journ. Geolog. Soc. Lond., vol. iv, p. 60.

of Tasmania, the *Banksia* cones of Victoria (which seem identical with those of *B. ericifolia*, though buried under many feet of trap). The leaves of the calcareous tuffs on the banks of the Derwent,* etc., appear however to belong to a different and warmer period.

From the above it would appear that the extinct flora of Australia was not entirely different from that now existing, and, following Mr. Jukes's line of argument, that Australia continued as dry land during the European Oolitic and Cretaceous periods. At this epoch Mr. Jukes assumes that the peculiar flora of Australia was introduced, and that the continent was again submerged during the Tertiary epoch, when it presented the appearance of two long islands, or chains of islands, one, the larger, representing the elevated land of eastern Australia and Tasmania, the other that of southeastern Australia, together with subsidiary groups in the western and northern parts of the continent.

These are the speculations of an able geologist and voyager, which I introduce without comment, and chiefly to observe that such a partition of the continent may be supposed to be favorable to the multiplication of forms of vegetable life out of fewer pre-existing ones, by the segregation of varieties. These groups of islands would present a precise analogy with the Galapagos and Sandwich groups, where we have the small islands of one Archipelago peopled by different species, and even genera. The subsequent elevation of these islets, and consequent union of them into larger ones, would further, according to Darwin's hypothesis (of the struggle of very different kinds of species and families for occupation of the soil resulting in a further separation of varieties into species), tend to enlarge the genera numerically within comparatively small geographical limits, and thus effect such a geographical distribution of plants as Australia now presents.

In our complete ignorance as to the condition of all the continents during the Palæozoic epoch, it is impossible to speculate on the earlier condition of the Australian flora. That previous to some Tertiary submersion of a great part of the continent, it was not altogether specifically different from what it now is, would appear from a fact insisted on by Mr. Jukes, that it was during such a submersion that those volcanos were active, the lavas of which now cover large tracts of southern Australia, and which we know to have buried a plant apparently identical with *Banksia ericifolia*, which is still one of the commonest trees in that part of the country: but the question of where the *Banksias* and their allies were created, and, if in other lands than

* Darwin's Journal, p. 535, and Volcanic Islands, p. 140; Strzelecki, p. 254; Milligan in Tasman. Journ., i, 131.

Australia, how they migrated thither, we have no means of answering. If the identifications of *Banksia* and other Proteaceous leaves in the Cretaceous and Miocene formations of Europe are worthy of confidence, it is possible that the Australian types may have migrated from the northern to the southern hemisphere, as, according to Darwin's speculations, the existing European plants in Australia have.

Some arguments in favor of the antiquity of the Australian flora as compared with the European may be derived from a consideration of its generic and ordinal peculiarities. If, as I have expressed it, a genus or order is rendered peculiar, that is, unlike its allies, by the extinction of the intermediate species, it follows that the greater the peculiarity the greater the number of lapsed forms. Applying this argument to the Australian flora, we must assume an extraordinary destruction of species that once linked it with the general flora of the globe, to account for its many peculiar genera, and these being represented by so many species. But as this destruction of species is primarily due to geological causes that influence climates, and so directly and indirectly lead to the extinction of species, and as geological events are of slow progress, it follows that we must regard the Australian flora as a very ancient one. Again, Darwin argues that a rich flora or fauna, marked by a preponderance of highly developed types, must have required a large area for its development: this is because, according to his view, the principle of natural selection favors the high forms, and is unfavorable to the low. Now it could easily be shown that the Australian flora is of as high a type as any in the globe, but under existing conditions has a very small area for its development, and presents fewer representatives of other floras to contend with than most; and we must hence, under these hypotheses, assume not only the antiquity of the flora, but that it was developed in a much larger area than it now occupies.

The only other geological speculation, founded upon anything like plausible grounds, that bears upon the origin of any of the plants now inhabiting Australia, is that of Mr. Darwin in reference to the European species, to which I have alluded at p. 23. It implies of course that the existing European types were introduced into the continent long subsequently to the peculiar Australian, and are plants of a later creation. I have already pointed out the difficulties attending its adoption, the chief of which is the admission of such a cold climate in the intertropical latitudes as that not merely a temperate, but a decidedly northern flora should have migrated across them; and that this migration, if conceded, must have been extensive and have introduced very many genera and species into the tropics appears likely, when we consider the fragmentary character of the assem-

blage of northern forms still left in Australia; for even when reduced to its most typical examples, it consists of nearly as many natural orders as species. The little colony of south Australian genera found under the equator, on Kini Balou, in Borneo, presents another difficulty, except indeed it be regarded as evidence of that previous southern migration of Australian forms from Europe to Australia, which I have just mentioned as conceivable.

There are then the Antarctic types to account for; were they of more recent introduction than the European or Australian? Darwin has alluded to the possibility of these having been transported by icebergs from higher southern latitudes, during a period of greater cold than now obtains in the southern hemisphere (as the Scandinavian and Arctic plants are supposed by Forbes to have been transported to Britain, etc., during the glacial period), and, with the north European plants already in Australia, to have ascended the mountains during the subsequent rise of temperature. This would imply that Australia was, during a cold Tertiary period, simultaneously peopled by all those Antarctic, European, and Australian types which now inhabit it, but that the latter flora was much less developed in number of species and genera than now; for I cannot but regard the Antarctic flora in the same light as the European, and as a mere fragment of a much more extensive one, whose other members perished in the battle for place waged with the European and Australian during those changes of climate and level that succeeded their first introduction. The ultimate numerical ascendancy of the Australian botanical element may have been gained during the subsequent partition of the continent into archipelagos of islands, which became so many colonies of Australian types of vegetation, prepared on the final rise of the land to descend and occupy the intermediate ground. The paucity of alpine plants of Australian genera is a fact which lends itself well to this idea; it implies that, during either the rise of land or increase of temperature, the tendency of the species of Australian type was to seek warmer regions, and that the boreal and antarctic types being better suited to a colder climate prevented to a great extent the establishment of such varieties of Australian type as might otherwise have been adapted to inhabit the same climate as themselves.

When I take a comprehensive view of the vegetation of the Old World, I am struck with the appearance it presents of there being a continuous current of vegetation (if I may so fancifully express myself) from Scandinavia to Tasmania; along, in short, the whole extent of that arc of the terrestrial sphere which presents the greatest continuity of land. In the first place, Scandinavian genera, and even species, reappear everywhere from Lap-

land and Iceland to the tops of the Tasmanian alps, in rapidly diminishing numbers it is true, but in vigorous development throughout. They abound on the Alps and Pyrenees, pass on to the Caucasus and Himalaya, thence they extend along the Khasia mountains, and those of the peninulas of India to those of Ceylon and the Malayan archipelago (Java and Borneo), and after a hiatus of 30°, they appear on the alps of New South Wales, Victoria and Tasmania, and beyond these again on those of New Zealand and the Antarctic Islands, many of the species remaining unchanged throughout! It matters not what the vegetation of the bases and flanks of these mountains may be; the northern species may be associated with alpine forms of Germanic, Siberian, Oriental, Chinese, American, Malayan, and finally Australian and Antarctic types; but whereas these are all, more or less, local assemblages, the Scandinavian asserts his prerogative of ubiquity from Britain to beyond its antipodes.

Next in importance and appearance along the arc indicated is that flora which may be called Himalayan,* and which consists of the endemic plants of that range, with a mixture of Siberian, Caucasian, and Chinese genera; this, gathering strength in its progress southeastward along the ranges of northern and eastern India, occupies the flanks of all the mountain-chains I have enumerated between the Caucasus and Malay Islands; but there the Himalayan flora disappears, and does not reappear in Australia or New Zealand, and scarcely a trace of it is found in Polynesia.

The Malayan flora† is in many respects closely allied to the Himalayan, but is wholly tropical in character. This also very gradually appears in the valleys of the western and central Himalaya, and multiplying in genera and species in the eastern Himalaya and Khasia ranges, it sweeps down the Malayan peninsula, occupies all the Malayan Islands, and then it too stops short without entering Australia, being, however, continued eastward in tropical Polynesia..

Lastly, there is the flora of the plains and lower hills of India,‡ which is of a drier character than the Malayan, and is equally characteristic of Africa. This commences gradually in northwest India, or even in eastern Persia, and occupies all central India, the Gangetic plain, the whole of the Madras peninsula, except the western coast and mountains, the valley of the Irrawaddi, and the lower flat districts of the Malay Islands, whence it is continued in great force over the whole of tropical Australia.

* Characterized by *Cupuliferæ, Magnoliaceæ, Ternstræmiaceæ, Laurineæ, Balsamineæ, Ericeæ, Fumariaceæ*, etc.

† *Vaccineæ, Rhododendron, Begoniaceæ, Quercus:* and equally typified by *Cyrtandraceæ, Dipterocarpeæ, Myristiceæ, Anonaceæ, Menispermeæ.*

‡ It consists of *Acanthaceæ, Sterculiaceæ*, and other orders, enumerated at p. xliii, *et seq.* [in the original Essay].

Reversing the position, and beginning at the southern extreme of this arc of vegetation, there is first the Antarctic flora (the complement of the Scandinavian), with its decided Australian representatives in *Centrolepideæ* and *Stylidieæ*, commencing in Fuegia, the Falklands, and Lord Auckland's and Campbell's group, reappearing in the alps of New Zealand, Tasmania, and Australia, and disappearing under the equator, on the alps of Borneo, being thus strictly confined to the southern hemisphere. Next there is the Australian flora proper, a large and highly developed one, diminishing rapidly after crossing the southern tropic, and as it advances towards the northwestern shore of the continent, reappearing in very small numbers in the Malay Islands, and terminated by a *Casuarina* on the east coast of the Bay of Bengal, and a *Stylidium* on the west. Not one representative of this vegetation advances further northwest.

Analogous appearances are presented by Africa and America. In Africa Indian forms prevail throughout the tropics, and, passing southwards, occupy the northern boundary of the south temperate zone; but there a very copious and widely different vegetation succeeds, of which but few representatives advance north to the tropic, and none to India, but with which are mingled Scandinavian genera and even species. In the New World, Arctic, Scandinavian, and North American genera and species are continuously extended from the north to the south temperate and even Antarctic zones; but scarcely one Antarctic species, or even* genus (*Forstera*, *Calceolaria*, *Colobanthus*, *Gunnera*, etc. etc.) advances north beyond the Gulf of Mexico.

These considerations quite preclude my entertaining the idea that the southern and northern floras have had common origin within comparatively modern geological epochs. On the contrary, the European and Australian floras seem to me to be essentially distinct, and not united by those in intervening countries, though fragments of the former are associated with the latter in the southern hemisphere. For instance, I regard the Indian plants in Australia to be as foreign to it, botanically, as the Scandinavian, and more so than the Antarctic; and that to whatever lengths the theory of variation may be carried, we cannot by it speculate on the southern flora being directly a derivative one from the existing northern. On the contrary, the many bonds of affinity between the three southern floras, the Antarctic, Australian, and South African, indicate that these may all have been members of one great vegetation, which may once have covered as large a southern area as the European now does a northern. It is true that at some anterior time these two

* *Acæna* is a remarkable exception.

floras may have had a common origin, but the period of their divergence antedates the creation of the principal existing generic forms of each. To what portion of the globe the maximum development of this southern flora is to be assigned, it is vain at present to speculate; but the geographical changes that have resulted in its dismemberment into isolated groups scattered over the Southern Ocean, must have been great indeed. Circumscribed as these floras are, and encroached upon everywhere by northern forms, their ultimate destiny must depend on that power of appropriation in the strife for place which we see in the force with which an intrusive foreign weed establishes itself in our already fully peopled fields and meadows, and of the real nature of which power no conception has been formed by naturalists, and which has not even a name in the language of biology. Everywhere, however, we see the more widely distributed, and therefore least peculiar forms of plants, spreading, and the most peculiar dying out in small areas, and the progress of civilization has introduced in man a new enemy to the scarce old forms, and a strong ally of those already common. Nor can it be doubted but that many of the small local genera of Australia, New Zealand, and South Africa, will ultimately disappear, owing to the usurping tendencies of the emigrant plants of the northern hemisphere, energetically supported as they are by the artificial aids that the northern races of man afford them.

ART. XXVII.—*On the Coloring Matter of the Privet and its application in the Analysis of Potable Waters;* by Mr. JEROME NICKLÈS.

THE berries of the privet (*Ligustrum vulgare*), which are often employed in Europe to color wines, contain, besides water and ligneous matter, a portion of glucose, a waxy substance and a beautiful crimson coloring matter, which is the principal element. This matter is soluble in water, alcohol and ether; it contains no nitrogen, and is much more stable than many allied substances. When exposed to a sufficient heat it gives a black porous charcoal, but the uncharred portions remain unchanged. It was not altered by boiling for forty-eight hours with distilled water, nor by digestion during six weeks with sulphurous acid. The fixed alkalies and their neutral carbonates turn its color to green, but the red is restored by acids so that it may be employed as a delicate test in place of litmus or the coloring matter of the dahlia. With a solution of acetate of alumina it gives a violet blue liquid, from which by boiling a fine blue lake is precipitated, which is insoluble in acetic acid, but dissolves in tartaric, citric and mineral acids to a red liquid, from which alkalies

throw down again the blue lake. The basic, and even the neutral acetate of lead, yield with the red coloring matter of the privet a blue precipitate, which is soluble in acetic acid. Ammonia readily alters this coloring matter, giving rise to a yellow substance not well defined in its character. From these observations it would appear that the red coloring principle of the berries of the privet is a substance *sui-generis* and distinct from any hitherto known. I therefore propose to designate it by the name of *liguline*.

In order to obtain liguline in a state of purity, the filtered juice of the berries was precipitated by neutral acetate of lead, and the well washed lake suspended in a small quantity of water was decomposed by sulphuretted hydrogen. The residue was then thoroughly washed by ether, in which the liguline is insoluble.* Being taken up by alcohol, and again treated by acetate of lead, sulphuretted hydrogen and ether, it might be supposed to be pure. I was, however, unable to obtain concordant results in a series of elementary analyses, the carbon of the direct lead compound varying between 21·56 and 23·00 per cent, and the hydrogen from 1·89 to 2·58.

It is probable that the process described by Mr. Glénard for the preparation of *oenoline*, the red coloring matter of wines (*An. de Chim. et de Phys.*, Dec. 1858, p. 368), would be preferable for the extraction of liguline. I accordingly applied it, but the berries having been gathered too late in the season, the coloring matter had become so far altered that my trial was unsuccessful, so that the question of the elementary composition of liguline remains unsettled.

The following further observations on this coloring matter are not without interest. It is not precipitated by gelatine, which throws down the red coloring matter of wines. With hypochlorite of lime it gives a yellow color and a yellow precipitate. With chlorid of gold, a yellow color and reduction of the metal. With chlorid of platinum, no change in the cold, but a brown color by heat. With chromate of potash a green; with bichromate brown, and with sesquichlorid, and ferroso-ferric sulphate of iron the same color. Chlorine destroys the color of liguline. The chlorids of sodium, barium and mercury, the nitrates of baryta, lead, mercury and bismuth, as also the sulphates of starch, soda, lime, zinc, manganese and cadmium are without action on the coloring matter of the privet.

The bicarbonates of lime and of the alkalies (unlike the neutral alkaline carbonates which turn it to green) give a blue color with liguline, and the same is true of the chlorids and nitrates of zinc and calcium. The colors thus obtained offer however some

* The author has previously stated that the coloring matter is soluble in ether—there is apparently some error of the copyist.—Note of the Translator.

peculiar differences when seen by transmitted light; in this way the blue produced by a chlorid of zinc and bicarbonate of lime appears red, while it is green with the chlorid of calcium or the nitrate of lime or zinc. The blue color produced by a solution of bicarbonate of potash, on the contrary, offers no variation when thus viewed by transmitted light.

The recent juice of the berries of privet alters readily even when mixed with alcohol; its fine crimson color turns to red, and the liquid then mingled with a solution of bicarbonate of lime gives a gray instead of a blue color, and gives a dirty blue with acetate of lead. This change appears to depend upon the development of ammonia from the transformation of the azotized matter of the juice; when separated from these matters and isolated, on the contrary, liguline may be preserved without change, either in aqueous or alcoholic solution. Its color is then an intense crimson.

Even the strong mineral acids in the cold do not alter liguline, but in the presence of alkalies on the contrary, it is rapidly altered, although the red color can be, to a certain extent, restored by an acid. This alteration is dependent upon the absorption of oxygen, as may be shown by introducing a mixture of liguline and potash ley in a glass tube over mercury, when rapid absorption takes place.

The property of liguline to produce blue with solutions of bicarbonate of lime renders it a delicate reagent for the detection of this salt in potable waters. For this purpose it suffices to let fall a drop of an aqueous or alcoholic solution of liguline into the water, the crimson tint which this communicates to distilled water is replaced by a beautiful blue. In place of the solution we may employ a test paper impregnated with the coloring matter, which is best as prepared from the lead precipitate. We may, however, employ the recent juice of the berries, taking care to redden the paper slightly by exposing it to the vapor of acetic acid before drying.

As a reagent for the detection of bicarbonate of lime in waters, liguline is greatly to be preferred to a tincture of logwood, and the paper prepared with it becomes a valuable reagent for the laboratory as well as for the naturalist in the field. I have found by this reagent that while bicarbonate of lime is indicated in the springs which flow from the jurassic strata, and especially those that supply the city of Nancy, no change of color is produced by a solution of liguline with the waters of other streams which have their source in rocks destitute of calcareous matter.

The observations which I have given above were made for the most part with the fruit of the privet gathered in the autumn of 1856, and I have in fact indicated in a note in the Bulletin of the local Society of Acclimation for the North-West district

(Nancy, 1857, p. 121). I have delayed publication in the hope to render my research more complete by a good elementary analysis, but I am now induced to publish the results already obtained that I may claim the right to continue and complete the investigations, having learned that Mr. Glenard proposes to undertake a similar research.

In conclusion, we may remark that the coloring matter of the privet offers a great analogy with that of the wines of Villefranche isolated and examined by Mr. Glenard; this analogy is shown by their composition and their properties. Its reaction with bicarbonate of lime may render it a valuable reagent in chemical analysis. The fact that it is not precipitated by gelatine, which, as is well known, throws down the red coloring matter of wines, will serve to distinguish the two when associated. It still remains to be decided whether the coloring principle of all red wines is the same, but this is a question foreign to our present subject.

ART. XXVIII.—*On the Method of Measurements, as a diagnostic means of distinguishing Human Races, adopted by Drs. Scherzer and Schwarz, in the Austrian circumnavigatory Expedition of the "Novara";* by JOSEPH BARNARD DAVIS.

WEIGHT and measure have been very frequently applied as means to determine the physical proportions of different human races, and to ascertain their essential diversities. But it may well be doubted whether they have ever been employed in that systematic and comprehensive manner, which will afford the results they are capable of yielding. Travellers have generally contented themselves by speaking in indefinite comparative terms of the people with whom they have come into contact. But few have submitted any considerable number of these people to the test of measurement, and thus ascertained their dimensions. Anthropology stands in need of many more accurate and extended observations, to derive the full results from these sources of knowledge.

The subject itself is a large one, and some have confined themselves to one branch of it, some to others. Where actual measurements have been carried out, many have contented themselves with taking the *stature* of a few, or a number, of the people; others have, besides, ascertained the length of the *limbs;* and a few have subjected the *head* to a series of superficial measurements. As we are fully assured that this latter division of

the body is the seat of those faculties which lie at the base of all the peculiarities of human races; bearing essentially and intimately upon their manners and customs, all their institutions, their religious impulses; their capacity for civilization, and the development to which it has attained, it is not surprising that it should have attracted the chiefest attention. Besides the superficial measurements of the head, a more extensive series of observations has been made upon the bony skull itself, with a view of determining its relative proportions, for comparison in the same race, or among different races. Many observers, advancing a step nearer, have endeavored to ascertain, by measure and by weight, the *internal capacity* of this marble palace. And, lastly, some have laboriously devoted their inquiries to the great central mass of the nervons system, and availed themselves of the opportunities that have occurred to them, to determine the size and the weight of the brain, and its different parts. As this last investigation comes nearest of all to the specialties of human beings—who are so finely discriminated by Professor Owen, as *archencephala*—it is to be regretted that the occasions for research among distinct races are so few, and have been so little availed of, and the investigation itself is so elaborate and nice, that hitherto this most interesting part of anthropological anatomy is, as it were, a *tabula rasa*, to use the language of one of the most laborious inquirers in this branch of science—Prof. Huschke, of the University of Jena. It is, however, fortunate that gauging the internal capacity of the skull should afford the means of so accurate an approximation to the volume and the weight of the brain; and thus, for the comparison of these important points among the different families and tribes of men. Hence, the labors of Tiedemann, the distinguished physiologist, who, with a very amiable design, undertook to show that the brain of a negro was not smaller than that of the European—an attempt similar to that of the late Sir William Hamilton. Tiedemann might have succeeded in impressing us with his own conclusion, had he not published the tables on which this conclusion was based, and which themselves refute such an erroneous opinion. To Tiedemann succeeded Professor Morton, of Philadelphia, Professor Van der Hoeven, of Leyden, and others. Among the most recent, is Prof. Huschke, of Jena, one of whose results of whose own estimation of the capacity of the skull, and of the size of the brain, is, that the Germanic races, among whom through our Anglo-Saxon forefathers we rank, as one great branch, have the largest brains of any people. They distinctly exceed the French in this respect.

That great diversities, capable of metrical appreciation, prevail among human races is very well known. Some of the tribes of North American Indians are remarkable for their great stat-

ure. Catlin assures us that the men among the *Crows*, whose hair will frequently reach the calves of their legs, are most of them six feet or more. Other tribes are of a decidedly lower stature. Of the gigantic Patagonians of South America, the most extravagant accounts have been given by travellers. But Capt. King affirms them, upon measurement, to be from 5 feet 10 inches to 6 feet high, which is supported by the statement of M. D'Orbigny, that some are 6 ft. 3½ inches, and the medium stature is above 5 ft. 8 inches English. On the contrary, the average height of the Bushmen is only 4 feet 4 inches. This gives a range of very nearly two feet between the tallest and the shortest races of men we are acquainted with. The other races of mankind are comprised within these limits of difference. Some tribes of the Negritos average about 4 feet 8 inches; the so-called Malay races, ascending to a mean of 5 feet 3 inches. But among the Negrito tribes of the Pacific there is, as that eminent ethnologist, Mr. Crawford, has clearly shown, a great diversity of stature. They dwell in islands scattered over a large extent of ocean, and although some tribes do not reach 5 feet in height, others, as those of New Caledonia, attain to 6 feet, and individuals among them even more. In the recent expeditions to the Andaman Islands, for the purpose of selecting a spot for a penal settlement, the inhabitants are spoken of as "dwarf Negrillos," and as "men of middle size." An individual who was measured, gave a stature of 4 feet 9½ English inches. (Selections from the Records of the Government of India, No. xxv: the Andaman Islands.) Thus, in stature alone, a very great diversity prevails. And it is remarkable that tribes in close proximity to each other frequently exhibit startling contrasts. Dr. Livingstone, whose opportunities had he been an ethnologist were so extraordinary, observed in the plains of the interior of Southern Africa, scattered among the Kafirs, who are a tall, fine and robust race, the hordes of the diminutive Bushmen. He was deeply impressed with what he saw, so contrary to all his preconceptions; and expresses his great surprise that such dissimilar races should be everywhere scattered about the country without being mingled, where they have dwelt for unlimited ages, exposed to all the same influences of air, climate, food, &c. The tall Patagonians and some tribes of the Fuegians, distinguished for their dwarf stature, afford a similar example of contrast.

The brothers Schlagintweit, following in the train of Mr. Hodgson, carried on an extensive series of metrical observations on the tribes of the Himalaya and of India. Many curious results, chiefly pointing to the different proportions of parts of the bodies and limbs of these people from those of Europeans, have been attained, which will be published in the ethnological portion of their projected work. After ascertaining the weight of

the individual and his strength, by means of the dynamometer, they made from 25 to 28 different measurements, chiefly of the head, and of other parts of the body and limbs. But Drs. Scherzer and Schwarz have striven, by a more complex and complete system of observation and measurement, to gain an image of the size and form of the individual, and of all his parts;—thus not merely to subserve the purposes of the anatomist, the physiologist and the ethnologist, but those of the artist also. Their more ambitious object of obtaining, in this way, to a natural classification of human races, is an evidence of laudable zeal; but we can hardly hope that their labors can do more than contribute towards the solution of this difficult problem. Although, it ought to be mentioned, that the late Baron Humboldt, a short time before his death, expressed his great satisfaction with the system of measurements of Drs. Scherzer and Schwarz; by which, he thought, we may at length arrive at a safer result in distinguishing and determining human races than by any other means.

After recording the age, weight, height, strength, color of the hair and eyes, and number of the pulsations of the radial artery, they divide their measurements into three sections, those of the *head,* the *trunk*, and the *extremities;* and of these they take no less than 70 different dimensions in all, by means of different instruments.

Their external measurements of the head are the most complete that have ever been employed. They embrace the face as well as the other parts of the head, and by means of a perpendicular line with plummet, and a small metre scale, they are able to ascertain pretty correctly the profile of the countenance. The number of their different measurements of and about the *head*, consisting of superficial distances, diameters, circumferences, &c. amounts to 31, those of the *trunk* to 18, and those of the *extremities* to 21.

When the frigate "Novara" reached Sydney, these gentlemen printed an account of their system of measurements, "for private circulation" among men of science, which is preceded by a number of ingenious observations. In these, they dwell upon the ease with which travellers intuitively discriminate the different nations and tribes of mankind; and yet the difficulty in some selected individuals and cases to carry out this diagnosis, especially when the eye is deceived by a substitution of dress; and express great confidence in a more minute examination by a systematic method of measurements. They insist with equal confidence that nature must recognize a definite plan by which man's different types are formed and distinguished; and conclude that we should dedicate the same amount of study and inquiry to the systematic arrangement of our own species, as has

long been applied to thousands of species of the vegetable and animal kingdoms.

In the course of these introductory remarks they mention their examination of the Chinese inmates of the prison at Hong Kong. Among these they found persons belonging to the *Hakka Tribe*, with stout and vigorous constitutions, fine, well-shaped, aquiline or long and straight noses, and a form of the eyes not resembling the specific obliquity of other Chinese. As criminals, they had been deprived of their tails, and Drs. Scherzer and Schwarz affirm that they had such a resemblance to the figures of some Europeans of the lower class, that, by a change of dress, they might pass amongst us without being recognized. They also mention how successfully Gützlaff, Medhurst, Huc and others have travelled the Empire in a Chinese dress without detection. And, no doubt, there are individuals so capable of assuming, and, as it were, substituting, the manners and expressions of others that the ordinary and slight attention which is paid to persons on a journey and among numbers, does not suffice to discriminate them.

Still, *the rule* must run counter to such a confusion; or the statement of the Austrian voyagers could not be true—that an anthropologist on the Island of Java is able, at first view, to classify most of the Malay tribes inhabiting the larger and smaller Islands on the Indian Archipelago, without ever mistaking. And the very remarkable account of the Abbé Huc proves that if there are differences among the races of men too subtle to be detected by the eye, yet they are not the less certainly appreciable. He informs us that he and his companion successfully eluded the detection of the unsuspecting or inattentive Chinese, but that to the Chinese *dogs* they always stood at once revealed as Europeans, by their peculiar smell. "The dogs barked continually at us, and appeared to know that we were foreigners." This is not the proper time to refer to the distinguishable odors of the different races of mankind, which travellers allude to. Huc said he could easily distinguish those of the Negro, the Malay, the Tartar, the Thibetan, the Hindoo, the Arab and the Chinese. Indeed, it is the same, with those having a delicate sense of smell, as to the French and other European races. And with respect to the fact of the penetrating and offensive scent attached to man, more especially to civilized man, Mr. Galton and others, who have traversed desert countries teeming with wild animals, give distinct and prominent testimony—which testimony is, in truth, not very complimentary to us.

We have been informed, on the authority of one who has seen much of the North American Indians, that they describe an odor to them peculiarly disgusting as being attached to the Jews. A fact, which, if correct, is little accordant with the ex-

traordinary hypotheses which would derive the Indians themselves from the lost tribes.

Finally, it may be mentioned, that by a recent communication from Dr. Scherzer we are informed that during the cruise of the "Novara," about 200 individuals of different races, but of about the same age, males and females, were subjected to measurement. The whole number of measures taken amount to nearly 12,000. Dr. Scherzer adds, that he does not consider these observations sufficient, but merely as a commencement of a system of thorough metrical examination;—that the paper on measurements has been translated into different languages, and copies of it left in the hands of physicians, and other men of science, in the different places and islands visited by the expedition, who promised to complete the observations on the aborigines, and to forward the results to Europe;—that the measurements already effected embrace those made on Negroes, Malays, Mongols, Papuans and Indians;—and that the greatest number were taken on individuals in the Nicobar Islands, Batavia, where natives of almost all the islands of the Indian Archipelago were met with; Manilla, Hong Kong, Sydney (Austral negroes), New Zealand, Tahiti (where were aborigines of New Caledonia and Norfolk Island), Chili and Peru.

The results obtained by the extensive series of measurements thus procured, will shortly be published to the world, in the volumes now in preparation at Vienna. The History of the important Voyage of the "Novara," a popular illustrated work, from the Journals of the Commanders, Commodore Wüllustorf and Dr. Scherzer, may be expected to be issued from the Imperial printing office, in Vienna, to be followed by an English translation, in the early part of the present year. It is proposed that this shall be succeeded by a number of other volumes on distinct subjects. 1. Those on nautical, astronomical, meteorological, magnetical, and other observations relating to Physical Geography, by Commodore Wüllustorf. 2. Geology, by Dr. Hochstetter. 3. Zoology, by Messrs. Frauenfeld and Zelebor. 5. Ethnography, by Dr. Scherzer. 6. Statistics and Natural Economy, by the same. 7. Medicine, (Pathological and Pharmacognostical Researches,) by Dr. Schwarz. And, lastly, 8. an Album selected from nearly 2,500 sketches made by Mr. Sellery, the artist of the expedition. Whenever this grand programme, which will have the best wishes of men of science in all countries, shall have been completed, the rich results of the first Austrian Circumnavigatory Expedition, placed as it has been in able and well instructed hands, will, we have no doubt, vindicate the national character in a new and much nobler field of enterprise; and give to that country a far more lasting and more dignified fame than any she has hitherto required.

Note.—Such is the inconvenience resulting from the use of a variety of metre scales, and such a number of methods of measurement, frequently taking quite different points for measures bearing the same name, as in the case of the skull especially, that the distinguished Professor Von Bær, of St. Petersburg, has just proposed a Congress of Anthropologists, to determine upon one uniform scale and to establish one system. By this means, all the results of measurements of the human body would be rendered of universal applicability.—*Nachrichten über die ethnog. craniol. Sammlung zu St. Petersburg. S.* 81.

ART. XXIX.—*Report of Assistant Charles A. Schott, on the latest results of the Discussion of the Secular Change of the Magnetic Declination, accompanied by tables showing the declination (variation of the needle) for every tenth year from the date of the earliest reliable observations, for twenty-six stations on the Atlantic, Gulf, and Pacific coasts of the United States.*

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IN accordance with the Superintendent's letter of January 21, 1859, I have prepared a set of tables for practical use, giving the secular change of the magnetic declination and showing for every tenth year, from the date of the earliest reliable observations to the present time, the magnetic declination (commonly called the variation of the magnetic needle) for stations on or near the northeastern coast of the United States and also for some stations on our southern and western coasts—as derived from my several discussions of the secular change in which have been included the latest data in possession of the Coast Survey. For the eastern and southern coasts, the following papers may be referred to: Coast Survey report for 1855, Appendix No. 48, pp. 306–337; Coast Survey report for 1858, Appendix No. 25, pp. 192–195, and Appendix No. 26, pp. 195–197. For the western coast, Coast Survey report for 1856, Appendix No. 31, pp. 228–235 may be consulted.

In general the secular change of the declination appears to be of a periodic character, but in no instance has a whole cycle been completed on either coast. Its length therefore remains necessarily in a great measure uncertain, and the tentative analytical process so far followed has for its main object the proper representation of all reliable observations made at any one station, so as to furnish the means of interpolation and also to enable us to calculate the magnetic declination for any required place and date, within the limits of the discussion. In the in-

vestigation of 1855 a linear function was used in the discussion which does not involve the duration of the period, and on this account the results were, in regard to time, of rather limited extent (see remark on p. 337 of Report for 1855). For the western coast stations, I still prefer to retain this form of the discussion. Subsequently, by means of the knowledge gained in that discussion, an attempt was made to substitute a circular function, directly involving a period or periods, the length of which, as well as all other numerical co-efficients in the formula for the secular change, has been determined by applying the method of least squares. The use of a circular function—commenced in 1858 with two stations, is now extended to eighteen, within the limits stated above, and it has been applied to some stations in Canada, the southern coast of the United States and Central America, in order to furnish material for the generalization of the law, so far as ascertained, in reference to epochs and rates of change. A secondary period within the first was traced at several stations, its length, however, being much more variable and uncertain, was found fluctuating between one-half and one-fifth of the primary period, while its amplitude was on the average fifteen times smaller than that of the primary wave for stations forming group 1, or within the geographical limits of Portland, Burlington and Williamsburg. This smaller amplitude was found nearly constant and equal to $0°{\cdot}4$.

To make the present paper more complete it contains also the record of all observations used in the discussion not heretofore published in the Coast Survey reports.

As long as the cause producing the secular change remains altogether unknown, it is not safe to trust too far to the continuation of the law thus empirically derived, and in the following tables no value, deduced by the formula, has been inserted antecedent to the first observation by more than ten years. The tabular values may therefore be regarded in the light of a strict interpolation between actual observations, and since the analytical treatment will equalize and remove, in a measure, accidental errors of observation, they may be considered as certainly more trustworthy than any single observation, particularly in cases where the number of observations available for the discussion exceeds half a dozen, properly distributed in relation to time. The probable error of any single representation will be found in the second table. For all ordinary use by the surveyor (or navigator) the tabular values are sufficiently precise, when greater accuracy is required the annual inequality of the declination and the diurnal variation for the time required must be taken into account; the former correction will probably not exceed, in any case, one minute, and the latter may amount in summer, in maximo, to minus or plus six minutes, and in winter to

minus or plus three minutes, numbers which were derived from Prof. Bache's discussion of the Philadelphia observations. The tables will also answer for intermediate places, for which they furnish the necessary data of interpolation.

It is proper to state that the present formulæ should be considered as liable to future changes and improvements depending on the accumulation of additional observations, and it is hardly necessary to state that their number also may hereafter be considerably increased by the accession of new material. The utility of a publication of tables showing the declination for every tenth year was suggested by Mr. T. B. Brooks. In the numerical calculations I was assisted by Mr. G. Rumpf of the Computing Division.

Formulæ expressing the secular change of the magnetic declination (commonly called variation of the magnetic needle) used for calculating the tabular values.—Group 1. Stations between Portland, Me., and Williamsburg, Va.

A positive sign of D indicates west declination, a negative sign, east declination. n equals the number of years (and fraction of a year) from 1830; positive for years after and negative for years before this epoch. Longitudes are reckoned from Greenwich.

No.	Locality.	Lat.	Long.	
1	Burlington, Vt.	44° 27′	73° 10′	$D=+11'.55-4'.10\cos(1°.30n+36°)+0'.21\cos(7°.2n+290°)$
2	Portland, Me.	43 39	70 16	$D=+10'.70-2'.63\cos(1°.33n+87°)$
3	Portsmouth, N. H.	43 05	70 43	$D=+10'.20-2'.45\cos(1°.37n+72°)$
4	Rutland, Vt.	43 36	72 55	$D=+9'.89-3'.66\cos(1°.5n+45°)$
5	Cambridge, Mass.	42 23	71 07	$D=+9'.65-2'.78\cos(1°.30n+71°)+0'.22\cos(2°.7n+220°)$
6	Newburyport, Ms.	42 48	70 49	$D=+9'.55-2'.56\cos(1°.4n+78°)$
7	Boston,	42 20	71 02	$D=+9'.16-2'.55\cos(1°.39n+76°)+0'.22\cos(3°.6n+222°)$
8	Providence, R. I.	41 50	71 24	$D=+9'.11-2'.99\cos(1°.45n+58°)+0'.19\cos(7°.2n+246°)$
9	Hartford, Conn.	41 46	72 40	$D=+8'.60-3'.59\cos(1°.25n+45°)$
10	New Haven, Conn.	41 17	72 55	$D=+8'.13-3'.49\cos(1°.33n+39°)$
11	Albany, N. Y.	42 39	73 43	$D=+7'.65-2'.74\cos(1°.42n+62°)$
12	Oxford, N. Y.	42 27	75 42	$D=+6'.65-3'.69\cos(1°.3n+40°)$
13	New York,	40 43	74 00	$D=+6'.47-2'.32\cos(1°.6n+55°)$
14	Philadelphia,	39 58	75 10	$D=+5'.37-3'.44\cos(1°.6n+39°)$
15	Hatboro', Pa.	40 07	75 08	$D=+5'.23-3'.28\cos(1°.54n+47°)+0'.22\cos(4°.1n+347°)$
16	Baltimore,	39 16	76 35	$D=+2'.70-2'.25\cos(1°.5n+49°)$
17	Washington, D. C.	38 53	77 00	$D=+2'.42-2'.0\cos(1°.5n+49°)$
18	Williamsburg, Va.	37 15	76 40	$D=+2'.22-2'.6\cos(1°.5n+22°)$

The following table contains the number (n) of observations (single or combined) upon which each formula is based; the probable error (E_o) of an observation expressed in minutes, as a measure of the degree of accuracy with which the observations are represented; the epoch of the last minimum of west declination (or of maximum east declination) together with the least west declination (greatest east), and lastly, the annual variation

for the years 1840, 1850, and 1860, expressed in minutes. The positive sign expresses west declination increasing (east diminishing).

Locality.	n	E_o	Epoch of min. W. decl'n.	Least W. decl'n.	Annual change. v1840	v1850	v1860
		′		°	′	′	′
Burlington, Vt.	9	± 5	1813	+7·4	+4·1	+3·4	+4·5
Portland, Me.	5	14	1765	+8·1	+3·6	+3·4	+3·0
Portsmouth, N. H.	4	10	1777	+7·7	+3·5	+3 5	+3·2
Rutland, Vt.	4	18	1800	+6·2	+4·9	+5·5	+5·7
Cambridge, Mass.	22	12	1782	+6·9	+4·3	+4·3	....
Newburyport, Mass.	4	12	1774	+7·0	+3·7	+3·6	+3·3
Boston,	8	10	1782	+6·7	+4·5	+4·3	+3·7
Providence, R. I.	30	5	1779	+6·1	+5·3	+3·8	+3·0
Hartford, Conn.	6	14	1794	+5·0	+4·0	+4·4	+4·6
New Haven,	14	10	1801	+4·6	+3·8	+4·4	+4·7
Albany, N. Y.	10	3	1787	+4·9	+3·9	+4·0	+3·9
Oxford, N. Y.	10	11	1799	+3·0	+4·0	+4·6	+4·9
New York,	13	13	1795	+4·1	+3·7	+3·9	+3·8
Philadelphia,	11	16	1805	+1·9	+4·7	+5·3	+5·4
Hatboro, Pa.	18	5	1796	+1·8	+4·2	+4·3	+4·4
Baltimore,	3	13	1798	+0·5	+3·2	+3·4	+3·4
Washington, D. C.	6	8	1798	+0·4	+2·8	+3·1	+3·1
Williamsburg, Va.	3	15	1815	-0·4	+2·4	+3·2	+3·7

Table of magnetic declinations for eighteen stations forming group 1, *on or near the northeastern coast of the United States, between the years* 1680 *and* 1860. *West declination is indicated by a plus sign, east declination by a minus sign, and is expressed in degrees and fractions of a degree.*

Year.	Burlington, Vt.	Portland, Me.	Portsmouth, N. H.	Rutland, Vt.	Cambridge, Mass.	Newburyport, Mass.	Boston.	Providence, R. I.	Hartford, Conn.
	°	°	°	°	°	°	°	°	°
1680	.....	.....	.....	.....	.....	.....	.....	.....	.....
1690	.....	.....	.....	.....	.....	.....	.....	.....	.....
1700	.....	.....	.....	.....	+ 9·9	.....	+ 9·7	.....	.....
1710	.....	.....	.....	.....	9·4	.....	9·0	+10·4	.....
1720	.....	.....	.....	.....	8·8	.....	8·3	9·5	.....
1730	.....	.....	.....	.....	8·4	.....	7·8	8·9	.....
1740	.....	.....	.....	.....	7·9	.....	7·4	8·3	.....
1750	.....	.....	.....	.....	7·5	.....	7·2	7·7	.....
1760	.....	+ 8·1	.....	.....	7·2	.....	7·0	6·9	.....
1770	.....	8·1	+ 7·8	.....	7·0	+ 7·0	6·8	6·8	.....
1780	.....	8·3	7·7	.....	6·9	7·0	6·8	6·1	+5·2
1790	+ 7·8	8·5	7·9	+6·3	6·9	7·2	6·8	6·3	5·0
1800	7·5	8·9	8·1	6·2	7·1	7·5	7·0	6·4	5·0
1810	7·3	9·4	8·5	6·3	7·5	7·9	7·3	6·5	5·2
1820	7·6	10·0	8·9	6·7	8·0	8·4	7·8	6·8	5·6
1830	8·30	10·6	9·4	7·3	8·58	9·0	8·41	7·46	6·1
1840	9·07	11·2	10·0	8·1	9·28	9·6	9·13	8·38	6·7
1850	9·69	11·8	10·6	8·9	10·0	10·3	9·88	9·14	7·4
1860	+10·30	+12·3	+11·2	+9·9	+.....	+10·8	+10·56	+ 9·68	+8·1

Year.	New Haven, Conn.	Albany, N. Y.	Oxford, N. Y.	New York.	Philadelphia.	Hatboro', Pa.	Baltimore.	Washington, D. C.	Williamsburg, Va.
	°	°	°	°	°	°	°	°	°
1680	.....	.....	.....	+8·8	.....	+8·5	.....	.....	.....
1690	.....	.....	.....	8·7	.....	8·3	.....	.....	+4·8
1700	.....	.....	.....	8·5	+8·8	7·9	.....	.....	+4·8
1710	.....	.....	.....	8·0	8·4	7·5	.....	.....	.....
1720	.....	.....	.....	7·6	7·9	7·0	.....	.....	.....
1730	.....	.....	.....	7·0	7·1	6·3	.....	.....	.....
1740	.....	.....	.....	6·4	6·3	5·6	.....	.....	.....
1750	.....	.....	.....	5·8	5·3	4·7	.....	.....	.....
1760	+6·1	.....	.....	5·2	4·4	3·8	.....	.....	.....
1770	5·5	.....	.....	4·7	3·5	2·9	.....	.....	+1·2
1780	5·0	.....	.....	4·4	2·8	2·2	.....	.....	+0·7
1790	4·8	.....	+3·0	4·2	2·2	1·8	.....	.....	+0·2
1800	4·6	.....	3·0	4·2	2·0	1·8	.....	+0·4	−0·2
1810	4·7	+5·4	3·1	4·3	1·9	2·1	+0·6	0·5	−0·4
1820	5·0	5·8	3·4	4·7	2·2	2·6	0·8	0·8	−0·4
1830	5·42	6·3	3·82	5·16	2·70	3·20	1·2	1·1	−0·2
1840	5·98	7·0	4·43	5·73	3·41	3·89	1·7	1·5	+0·1
1850	6·71	7·7	5·15	6·37	4·25	4·61	2·4	2·0	+0·6
1860	+7·46	+8·3	+5·95	+7·01	+5·19	+5·32	+2·9	+2·6	+1·2

Note.—At Cambridge, Mass., the observations after 1855 require farther examination. At Williamsburg, the values between 1700 and 1770 were not considered sufficiently reliable for insertion. The expression for Baltimore depends for length of period and time of minimum on the Washington formula.

The total number of observations upon which the tabular values and the formulæ are based is 180, the average number of any one station is 10, and the average probable error of any single representation is $\pm 11'$.

If we arrange the stations geographically, we find that at the eastern stations the minimum (west) declination occurred earlier than at the more western and southern stations; thus, from six stations between Portland and Providence it occurred about the year 1777; in the Connecticut and Hudson valleys and along the sea-coast as far south as Washington, the year of the minimum does not differ much from 1797; Williamsburg in Virginia gives 1815. The transition as we pass from the New England states is somewhat abrupt, but too well marked to be accidental. Extending the investigation farther north, I find for Quebec, Canada, the year of the minimum 1769; going farther west we find that at Toronto it must have occurred before the year 1842, and at York Fort, Hudson Bay, I find the year 1842 (as already ascertained by General Sabine, after the receipt of Capt. Blakiston's observations of 1857). This latter station is nearly halfway across the continent, and if we proceed to the western coast we find that the eastern declination there has not yet reached its maximum (equivalent to a western minimum), but it is highly probable that it will reach it before the close of the

present century. The present reverse or western motion of the isogonic lines in our eastern states which commenced about the year 1777 will gradually be communicated to the more westerly stations, and will, it is highly probable, be participated in our western coast before or at the close of the present century, the direction of the motion in this latter locality being at present still to the eastward and southward, though with a diminishing rate (see p. 235 of C. S. report of 1856).

The following equations constructed for the two northernmost stations may be added here:

York Fort, Hudson Bay, $D=+5°{\cdot}1-14°{\cdot}2\cos(1°{\cdot}6n+340°)$
Quebec, Canada, $D=+12\cdot84-3\cdot7\cos(1\cdot6n+97)$

The second group comprises the stations on the southern portion of the Atlantic coast and Gulf coast; only three in number, to which have been added some stations located further south.

Group II. *Southern Stations.*

No.	Locality.	Lat.	Long.	Magnetic declination.
1	Charleston, S. C.	32 45	79 51	$D=-2°{\cdot}12-2°{\cdot}02\cos(1°{\cdot}55n+56°)$
2	Savannah, Ga.	32 05	81 05	$D=-2°{\cdot}95-1°{\cdot}24\cos(1°{\cdot}5n+20)$
3	Mobile, Ala.	30 41	88 02	$D=-6°{\cdot}5-0°{\cdot}77\cos(1°{\cdot}6n+16)$

Locality.	n	E_0	Epoch of max. east declination.	Max. east declination	Annual change.		
					1840	1850	1860
Charleston, S. C.	5	± 9′	1794	−4°·1	+3′·1	+3′·2	+3′·2
Savannah, Ga.	4	12	1817	−4·2	+1·1	+1·5	+1·8
Mobile, Ala.	6	12	1820	−7·3	+0·7	+0·9	+1·1

Proceeding in a southerly direction the next station discussed outside of the boundaries of the United States is Havana, Cuba, lat. 23° 09′, long. 82° 22′, for which place I found $D=-4°{\cdot}82-1°{\cdot}45\cos(1°{\cdot}3n+26°)$ with 1810 as the year of maximum east declination. The values collected for Jamaica were not discussed, but the nine values I was able to obtain, will be found in the appended record. For Panama, New Granada, lat. +8° 57′, long. 79° 29′, the southernmost station discussed, I find $D=-6°{\cdot}9-1°{\cdot}04\cos(1°{\cdot}2n+74°)$, an equation satisfying the observations, but not considered as preferable to the following expression, $D=-5°{\cdot}57-2°{\cdot}21\cos(1°{\cdot}2n+34°)$, which supposes the maximum to occur in 1802.

Going westward and northward I found for Vera Cruz, Mexico, lat. 19° 12′, long. 96° 09′, $D=-4°{\cdot}2-5°{\cdot}04\cos(1°{\cdot}1n+7°)$, with the maximum east declination in 1824.

The following table has been calculated from the preceding equations:

Year.	Charleston, S. C.	Savannah, Ga.	Mobile, Ala.
	°	°	°
1770	−3·7	....	....
1780	−4·0	....	....
1790	−4·1	....	....
1800	−4·1	−4·1	−7·1
1810	−4·0	−4·2	−7·2
1820	−3·6	−4·2	−7·3
1830	−3·2	−4·1	−7·2
1840	−2·8	−4·0	−7·1
1850	−2·2	−3·7	−7·0
1860	−1·7	−3·5	−6·8

The following formulæ for stations of the western coast, between San Diego and Cape Disappointment, forming group 3, have been copied from p. 234 of the report for 1856.

No.	Locality.	Lat.	[Long.]	
		° ′	° ′	
1	San Diego,	32 42	117 13	$D=-12°17-0·019n+0·00018n^2$
2	Monterey,	36 38	121 54	$D=-14·19-0·050n+0·00047n^2$
3	San Francisco,	37 48	122 27	$D=-15·14-0·028n+0·00025n^2$
4	Cape Mendocino,	40 25	124 22	$D=-16·29-0·029n$
5	Cape Disappointment,	46 17	124 02	$D=-19·65-0·019n$

The total number of observations used for the construction of the above formulæ is 21, the greatest number for any one station being 6, the least 3, the average probable error of any single representation is ±12′. The annual change (increasing east declination) may be taken the same for all stations, viz., in 1840, −1′·6, in 1850, −1′·2, in 1860, −0′·8.

Year.	San Diego.	Monterey.	San Francisco.	Cape Mendocino.	Cape Disappointment.
	°	°	°	°	°
1790	−11·1	−11·4	−13·6	−15·1	−18·9
1800	11·4	12·3	14·1	15·4	19·1
1810	11·7	13·0	14·5	15·7	19·3
1820	12·0	13·6	14·8	16·0	19·5
1830	12·2	14·2	15·1	16·3	19·7
1840	12·3	14·6	15·4	16·6	19·8
1850	12·5	15·0	15·6	16·9	20·0
1860	−12·6	−15·3	−15·8	−17·2	−20·2

The next station discussed, south of California, is San Blas, Mexico, lat. 21° 32′ N., long. 105° 16′ W. of Gr., which gave the following expression (see p. 234, C. S. report for 1856),

$$D=-8°·63-0·042n-0·00031n^2,$$

which equation, when compared with those above, shows a reversal in the sign of the coefficient of n^2 or an opposite curvature. The annual easterly increase at San Blas in 1850 according to the above formula was 3′·3. This station, however, is already within the area of the peculiar form of the isogonic lines, which position may possibly render an immediate compar-

ison impracticable. The station Sitka, in Russian America, is the next place discussed north of Washington Territory. I find for it the approximate formula,

$$D = -28°{\cdot}12 - 0{\cdot}0607n - 0{\cdot}00025n^2.$$

It depends for its latest declination (1858) on the tabular value assigned by Mr. Evans on his late map of the lines of equal magnetic variation reduced to 1858.

Record of all observed declinations made use of in the above paper, not heretofore published in the U. S. Coast Survey reports.

The following record, containing only additional observations, we have to consult the preceding reports of 1854,* 1855–'56, and '58, if we desire to collect all results which may have been used at any one station. The stations are arranged geographically, commencing with the northern and eastern stations and concluding with the stations on the western coast. D= observed declination.

York Fort, Hudson Bay.

From the Proceedings of the Royal Society of London for January 7, 1858, by Major Gen. Sabine.

1725,	D=	19° 00′	W.	Capt. Middleton.
1787,		5 00	"	Hansteen's map.
1819, Sept.,		6 00	E.	Sir J. Franklin.
1843, July,		9 25	"	Capt. Lefroy.
1857, Aug.,		7 37	"	Capt. Blakiston.

Quebec, Canada.

1649,	D=	16° 00′	W.	P. Bressau, Hansteen's Erdmag's, Barlow Cycl. Met.
1686,		15 30	"	DeHayes, " " "
1810,		11 00	"	Becquerel, traité du magnetisme.
1814,		11 50	"	Kent, Becquerel traité du magnetisme.
1831,		13 38	"	Bayfield, " " " "
1842,		14 12	"	Capt. Lefroy.
1859, July,		16 17	"	Chas. A. Schott, Asst. U. S. Coast Survey.

Burlington, Vt.

See former observations in 1855 report, pp. 326–337.

1837,	D=	8° 45′	W.	Prof. Benedict.
1840,		9 42	"	J. Johnson, Thompson's Hist. of Vermont.
1845, June,		9 22	"	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.

Portland, Me.

1763,	D=	7° 45′	W.	J. Winthrop, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1775,		8 30	"	J. F. DeBarre's Atlantic Neptune, London, 1781.

* The table of the declinations in that report is reprinted and enlarged in the report of 1855.

1845, June,		11	28 "	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1859, July,		12	20 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Portsmouth, N. H.

1771,	$D=$	7°	46′ W.	Holland, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1771,		7	48 "	Holland.
1775,		7	45 "	J. F. DeBarre's Atlantic Neptune.
1859, July,		11	15 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Rutland, Vt.

1789, April,	$D=$	7°	03′ W.	Dr. Williams, Sill.'s Journal, xvi, 1829.
1810, May,		6	04 "	" " " " " "
1811, Sept.,		6	01 "	" " " " " "
1859, July,		9	49 "	Chas. A. Schott, Asst. U. S. Coast Survey.

Cambridge, Mass.

See pp. 317–318 of C. S. report of 1855, also C. S. report, 1856, p. 222.

1845, June,	$D=$	9°	32′ W.	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1855, May,		10	54·6 "	W. C. Bond (in a letter to Supt. of C. S.)
1856, May,		10	50·3 "	" " " "
1856, July,		10	06 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Note.—More recent observations still require examination.

Newburyport, Mass.

1775,	$D=$	6°	45′ W.	J. F. W. DeBarre's Atlantic Neptune.
1781,		7	18 "	Dr. Williams, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1859, July,		10	58 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report, 1856, p. 215.

Boston, Mass.

See C. S. report, 1855, pp. 316, 317–337.

Providence, R. I.

See C. S. report, 1855, pp. 307, 308, 309, 337.

Hartford, Conn.

1786,	$D=$	5°	25′ W.	Dr. Williams,	Prof. Loomis's collection in Sill.'s Journal, vol. xxxiv, 1838.
1810,		4	46 "	Asher Miller,	
1824,		5	45 "	N. Goodwin,	
1828,		6	03 "	"	
1829,		6	03 "	"	
1859, July,		8	04 "	An interpolated value from observations at Springfield and New Haven in 1859 and 1855.	

New Haven, Conn.

See C. S. report, 1855, pp. 319, 320, 337.

Albany, N. Y.

1847, Nov.,	$D=$	7°	35′ W.	Regent's report (geological survey).

1856, Sept., 8 35 " Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

See also C. S. report, 1855, pp. 328–337, and C. S. report, 1858, p. 191.

Oxford, N. Y.

The following observations marked E. B. W. C. are from a letter of Mr. E. B. W. Call to the Superintendent C. S. Dec. 22, 1858.

1792–95, $D=$ 3° 00′ W. E. B. W. C.
1817, 3 00 " "
1828, July, 4 30 " "
1834, Oct., 3 52 " Regent's report, Sill.'s Jour., xxxiv, 1838.
1836, Oct., 4 09 " " " " " "
1838, July, 4 30 " " " observed at Guilford.
1849, Nov., 5 11 " E. B. W. C.
1857, April, 5 44 " "
1858, Feb., 5 47 " "
1858, Dec., 5 50 " "

New York.

See C. S. report of 1855, pp. 320, 321, 333, and 337, also C. S. report, 1856, p. 217.

Philadelphia.

See C. S. report of 1855, pp. 313, 314, and 337.

Hatboro', Pa.

See C. S. report of 1858, pp. 192, 193, 194, and 195.

Baltimore, Md.

1808, $D=$ 0° 10′ to 15′ W. D. Byrnes, vol. xviii, 1830, Sill.'s Journal.

See also C. S. report, 1856, pp. 219, 227, also C. S. report, 1858, p. 191.

Washington, D. C.

See C. S. report, 1858, pp. 195, 196, 197.

Williamsburg, Va.

1694, $D=$ 5° 00′ W. Sill.'s Journal, vol. xxxiv, 1838, Prof. Loomis's collection.
1780, 0 50 " " " " " "
1809, 0 33 E. " " " " "
1856, Aug., 1 04 W. Deduced from observations at Petersburg, Old Point Comfort, and Norfolk.

Charleston, S. C.

1857, April, $D=$ 1° 56′ E. Derived from observations at Savannah in 1852 and 1857.

See C. S. report, 1855, pp. 322, 323.

Savannah, Ga.

1817, $D=$ 4° 00′ E. Becquerel traité du magnetisme.
1838, 5 05 " Silliman's Journal, xxxiv, 1840.
1839, 3 31 " " " " "

See also C. S. report, 1856, p. 220, and C. S. report, 1858, p. 192.

Mobile, Ala.

See C. S. report, 1855, p. 323, also C. S. report, 1858, p. 192.

Havana, Cuba.

See C. S. report, 1855, p. 324.

1857, Jan.,	$D=$ 5° 15′ E.	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Jamaica, W. Indies.

1732,	$D=$ 6° to 6° 05′ E.	J. Harris at Black river, in March and April. Phil. Trans., 1733.
1789–93,	6 50 "	J. Leard, map of Port Royal.
1791–92,	6 45 "	" " "
1819,	4 50 "	DeMackau, Becquerel's traité du magnetisme, Paris, 1846.
1821,	4 50 "	DeMayne, " "
1822,	4 54 "	Owen, " "
1832,	5 13 "	Foster, " "
1833?	4 40 "	From a map.
1840?	4 00 "	Gen. Sabine's isogonic map of the Atlantic Ocean.
1857, March,	3 40 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Panama, New Granada.

1775, Nov.,	$D=$ 7° 49′ E.	Encycl. Brit.
1791, Dec.,	7 49 "	" "
1802,	8 00 "	" "
1822,	7 00 "	Hall, Becquerel's traité du magnetisme.
1837,	7 02 "	Sir E. Belcher.
1849,	6 55 "	Major Emory (Mexican Bound. Survey).

See also C. S. report, 1856, p. 223.

Vera Cruz, Mexico.

1726–27,	$D=$ 2° 15′ E.	J. Harris, Phil. Trans. R. S. anno 1728.
1769,	6 40 "	Encyc. Brit., 7th edition, 1842.
1769, March,	6 28 "	"
1776,	7 30 "	Don Ulloa, Encyc. Brit.
1815,	10 37 "	Malony.
1819, April,	9 16 "	Wise.

See also C. S. report, 1856, p. 214.

San Diego, Monterey, San Francisco and Cape Mendocino, California, and for Cape Disappointment, Washington Territory, see C. S. report, 1856, pp. 228 to 235.

Sitka, Russian America.

1804,	$D=$26° 45′ E.	Lissiansky,	Becquerel's traité du magnetisme.
1824,	27 30 "	Kotzebue,	
1829,	28 19 "	Erman,	
1858,	30 00 "	From Evans' map of isogonic lines for 1858.	

Washington, D. C.

Art. XXX.—*Caricography;* by Prof. C. Dewey.

(Continued from vol. xxviii, p. 232, Second Series.)

No. 260. *Carex argyrantha*, Tuckerman.

Spica composita; spiculis 4–8, ovato-rotundis vel obovatis sub-approximatis alternis albis, inferiore subremota, supernè staminiferis, squamosi-bracteatis, distigmaticis; fructibus ovatis compressis erectis vel sub-patulis nervosis margine membranaceo-alatis viridibus acuminato-rostratis brevi-bifidis, squamam membranaceam albam lanceolatam aequantibus.

Culm 1½–3 feet high, smooth, lax, reclining, and twice longer than the leaves; spikelets nearer above and white; fruit margined or winged widely for its length and width; light green.

Rocky Woods, Amherst and Sunderland, Mass.; *Prof. Tuckerman*, by whom the plant and his description have been kindly presented. It is related to *C. Deweyana*, and the white-glumed family, silver-flowered, according to its name, but appears to be new and distinct; discovered the last season.

Note. *C. Rugeliana*, Kunze, suppl. to Schkuhr, No. 56, p. 189, is *C. aestivalis*, Curtis, according to Boott, Illust., p. 54, No. 133. *C. miser*, Buckley, in this Journal, vol. xlv, p. 173, and vol. xlviii, p. 140, is considered by Dr. Boott to be *C. juncea*, Willd. System. Veg., 1826, No. 226. See Boott, Lin. Trans. vol. xx, p. 116, and Illust., p. 55. The descriptions of Willd. & Kunze sustain this conclusion of Dr. Boott.

261. *C. paludosa*, Goodenough. Schk. fig. 103.

Spicis pistilliferis, 2–4, sæpe 3, cylindraceis erectis oblongis, arctè-floriferis sub-approximatis, superioribus sessilibus, inferiore sæpe longo-pedunculata vix vaginata inferne attenuata et hinc sublaxiflora, alternatis et foliaceo-bracteatis; perigyniis (fructibus) ovatis in breve rostrum bidentatum attenuatis vel ovalibus acuminatis brevi-rostratis distinctè et multi-nervosis subcompressis subglabris tristigmaticis, squamam angustam lanceolatam aequantibus vel lanceolata cuspidata brevioribus.

Culm 1½–2 feet high, erect, triquetrous, scabrous above, longer than the rough-edged leaves, with leafy bracts equal to or surpassing the culm. Varies, like the European plant, in the length and thickness of its spikes, and in its glumes or scales.

Near Boston—*Wm. Boott, Esq.*, in 1859, probably introduced not many years since. Common in England, Germany, and Sweden.

262. *C. monile*, Tuckerman. Boott, Illust., No. 71.

Spicis staminiferis, 2–4, longis, cylindraceis, gracilibus cum squama longo-lanceolata; pistilliferis 2, rarò 1, oblongo-cylindraceis, subremotis, brevi-pedunculatis, sub-densifloris, infima interdum basin attenuata subnutante, foliaceo-bracteatis vix vaginatis; fructibus globosis vel ellipsoideis inflatis brevi-rostratis bidentatis glabris multi-nervatis stramineis cum ore rostri sub-obliquo, squama angusta oblonga lanceolata sub-duplo longioribus.

Culm 15-30 inches high, erect, triquetrous, longer than the leaves; bracts surpassing the culm, bright yellowish green.

In marshes, not abundant, N. England—*Tuckerman;* Rhode Island—*Olney;* New Jersey, Ohio and westward.

Note 1. The Carex, No. 197, vol. xlix, p. 47 of this Journal, accidentally misnamed, is *C. Vaseyi*, Dew., of which No. 197 contains the accurate description. From the preceding, *C. monile*, Tuck., the fruit is very different, being ovate, long-conic, subtriquetrous-inflated, glabrous and scabro-rostrate. *C. Vaseyi* is the plant referred to by Dr. Boott, Illust., No. 71, as different from *C. vesicaria* and *C. monile*, but not named by him among those sent him from Penn Yan, N. Y., by Dr. Sartwell. The reference of No. 197 above to No. 71 in the Illust. is, of course, an oversight. The correction on 197 is *C. Vaseyi*, Dew.

Note 2. *C. bullata*, Schk., Fig. 166, described in this Journal, vol. ix p. 71, 1825, from living specimens, and named from its (ball-shaped) nearly globose fruit, seems to me, as it does to Carey in Gray's Manual, distinct from *C. cylindrica*, Schw., and from the following number.

263. *C. physema*, Dew. *C. bullata*, Boott, Illust., No. 71, and Carey (non Schk).

Spicis staminiferis, 2–3, cylindraceis, gracilibus, contiguis, infima bracteata; pistillifera 1, interdum 2, subrotunda vel oblongo-cylindracea perdensiflora et crassa remota et subfulva, infima pedunculata et subnutante vel erecta longi-foliacea bracteata; fructibus turgidi-ovatis longi-cylindraceis rostratis bifurcatisque, inflatis glabris et scabro-dentatis vel serratis, squama lanceolata acuta albi-marginata longioribus et latioribus.

Culm 1–2 foot high or more, firm, slender for the thickness of pistillate spike, stiff, triquetrous and often roughish, shorter than the narrow, flat and prim leaves; bright green.

Humid meadows, New England to Pennsylvania.

On *C. bullata*, Schk., Fig. 166, the pistillate spikes are shown as long and loose-flowered, and the fruit globose (ovate-globose), inflated abruptly, contracted into a slender, round, long, scabrous bifurcate beak, with a glume about half as long.

On the Carex, Boott, Illust., No. 39, the fruit is very large and inflated-ovate, with a conic, tapering, three-sided beak, scabrous-dentate, forming thick, dense and large spikes. If the figure by Dr. Boott shows *C. bullata*, Schk., then the figure in Schk. is a palpable caricature. Trusting to the correctness of the figure of Schk., I may yet say that Dr. Boott has presented a well known and distinct form. It is obvious that it should have another name, which designates the *inflation* of the fruit or perigynium, like the other.

264. *C. Olneyi*, Boott, Illust., No. 40.

Spicis stamiferis, 2–4, saepe 3, cylindraceis approximatis gracilibus; pistilliferis saepe 2, vel 1–3, cylindaceis sub-crassis densifloris stramineis approximatis plus minus pedunculatis, infima basin tereti laxiflora saepe subnutante, bracteatis; fructibus inflati-ovatis brevi-conico-rostratis scabro-bifurcatis nervosis divergentibus, squama lanceolata acuta vel cuspidata longioribus.

Culm 15–22 inches high, strong, obtusely triquetrous, scabrous above, shorter than the long and stiff margined leaves.

Rhode Island—*Olney;* probably associated with this group over the country. Were it not for the very different spikes, the fruit would closely ally this species to *C. physema*, as being a smaller form of it.

Remark.—This Vesicaria group is of difficult determination, and there has been much confusion in consequence. Hoping that some light has shined upon it, I can only say with Dr. Boott, that "future observation must determine in America the value to be affixed to the species of this group." The following shows the group as here exhibited:

C. monile, Tuckerman. Boott, Illust., No. 39.
C. Vaseyi, Dew. · *C. monile*, Sart., Exsic. No. 151, and Sill. Journ., [1] vol. 48, p. 47, putting *C. Vaseyi* for *C. monile.*
C. bullata, Schk. Dew., Sill. Journ. vol. 9, p. 71 (not Boott or Carey).
C. physema, Dew. *C. bullata*, Boott, and Carey in Manual.
C. Tuckermani, Boott. Dew., Sill. Journ., vol. 49, p. 47. *C. cylindrica*, Carey.
C. Olneyi, Boott, Illust., and as above.

As Dr. Boott has found *C. lenticularis*, Mx., among the northern Carices, it is now distinguished and identified. The great difficulty had been in the imperfection of the description of Michaux. This is removed by Dr. Boott, which the younger botanists will be glad to see, and also the more complete description.

C. lenticularis, Mx. Boott, Illust., No. 76, Fig. 77. *C. concolor*, R. Br. in this Journal.

Spicis cylindraceis obtusis approximatis et sessilibus; staminifera unica, interdum 2, infima brevi, vel terminali superne sæpe fructifera; pistilliferis 2–5, vel rarò pluribus, foliato-bracteatis, interdum inferne staminiferis, infima vix sessili; fructibus ellipticis convexo-lenticularibus, interdum sub-ovatis, per-brevi-rostellatis nervosis stramineis glabris, squama oblonga *obtusa* pallida longioribus; planta matura concolore.

Culm 8–14 inches high, triquetrous, erect, leafy towards the base, leaves about the length of the culm, and sheaths longer than the culm; terminal spike staminate, often only staminate below, sometimes a shorter staminate sessile spike near it; pistillate spikes 3–4, or more or less, cylindrical, erect, the lowest sometimes vaginate-bracted; stigmas 2; fruit sub-ovate, longer than the oblong obtuse white-edged scale; whole plant light green, nearly of the same color.

Lake of Swans and Arctic America, *Michaux and Richardson.* Var. Albi-montana, has fruit less oval, or ovate and acutish, tapering above, resembling somewhat *C. torta*, but taller than the Arctic plants. White Mountains, N. H., about ponds—*Tuckerman.* In Harrison, Me., about ponds, in 1859—*Rev. J. Blake.*

Errata.—On p. 232, vol. xxviii, for *C. gynocratis* read *C. gynocrates.* On p. 231, same vol., for vol. xxiv, p. 48, read vol. xxvii, p. 81, Sec. Ser., and for the numbers 254, 255 and 256, read 257, 258 and 259.

ART. XXXI.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part II.

(Concluded from p. 111.)

On Geometrical Ratios existing between Equivalent Numbers.

THE First Part of this paper was devoted to the examination of relations between the equivalent numbers of certain elementary bodies depending upon the number 44–45, and it was attempted to show:—

1st. That such relations extend to nearly all the elements:—

2d. That the particular groups collected together by this relation consist of bodies whose properties are analogous, and that the classification is in harmony with the distinguishing characteristics of the substances classified.

The first portion of this Second Part will present a species of relation wholly distinct, it is believed, from any that has hitherto been pointed out, and which may be not inappropriately termed Geometrical Ratios, to distinguish them from the more familiar arithmetical relations which have been heretofore exclusively studied by chemists.

The arithmetical relations are susceptible of at least an hypothetical explanation, on the supposition that the common difference in a series of elements may represent the equivalent number of a substance as yet undetermined, which, by its combinations in varying proportions, gives rise to the bodies constituting the successive terms of the series. The analogies which are now to be considered are more difficult of explanation, even by hypothesis. Their accuracy, sometimes absolute, renders improbable the supposition that they are mere casual coincidences. In science it is not permitted to neglect facts merely because we cannot satisfactorily account for them.

The nature of these relations consists in this, that if we take two substances and examine the ratio which subsists between the numbers representing their atomic weights, we may find in certain cases, that it is identical with the ratio subsisting between the atomic weights of two other substances, and so on through a considerable number of elements. The ratio between the atomic weights, for instance, of oxygen and nitrogen, is that of four to seven, so likewise is that between those of zirconium and potassium, potassium and barium, with absolute exactitude. What renders this the more remarkable is, that all three of these last substances are striking exceptions to Prout's law that the equivalents of the elements are exact multiples of that of hydrogen; they all have decimals, zirconium 22·4, potassium 39·2, barium 68·6. Now the ratio just mentioned gives these num-

bers with their decimals with perfect exactness. The same species of relation exists between many other elements, as will be seen by the table below :—

(1.)

Oxygen-Nitrogen Ratio, or that of Four to Seven.

			Atomic wts. calculated.		Atomic wts. received.
Nitrogen	= 14,	$\frac{4}{7}$ of 14	= 8	Oxygen	= 8
Barium	= 68·6,	$\frac{4}{7}$ " 68·6	= 39·2	Potassium	=39·2
Potassium	= 39·2,	$\frac{4}{7}$ " 39·2	= 22·4	Zirconium	=22·4
Calcium	= 20,	$\frac{4}{7}$ " 20	= 11·43	Magnesium	=12
Magnesium	= 12,	$\frac{4}{7}$ " 12	= 6·86	Glucinum	=7*
Strontium	= 43·75,	$\frac{4}{7}$ " 43·75	= 25	Titanium	=25
Lead	= 103·5,	$\frac{4}{7}$ " 103·5	= 59·16	Tin	=59
Antimony	= 120·3,	$\frac{4}{7}$ " 120·3	= 68·76	Vanadium	=68·60
Bismuth	= 208,	$\frac{4}{7}$ " 208	= 118·84	Antimony	=120·3
Mercury	= 100,	$\frac{4}{7}$ " 100	= 57·16	Cadmium	=56
		$\frac{4}{7}$ " 57·16	= 32·66	Zinc	=32·60
Molybdenum	= 48,	$\frac{4}{7}$ " 48	= 27·42	Chromium	=26·70
Chlorine	= 35·5,	$\frac{4}{7}$ " 35·5	= 20·28	Silicon	=21
Fluorine	= 19,	$\frac{4}{7}$ " 19	= 10·86	Boron	=10·9†
Boron	= 10·9,	$\frac{4}{7}$ " 10·9	= 6·23	Carbon	=6

It will be remarked that mercury, cadmium and zinc are here again brought together. Although four-sevenths of the atomic weight of mercury is not that of cadmium exactly, nevertheless four-sevenths the resulting number so obtained, viz. 57·16, gives for the atomic weight of zinc 32·66, an exceedingly close approximation.

The last three equations in the table show us that the atomic weight of silicon stands nearly in the same numerical ratio to that of boron, as that of chlorine does to that of fluorine, and that both these ratios, especially the latter, approach nearly to that existing between the atomic weight of boron and carbon.

Now as the atomic weight of boron is by no means positively determined, it may be allowable to examine how far an hypothetical number will fulfill these several ratios. Let us assume:—

Fluorine : Chlorine : : Carbon : Boron

we find:—

$$\frac{\text{Chlorine } 35{\cdot}5 \times \text{Carbon } 6}{\text{Fluorine } 19} = 11{\cdot}2104,$$

and obtain for the hypothetical equivalent of boron the number 11·21 (neglecting the last decimal figures).

Let us now examine how far the number 11·21 will fulfill the second proposed ratio, viz.

Carbon : Boron : : Boron : Silicon

* Taking Glucina as G_2O_3.

† See Jahresbericht, 1858.

we have $\frac{(\text{Boron } 11{\cdot}21)^2}{\text{Carbon } 6} = 20{\cdot}944$:—

a close approximation to 21, the received number.

If, therefore, we assume the atomic weight of boron to be a mean proportional between those of carbon and silicon, which we may do by an alteration certainly within the bounds of possible error in determinations so far made, we find that the proportion between the weights of carbon and boron, of boron and silicon, is the same as that between those of fluorine and chlorine.

(2.)

Carbon-Nitrogen Ratio, or that of Three-Sevenths.

The atomic weight of Carbon stands to that of Nitrogen in the ratio of 3 to 7, a proportion which is found exactly or approximately to extend to certain other elements.

Nitrogen	= 14,	$\frac{3}{7}$ of 14	= 6	Carbon	= 6
Fluorine	= 19,	$\frac{3}{7}$ " 19	= 8·14	Oxygen	= 8
Iron	= 28,	$\frac{3}{7}$ " 28	= 12	Magnesium	= 12
Cerium	= 47,	$\frac{3}{7}$ " 47	= 20·14	Calcium	= 20
Phosphorus	= 31,	$\frac{3}{7}$ " 31	= 13·29	Nitrogen	= 14
Bromine	= 80,	$\frac{3}{7}$ " 80	= 34·29	Chlorine	= 35·5
Arsenic	= 75,	$\frac{3}{7}$ " 75	= 32·14	Zinc	= 32·6
Lead	= 103·5	$\frac{3}{7}$ " 103·5	= 44·4	Strontium	= 43·75
Cadmium	= 56,	$\frac{3}{7}$ " 56	= 24	2 Magnesium	= 24
Uranium	= 60,	$\frac{3}{7}$ " 60	= 25·71	Chromium	= 26·7

(3.)

The proportions expressed by the preceding tables may be differently presented, and perhaps rendered more striking. As the numbers which express the equivalent weights of the elements are altogether relative, it is of course a mere question of convenience which is selected as unity. If in place of adopting the equivalent of one substance, as that of hydrogen or oxygen, as a permanent unit, we successively make those of the substances contained in the right hand column of the table in section 1. our unit, and consider the effect of such a change upon the equivalents of the substances contained in the left-hand column, we shall obtain the following results:

Making the							
equivalent of	Oxygen	100,	the eq.	of	Nitrogen	becomes	175
"	Potassium	100,	" "	"	Barium	"	175
"	Zirconium	100,	" "	"	Potassium	"	175
"	Titanium	100,	" "	"	Strontium	"	175
"	Tin	100,	" "	"	Lead	"	175·4
"	Vanadium	100,	" "	"	Antimony	"	174·9
"	Boron	100,	" "	"	Fluorine	"	174·3
	&c.				&c.		

The same view may be extended to Section 2. Assuming the atomic weight of the substances in the left-hand column successively as unity, or one hundred, we obtain the following results:

Making the equivalent of						
	Nitrogen	100	that of	Carbon	becomes	42·8
	Fluorine	100	" "	Oxygen	"	42·1
	Iron	100	" "	Magnesium	"	42·8
	Cerium	100	" "	Calcium	"	42·6
	&c.			&c.		

It is therefore clear that the atomic weight of each substance in the left hand column of these several tables bears to that of the corresponding substance in the right-hand column a definite numerical ratio, which is identical, or nearly so with that borne by the atomic weight of any other substance in the same column of the same table to that of the corresponding substance in the other column.

(4.)

We have seen that many elements stand to other elements in the same relation as nitrogen to oxygen—many in the same relation as nitrogen to carbon. Apart from these more general ratios, many elements may be classed together in double or treble pairs, such that the two elements in one pair stand to each other in the same numerical ratio as the two elements of a second or third pair, the two elements constituting each pair being more or less closely allied to each other in properties, though the pairs are not necessarily analogous with those with which they are compared.

For example, arsenic stands to antimony in the same numerical ratio as selenium to tellurium, within an extremely small fraction, so that by multiplying and dividing we have:—

$$\text{Arsenic } 75 \times \frac{\text{Tellurium } 64}{\text{Selenium } 40} = 120, \text{ Antimony} = 120{\cdot}3,$$

So in like manner magnesium stands to zirconium in the same ratio as fluorine to chlorine:—

$$\text{Magnesium } 12 \times \frac{\text{Chlorine } 35{\cdot}5}{\text{Fluorine } 19} = 22{\cdot}42, \text{ Zirconium} = 22{\cdot}40,$$

So carbon stands to boron in the same ratio as silver to gold:—

$$\text{Carbon } 6 \times \frac{\text{Gold } 197}{\text{Silver } 108} = 10{\cdot}94, \text{ Boron} = 10{\cdot}90.$$

These ratios, as well as those that follow, are very close approximations, certainly within the limits of error which may easily exist in the determinations on which we depend. For antimony we find 120, the recent determinations vary from 119 to 122, that which appears most reliable is 120·3, as adopted by the Jahres-

bericht. In the second case, the number found for zirconium varies but $\frac{1}{111}$ from that generally received. In the third, the number found for boron is intermediate between the number adopted in the Jahresbericht 10·90 and that found by Deville, 11. Discrepancies such as these are trifling in the extreme, and have not the slightest real significance.

Phosphorus stands to nitrogen in nearly the same ratio as strontium to calcium:—

$$\text{Phosphorus } 31 \times \frac{\text{Calcium } 20}{\text{Strontium } 43{\cdot}77} = 14{\cdot}16, \text{ Nitrogen} = 14.$$

Tin stands to titanium in nearly the same ratio as iron to magnesium:—

$$\text{Tin } 59 \times \frac{\text{Magnesium } 12}{\text{Iron } 28} = 25{\cdot}28, \text{ Titanium} = 25.$$

Tin stands to zinc in almost exactly the same ratio as gold to silver, or as boron to carbon:—

$$\text{Tin } 59 \times \frac{\text{Silver } 108}{\text{Gold } 197} = 32{\cdot}35, \text{ Zinc} = 32{\cdot}60.$$

$$\text{Tin } 59 \times \frac{\text{Carbon } 6}{\text{Boron } 10{\cdot}9} = 32{\cdot}48, \text{ Zinc} = 32{\cdot}60.$$

These and many other analogies of the same character are brought together in the following table, by which the ratios of comparison between each pair are shown, up to the second place of decimals, beyond which it is pseudo-accuracy to go, in view of the data.

Ratio of tellurium to selenium, or	$\frac{\text{Te } 64}{\text{Se } 40} = 1{\cdot}60$
Ratio of antimony to arsenic, or	$\frac{\text{Sb } 120}{\text{As } 75} = 1{\cdot}60$
Ratio of zirconium to magnesium,	$\frac{\text{Zr } 22{\cdot}4}{\text{Mg } 12} = 1{\cdot}87$
Ratio of chlorine to fluorine,	$\frac{\text{Cl } 35{\cdot}5}{\text{F } 19} = 1{\cdot}87$
Ratio of boron to carbon,	$\frac{\text{B } 10{\cdot}9}{\text{C } 6} = 1{\cdot}82$
Ratio of gold to silver.	$\frac{\text{Au } 197}{\text{Ag } 108} = 1{\cdot}82$
Ratio of strontium to calcium,	$\frac{\text{Sr } 43{\cdot}77}{\text{Ca } 20} = 2{\cdot}19$
Ratio of phosphorus to nitrogen,	$\frac{\text{P } 31}{\text{N } 14} = 2{\cdot}21$

Ratio of iron to magnesium,	$\frac{\text{Fe } 28}{\text{Mg } 12} = 2{\cdot}33$
Ratio of tin to titanium,	$\frac{\text{Sn } 59}{\text{Ti } 25} = 2{\cdot}36$
Ratio of tin to zinc,	$\frac{\text{Sn } 59}{\text{Zn } 32{\cdot}6} = 1{\cdot}81$
Ratio of gold to silver,	$\frac{\text{Au } 197}{\text{Ag } 108} = 1{\cdot}82$
Ratio of zirconium to aluminum,	$\frac{\text{Zr } 22{\cdot}4}{\text{Al } 13{\cdot}7} = 1{\cdot}64$
Ratio of calcium to magnesium,	$\frac{\text{Ca } 20}{\text{Mg } 12} = 1{\cdot}67$
Ratio of potassium to sodium,	$\frac{\text{K } 39{\cdot}2}{\text{Na } 23} = 1{\cdot}70$
Ratio of arsenic to phosphorus,	$\frac{\text{As } 75}{\text{P } 31} = 2{\cdot}42$
Ratio of cobalt to magnesium,	$\frac{\text{Co } 29{\cdot}5}{\text{Mg } 12} = 2{\cdot}46$
Ratio of nickel to magnesium,	$\frac{\text{Ni } 29{\cdot}6}{\text{Mg } 12} = 2{\cdot}47$
Ratio of lead to tin,	$\frac{\text{Pb } 103{\cdot}5}{\text{Sn } 59} = 1{\cdot}75$
Ratio of cadmium to zinc,	$\frac{\text{Cd } 56}{\text{Zn } 32{\cdot}6} = 1{\cdot}72$

(5.)

It has been the object of this paper to develope as far as possible those numerical relations existing between the atomic weights of the elements which have not been previously observed. It is, perhaps, impracticable in the present state of chemical science to explain why such relations should exist, nevertheless, nothing which tends to an exacter knowledge of the laws which govern the proportions in which the elements combine should be neglected. In this way, little by little, the materials are collected for future generalizations, with the reasonable hope of eventually arriving at an intimate knowledge of the true constitution of the materials which compose our globe.

The substances which compose the chlorine group form a well marked family to which fluorine seems also to belong. The analogies which unite chlorine, bromine and iodine, appear to multiply with each new investigation of their properties. By none have they been more strikingly illustrated than by the recent experiments of Plücker on the spectra obtained from the light of electric discharges through gases in a state of extreme

rarefaction, demonstrating that those given by Cl, Br and I, resembled each other in exhibiting great numbers of black lines of extreme fineness, so fine indeed as to be almost mathematical lines, and that in this respect these elements differed wholly from all others. With so remarkable an experiment as this before us, supported by the very numerous other relations which exist between these substances, we might naturally look to find striking numerical correspondences between their atomic weights, but the analyses upon which we at present depend give us numbers which afford no relations positively exact. Of the four numbers 19, 35·5, 80 and 127, two are prime, one fractional, and one has many divisors. Such numbers are very unpromising for the development of numerical relation.

Thus far, however, at least relations do exist: that it is possible to express the value of the equivalent of any one of these substances in terms of any two of the remaining—each equivalent is a function of any other two, thus:—

$$I = 10\,Cl - 12\,F \qquad Cl = \frac{12\,Br - 7\,I}{2}$$

$$I = \frac{5\,Br - F}{3} \qquad Cl = \frac{I + 12\,F}{10}$$

$$I = \frac{12\,Br - 2\,Cl}{7} \qquad Cl = \frac{Br - 7\,F}{6}$$

$$Br = \overline{6\,Cl - 7\,F} \qquad F = \frac{10\,Cl - I}{12}$$

$$Br = \frac{2\,Cl + 7\,I}{12} \qquad F = \frac{6\,Cl - Br}{7}$$

$$Br = \frac{3\,I + F}{5} \qquad F = 5\,Br - 3\,I$$

If these equations represented approximations, no matter how close, they would not be worthy of consideration. They are perfectly exact. The quasi-connection of oxygen with this series will be pointed out further on.

(6.)

It has been remarked that the atomic weights of the members of the oxygen series proper differ from each other by whole multiples of the atomic weight of oxygen, but the extent to which differences of this kind exist amongst the atomic weights of the elementary bodies has perhaps not been pointed out.

The elements having the highest atomic weights are bismuth and gold, commencing with the first of these substances, and diminishing its atomic weight successively by whole multiples (in one case by a sub-multiple) we obtain the following results:—

Differences.		
	208, Bismuth	= 208
8×10	128 ..... Iodine	= 127
8	120, Antimony	= 120·3
8×2	104 ..... Lead	= 103·5
8	96, Osmium	= 97–99
8×2	80 ..... Bromine	= 80
8×½	76, Arsenic	= 75
8	68, Vanadium	= 68·6
	68·6 ..... Barium	= 68·6
8	60, Uranium	= 60
8	52, Ruthenium	= 52·2
8	44 ..... Strontium	= 43·77
8	36 ..... Chlorine	= 35·5
8×2	20 ..... Fluorine	= 19
	Calcium	= 20
8	12 ..... Magnesium	= 12

It has not been usual to classify vanadium, osmium or ruthenium with the arsenic series, but Schafarik in a paper on the vanadium compounds,* and Hallwachs and Schafarik in a subsequent paper† on allied subjects (to neither of which the author had access while engaged on the first part of this paper except in the form of a brief notice) are of opinion that vanadium should be removed from the molybdenum group in which it has heretofore been classed, and be transferred to the arsenic series, an opinion which they support by arguments founded on the fact that the specific volume as well of vanadic acid as of the

* Sitzungsbericht of the Acad. of Sciences of Vienna, Bd. xxxiii, ext. in Chemisches Centralblatt, 1859, 97.

† Same vol. of Sitzungsbericht, ext. in Chemisches Centralblatt, 1859, p. 161.

metal itself, corresponds with those of the metals of the arsenic series, viz.:

		Specif. Vol.	
Vanadic acid,	VO_3	26·5	
Arsenious acid,	AsO_3	26·6	
Antimonic oxyd,	SbO_3	25·9	
Bismuthic oxyd,	BiO_3	25·9*	28·4†

They also point out the fact that vanadinite (vanadinbleierz) is isomorphous with the analogous compounds of the arsenic series, mimetene (kampylit) and pyromorphite. Professor Mallet has shown that reasons can be given for classifying osmium also in the arsenic series.‡

Again, commencing with gold we obtain the following succession of numbers.

Differences.				
8×11	196	Gold	=197	
	108	Silver	=108	
8	100	Mercury	=100	
8	92	Tungsten	= 92.	
8×3½	64	Tellurium	= 64	
8	56	.....	Cadmium	=56
8	48	Molybdenum	= 48	
8	40	Selenium	= 40	
8	32	.....	Zinc	=32·6
8×2	16	Sulphur	= 16	
8	8	Oxygen	= 8§	

In an interesting paper published in the Memoirs of the Am. Acad. of Boston, vol. v, Prof. J. P. Cooke, of Harvard University, attempted a classification of all the elementary bodies by making them all functions of a general series of the form $a+dx$. By giving fixed values to a and d particular expressions were obtained, giving rise to series by the changes in the value of the variable x. These particular series were characterized by the value of d, which in different series became successively 3, 4, 5,

* Boullay. † Karsten. ‡ Am. Jour. Science, xxix, 49.

§ If to the at. wt. of Li which may be taken at 7, we add 16=2×8, we have 23 the exact at. wt. of Na, and a further addition of 16 gives very nearly the at. wt. of K.

6, 8 and 9. The values of a were likewise always small, never exceeding 8. The values of x were restricted to whole numbers.

This view, although new and ingenious, is evidently exposed to a serious objection: the series include too much. The form entitled by Prof. Cooke the "Three Series," and expressed by $1+3x$, includes one-third of all possible whole numbers, and so to a proportionally great extent with all the other forms. As the author did not make positive exactness essential, it is evident that a wide scope was given for the classification of elements under any particular series: therefore this theory could only find favor as far as it might be made to conform to a wholly unobjectionable classification of the elements. In this it only partially succeeded, as will appear from an examination of the substances included in the various groups.

In the first series, chromium, manganese, osmium and gold are classed as affiliated to the chlorine group, into which however the series only admits fluorine with an error of 2, or 12 per cent.

In the second, arsenic and manganese are made affiliated with the sulphur group, which includes also molybdenum, vanadium, &c.

In the third, oxygen is classed with the nitrogen-phosphorus group, into which antimony only enters with an error of 7·7.*

In the fifth we find tin and titanium, the platinum group, gold, mercury, and most of the magnesia group.

In the sixth we have part of the remainder of the magnesia group, the metals of the alkaline earths and lead, the alkaline metals, hydrogen and silver, with copper and manganese as affiliations. Rendering all justice to the author for the originality and ingenuity of his views, it must be admitted that the flexibility of his series has led to a classification not in all respects supported by analogies. Five out of his six series contain one or more members of the magnesia group either directly or as affiliated members.

Dumas has followed out these ideas of Prof. Cooke and extended them. In place of referring his series to the type of the formic acid series alone, he includes the types of substituted ammoniums and stannethyls, under the general form $na+xd+yd'$; thus adopting an expression even more general than that of Prof. Cooke, but restricting it more closely in its application.

Perhaps the most beautiful and important of the relations pointed out by Dumas, are the parallel series of numbers, which by the subtraction of each term of the one from the corresponding term of the other, exhibit a constant difference within a certain degree of approximation.

* This probably arises from the fact that the equivalent of antimony as generally received at the time when Prof. Cooke wrote was higher than it is now known to be, and by diminishing the value of the variable x corresponding to this term, by 1, a nearer approximation would be obtained.

The analogies pointed out in the several parts of this paper evidently lead directly to the construction of series of the same kind some of which, together with others depending upon yet different relations are given below.

−Oxygen =−8	Chlorine =35·5	Bromine =80	Iodine =127
−Zinc =−32·6	Magnesia =12	Cadmium =56	Lead =103·5
24·6	23·5	24	23·5

In connection with these two parallel series we may remark that oxygen appears to constitute a negative term in the chlorine series, precisely as nitrogen does in the phosphorus and zinc in the cadmium series, with considerable approximation to exactness. If the atomic weight of chlorine were taken at 36 instead of 35·5 it would constitute an exact numerical mean between the atomic weights of bromine and oxygen supposing the latter to be taken with a negative sign. We have before seen that nitrogen, the negative member of the phosphorus series, is less closely allied with the rest than they are with each other. Still greater is the step between chlorine and oxygen, so great indeed that it is very doubtful whether they can properly be classed in the same series, although certain cases of isomorphism can be urged in favor of such a classification.

We here see a new instance of the existence of the relation of 44—45 developed in the first part of this paper.

Differences.	Calculated equivalents.		Received equivalents.
	− 8	Oxygen	= 8
44	36	Chlorine	= 35·5
44	80	Bromine	= 80
44	124	Iodine	=127

Gold =197	M =152·5	Silver =108	Mercury =100
Mercury =100	Cadmium = 56	Magnesium = 12	Glucinum = 4·7
97	96·5	96	95·3

M here represents a possible metal as yet unknown. In the first part of this paper mention was made of the possibility of the existence of such a metal, having an atomic weight representing the arithmetical mean between those of gold and silver.

Bismuth =208	M′ =164	Antimony =120·3	Lead =103·5
Antimony =120·3	Arsenic = 75	Phosphorus = 31	Nitrogen = 14
87·7	89	89·3	89·5

In this double series we find nearly the same number (89 approximatively) obtained by subtraction, as by addition in the last of the double series, part first, section 8. If in that just presented we replace lead by arsenic and give a negative sign to

the atomic weight of nitrogen, we shall still have a number conforming to the character of the series, viz., 89.

In this and the following series the symbol M′ represents a possible metal constituting an intermediate term between antimony and bismuth. That such a metal might exist was pointed out in the consideration of the phosphorus series.

Bismuth =208	M′ =164	Antimony =120·3	Arsenic =75
Gold =197	M =152·5	Silver =108	Copper =63·4
11	11·5	12·3	11·6

M as before represents an hypothetical metal having for its atomic weight a mean term between those of silver and gold.

Uranium 60	Vanadium 68·6	Tungsten 92
Magnesim 12	Calcium 20	Strontium 43·75
48	48·6	48·25

Molybdenum 48	Vanadium 68·6	Tungsten 92	2 Tantalum 137·6
Magnesium 12	Zinc 32·6	Cadmium 56	Mercury 100
36	36	36	37·6

The parallelism between the three first terms of these last two series commencing respectively with molybdenum and magnesium is positively exact, perhaps the only known case in which absolute exactitude obtains. It is probable that other cases of parallel series exist, and will be discovered.

It is not easy to fix the exact amount of importance which attaches to the numerical relations up to this time ascertained to exist between the atomic weights of the elements. Some are no doubt mere casual coincidences, and relations remarkably exact and symmetrical may exist between the atomic weights of bodies which have no analogies in their properties: for example we may take calcium 20, selenium 40, uranium 60, bromine 80, mercury 100. Here the differences are not only exact, but all the subsequent numbers are multiples of the first, and this between bodies remarkably dissimilar in their properties—a striking proof of the necessity of caution in inferring relations of properties as following from relations of numbers. But on the other hand, to reject the relations of number when accompanied by analogy of properties, as unmeaning and unimportant, would be to err quite as much on the other side. When the received equivalent of an element forming a term in a well marked series differs from that obtained by calculation, it naturally leads, as Prof. Mallet has remarked, to suspect an error and desire a redetermination. The fact that a group of elements allied in their chemical characters may be arranged in a series having a common difference or a definite ratio between its terms, confirms the propriety of grouping those elements together, and such analogies may in doubtful cases assist us in arriving at a correct classification.

Philadelphia, Feb. 27, 1860.

ART. XXXII.—*Ornithichnites, or tracks resembling those of Birds;* by ROSWELL FIELD, of Greenfield, Mass.

WHEN fossil footprints were first discovered in the sandstones of the Connecticut Valley, it was indeed thought to be a great discovery, but that the tracks, thus found were made by birds was received by men of scientific attainments with great distrust and skepticism. That they were tracks made by once living animals there could be no doubt, but that they were ornithichnites was very much doubted.

It was not indeed until after my esteemed friend Dr. Hitchcock had spent much time in comparing, describing, and in distributing specimens, that the scientific public became satisfied that they were the tracks of once living birds.

The great and only proof that they were the tracks of birds, is found in the organization of the fossil foot, in the numbers of joints or lateral expansions in the toes; in this they are supposed to agree, and probably do, with living types; this with alternate steps of right and left feet is all the evidence we have that they were the tracks of birds.

Living in the immediate vicinity of Turner's Falls, the locality that has furnished the most numerous, and beyond all comparison the most beautiful specimens, my attention was drawn many years since to this particular subject. It was from my farm that the late Dexter Marsh obtained his choicest specimens. And it was in the vicinity of these Falls where my much lamented friend Dr. Deane found "new walks in an old field:" Where our barren and rocky wastes became to him a garden of delight.

It was here I witnessed their labors with pleasure, and in a more obscure way have followed in their footsteps. I think I may safely say that I have uncovered more footprints, and found more new species, and a greater variety of tracks than any other man, I think I might also say with propriety than all others that have preceded me, and if I have learnt anything on this subject I have learnt it at the quarries. It is there, and there only I have studied the history of Triassic days, and the more I have studied, the greater have been my doubts as to the ornithic character of any of the tracks which these tables of stone contain. I have seen thousands of tracks that others have not seen. With injudicious blasting, and the carelessness of workmen many choice specimens have been broken and lost; other slabs literally covered with footprints, have been spoilt by suncracks the shrinkage of the mud in drying; the stratum over which the animal moved being either too hard or too soft to receive or retain good impressions, all such are rejected and lost to the student, at the quarries.

I have no new theory to advance, and none to build up, but if I can rightly decipher these fossil inscriptions impressed on the tombstones of a race of animals that have long since ceased to exist, they should all of them be classed in the animal kingdom as Reptilia. If I have not studied this subject in vain they were all quadrupedal. That they usually walked on two feet I admit, and that they could as readily walk on four when necessary is equally true. In proof of this we find tracks as perfect as if made in plaster or wax, which to all appearance as to the number of toes, and the phalangeal or lateral expansions in the toes agree perfectly with those of living birds, and still we know by the impressions made by their forward feet that these fossil tracks were made by quadrupeds. In other cases where the animals sunk deep in the muddy stratum over which they moved, it is plain that they dragged their tails in the mud, leaving a groove plowed up from one half inch to an inch in width. This groove is not always found on the surface where the foot rested, the weight of the animal causing the foot to sink through the yielding stratum, whilst the tail dragged on the one above. This we know was the case with animals that were surely quadrupeds, but they show the appendage of a tail only when their feet sunk deeply in the plastic clay. Thus the proof that we once relied upon to prove them birds can be relied upon no longer.

That there were quadrupeds in those sandstone days and that these had hind feet perfectly agreeing with the stony bird-tracks, throws great doubt and distrust on the question, whether there were any true birds in this age of reptiles.

If there were birds they were doubtless apterous and naked, for we should naturally suppose that where so large a number of birds congregated upon the muddy banks, that in dressing and pluming their feathers some of them must have been trodden under foot, but the impression of feathers has never been found, although we find the smallest leaves of vegetables, and the pathway and tracks of annelids, and insects, some of them so small that they can hardly be seen with the naked eye. Even the Otozoum whose giant-like track measures twenty inches in length, once supposed to have been a biped reptile, by later discoveries is proved to have been four-footed. Other new discoveries have reduced the number of so-called birds, transferring them to the class of quadrupeds, which I verily believe is the proper place for all of them. The smoothness of the bottom of the foot in our fossil tracks agrees better with some species of batrachians that now live in and about the water, than it does with such animals as live on the land. Had birds existed at this early geological period, when the sandstone of the Connecticut valley was being deposited, there has indeed been a woful gap in their history, from then up to near the historical period, while the die from

which they were struck at their creation was not broken, but a new edition produced in these latter days. The work perhaps may have been revised but has not been enlarged as respects the size of the animal. I know that many eminent men and men of great scientific attainments, men who have spent much time and labor for many years in the investigation of this subject have come to different conclusions, and it may not become me to say that their conclusions are wrong. I would only add that when fossil tracks were first discovered there was so little known of the formation of the feet of fossil or of living animals, and particularly of their footprints that it is possible the first discoverers might have been mistaken as to the ornithic character of the footprints. The study of these fossils so very interesting to the geologist and naturalist, still merits their earnest attention. There is no known locality where they are found in such abundance, and in such perfection as at Turner's Falls, the northern terminus of the sandstone beds. Very few indeed have any conception of the marvelous perfection of this fossil inscription, or of the multitudes of once living creatures whose existence they commemorate.

March, 1860.

[Mr. Field is a plain farmer, who makes no claim to be an authority in science, but like Hugh Miller has hammered his geology out of the rocks on which he lives. He is well known as one of the most successful collectors of the foot-marks of the Connecticut sandstone, and his testimony as to the impression made on himself of their probable character and origin, has the merit of a conviction making head in an honest mind against all the weight and bias of opposing authorities.—Eds.]

Art. XXXIII.—*Eighth Supplement to Dana's Mineralogy;* by Geo. J. Brush, Professor of Metallurgy in Yale College.*

List of Works, etc.

F. X. M. Zippe: Lehrbuch der Mineralogie mit naturhistorischer Grundlage. pp. 450, 8vo, mit 334 Fig. Wien, 1860.

A. Descloizeaux: Sur l'emploi des propriétés optiques biréfringentes pour la détermination des espèces cristallisées. p. 88, 8vo. Extrait des Annales des Mines, tome xiv, 1858.

A. Delesse: Etudes sur le Métamorphisme. Roches granitiques, pp. 77, 8vo. Extrait des Annales des Mines, tome xii.

Carl F. Naumann: Elemente der Mineralogie, fünfte vermehrte und verbesserte Auflage. 8vo, pp. 460, mit 483 Fig. Leipzig, 1859.

N. von Kokscharow: Materialen zur Mineralogie Russlands, dritter Band, Lieferung 1—3, pp. 128, mit Atlas. St. Petersburg, 1859.—This continuation of Kokscharow's great work, contains monographs of the species garnet, magnetite, anal-

* In the absence of Professor Dana I have endeavored to give an abstract of the results of the mineralogical researches, published since the appearance of the seventh Supplement.—G. J. B.—New Haven, April 1, 1860.

cime and euclase, together with important additional notes upon beryl, cancrinite, nepheline, phenacite, and apatite.

ADAM: Extrait du Tableau Minéralogique. 8vo, pp. 14. Paris.

J. F. L. HAUSMANN: Ueber die Krystallformen des Cordierites von Bodenmais in Bayern. 4to. pp. 16. Göttingen, 1859.

GUSTAV ROSE: Ueber die heteromorphen Zustande der kohlensauren Kalkerde, zweite Abhandlung, mit drei Kupfertafeln. 4to. pp. 48. Berlin, 1859.

VICTOR VON LANG: Versuch einer Monographie des Bleivitriols. 8vo, pp. 54, mit xxvii Tafeln. Wien, 1859.

D. D. OWEN: *First Report of a Geological Reconnaissance of the Northern Counties of Arkansas.* Little Rock, 1858.—In this report Dr. Elderhorst has contributed much that is of interest in regard to the ores of zinc, lead and manganese, and presented his facts in a manner that will make them of permanent practical value.

H. KOPP und H. WILL: Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften für 1858. Giessen, 1859. pp. 859.—Pages 673 to 812 contain Dr. Kopp's excellent review of the progress of mineralogical science for the year 1858.

H. C. SORBY: On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks. 8vo, pp. 48, with plates. Quart. Jour. Geol. Soc. Lond., xiv, pp. 458–500.

H. DAUBER: Ermittelung krystallographisher Constanten und des Grades ihrer Zuverlässigkeit. Pogg. Ann., cvii, 267, 343, cviii, 439.

W. H. MILLER: On the employment of the Gnomonic Projection of the Sphere in Crystallography. L., E. and D. Phil. Mag. for July, 1859.

DAUBRÉE: Memoire sur la relation des sources thermales de Plombières avec les filous métalifères, et sur la formation contemporaine des zéolithes. Extrait du Bulletin de la Société Géologique de France, [2], xvi, p. 562–591.

A. KENNGOTT: Uebersicht der Resultate mineralogischer Forschungen im Jahre, 1858. 8vo, pp. 229. Leipzig, 1860.—This is a continuation of Dr. Kenngott's excellent reports, and contains a résumé of the results of mineralogical researches made in 1858. Its completeness, and the careful criticisms by Dr. Kenngott render it a most valuable auxiliary in the study of mineralogy.

F. HESSENBERG: Mineralogische Notizen, No. 3. 4to, pp. 32. Frankfurt, 1860.—This number of Hessenberg's mineralogical notices, contains contributions to the crystallography of the species lievrite, realgar, heavy-spar, calcite, sphene, anatase, crocoisite, and malachite.

*Descriptions of Species.**

ALBITE [p. 240, VI].—Analyses of albite, (1.) from Oberhalbstein in Graubunden by Desclabissac (Zeitsch. d. deutschen geolog. Gesell., x, 207). (2.) from Calveras County, California, associated with auriferous pyrites and native gold by F. A. Genth (this Journal, [2], xxvii, 249):

	Si	Al	Fe	Ca	Mg	Na	K	ign.	
1.	68·50	18·11	——	0·56	0 66	12·17			=100·00
2.	68·39	19·65	0·41	0·47	——	10·97	*tr.*	0·21	=100·10

ALLANITE [p. 208, I—VI].—Analyses of *orthite* (1.) from Arendal by Zettel (Ann. d. Chem. u. Pharm., cxii, 85). (2.) mean of four analyses of orthite from Suontaka in Finland by Mendeljef (Kopp's Jahresbericht, 1858, 703):

	Si	Al	Fe	Mn	Ce	La, Di	Ca	Mg	K	Na	H	C	
1.	32·70	17·44	16·26	0·34	3·92	15·41	11·24	0·90	0·51	0·24	2·47	0·28	=101·71
2.	48·0	2·4	34·8	——	3·3	Y 1·5	9·3	—	—	—	0·7	——	=100·00

* The paging refers to Dana's Mineralogy, and the Roman numerals, in many places added, to the preceding Supplements.

ANORTHITE [p. 234, II, VI].—Potyka (Pogg. Ann., cviii, 110) has found *anorthite* to be a constituent of the rock occurring associated with hornblende at the Konchekowskoi Kamen in the Urals. The sp. gr. in fragments was 2·731 (17·1° C.), in powder 2·7325 (16·8°). B.B. difficultly fusible. Only partially decomposed, and with gelatinization, by chlorhydric acid.

$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{N}a$	
45·31	34·53	0·71	16·85	0·11	0·91	2·59	= 101·01

corresponding very closely with the formula $\dot{C}a^3\dot{S}i+3\dot{A}l\dot{S}i$, or considering silica as $SiO_2=\dot{C}a\dot{S}i+\dot{A}l\dot{S}i$.

Anorthite from Carlingford, Ireland, gave on analysis by Prof. Haughton, $\dot{S}i$ 45·87, $\dot{A}l$ 34·73, $\dot{C}a$ 17·10, $\dot{M}g$ 1·55=99·25 (Greg, L., E. and D. Phil. Mag., [4], xix, 13).

ANTIGORITE [p. 281, I, IV].—For description of optical properties of, see Haidinger's article in Pogg. Ann., lxxvii, 94, and Descloizeaux, Ann. des Mines, xiv, (1858.)

APATITE [p. 396, I—VII].—A description of crystallized *apatite* from Pfitsch-Thal in Tyrol is given by G. vom Rath.—Pogg. Ann., cviii, 353.

ATACAMITE [p. 138, I, III, IV, V].—Bibra gives for the composition of *atacamite* from Algodan Bay in Bolivia:

$\dot{C}u$	Cu	Cl	$\dot{H}$	$\dot{S}i$	
56·00	14·54	16·11	12·13	0·91	= 99·60

—Kopp's Jahresbericht, 1858, 740.

ARAGONITE [p. 448, II, III, IV, V, VII].—Luca has described a variety of aragonite, occurring in the Lias of Gerfalco in Tuscany, which he calls *mossottite*. The mineral is a prismatic fibrous radiated aggregate of a light green color. Sp. gr.= 2·884. On heating crumbles and loses its color. Composition:

$\dot{C}a$	$\dot{S}r$	$\dot{C}$	$\dot{C}u$	$\dot{F}e$	Fl	$\dot{H}$	
50·08	4·69	41·43	0·95	0·82	*tr.*	1·36	= 99·33

According to Marcel de Serres, the same variety of aragonite occurs in the province of Messima.—(Kopp's Jahresbericht, 1858, 732.)

A variety of aragonite from Nertschinsk has been named *oserskite* by Breithaupt (B. and H. Zeit., xvii, 54).

AXINITE [p. 213].—This species has been observed at Cold Spring in New York.

BISMUTH [p. 20].—A specimen of *native bismuth*, associated with native gold, from the Peak of Sorato, analyzed by F. A. Genth (this Jour., [2], xxvii, 247) contained:

Bi	Te	Fe	
99·914	0·042	*tr.*	= 99·956

BORACITE [p. 393, II, III, IV].—The identity in chemical composition of *boracite* and *stassfurthite* was shown by Chandler (Suppl. IV), and is further confirmed by the analyses of Siewert and Drenkmann (Zeitschrift f. d. ges. Naturwissen, xi, 365, in Kopp's Jahresbericht, 1858, 735). The direct determinations made on the washed and ignited mineral in seven partial analyses gave as the mean:

$\dot{M}g$	$\dot{F}e$	$\ddot{B}$	
30·83	0·32	69·05	= 100·20

But H. Ludwig found (Arch. Pharm., [2], xcvi, 129, in Kopp's Jahresbericht, 1858, 735) that the unwashed air-dried stassfurthite contained

MgCl	$\dot{M}g$	$\ddot{B}^a$	$\dot{H}$
11·75	23·80	58·45	6·00

(*a*) By the difference.

A direct determination of $\ddot{B}$ gave 59·72 p. c., and Ludwig considered it a hydrous boracite containing a variable mixture of chlorid of magnesium. Heintz, however, has shown that chlorid of magnesium is an essential constituent of the mineral and that even after long washing with hot water it still contains a considerable amount of this chlorid. His results (calculated from five partial analyses) gave (Jour. f. prakt. Chem., lxxvi, 243):

Cl	Mg	Ṁg	F̈e	B̈	Ḣ		
8·14	2·84	25·74	0·43	61·22[a]	1·63	=	100

a By the difference.

from which he draws the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}$. In communicating these observations of Heintz to the Berlin Academy, H. Rose (loc. cit.) adds that the boracite from Lüneberg also contains chlorine as an essential constituent, and this has been substantiated by the recent analyses of Dr. Julius Potyka (Pogg. Ann., cvii, 433), and also by Heintz (Jour. f. prakt. Chem., lxxvii, 338).

Potyka analyzed both boracite and stassfurthite, and four analyses of boracite are given by Heintz, having been made under his direction by Siewert and Geist. The results are as follows:

	MgCl	Ṁg	Ḟe	B̈	Ḣ	
1. Clear boracite crystals,	10·90	25·24	1·59	62·91	0·55=101·19	Potyka.
2. Clouded " "	10·41	26·19	1·66	61·19	0·94=100·39	"
3. Stassfurthite,	10·73	26·15	0·40	60·77[a]	1·95=100·00	"
4. Boracite,	11·14	26·00	1·52	61·34[a]	—=100·00	Siewert.
5. "	11·71	24·86	1·13	62·30[a]	—=100·00	"
6. "	11·11	25·45	1·83	61·61[a]	—=100·00	Geist.
7. "	11·54	25·43	1·05	61·98[a]	—=100·00	"

a. By the difference.

Potyka's specimens, after pulverization, were washed with cold water until the wash water no longer gave any reaction with nitrate of silver or chlorid of barium; he found that both boracite and stassfurthite were slightly soluble in hot water. All the specimens of boracite were from Lüneberg. Heintz was unable to find a weighable quantity of water. Both Potyka and Heintz express the composition of boracite by the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl=\ddot{B}$ 62·50, Ṁg 26·86, MgCl 10·64=100. Stassfurthite appears to be a boracite with one atom of water, $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}$ $=\ddot{B}$ 61·27, Ṁg 26·33, MgCl 10·42, Ḣ 1·98=100.

Bromyrite [p. 93].—F. Field has analyzed the *bromyrite* which occurs in octahedrons imbedded in carbonate of lime at Chañarcillo in Chile (Quar. Jour. Chem. Soc., x, 241). The crystals had the color and lustre of amber, and are much harder than the chloro-bromids or chlorid, and appear to be little affected by light. Composition,

Ag	Br		
57·43	42·57	=	Ag Br

Cancrinite [p. 233, II].—P. v. Pusirewsky has analyzed the *cancrinite* from the Ilmen Mts. and from Mariinskaja in the Tunkinsk Mts. (Kokscharow, Mat. Min. Russlands, iii, 76):

	S̈i	Ä̈l	F̈e	Ṅa	Ċa	C̈	Ḣ	S̈	
1. Ilmen Mts.,	35·71	29·58	—	18·78	5·56	5·56	3·76	0·32	=99·27
2. " "	36·21	29·56	0·19	18·27	5·81	5·54	3·64	—	=99·22
3. Tunkinsk Mts.,	37·72	27·75	—	21·60	3·11	5·61	4·07	—	=99·86

The variety from the Ilmen Mts. had a light rose-red color and sp. gr.=2·489. That from Mariinskaja was yellow, sp. gr.=2·454. Pusirewsky writes the formula $2(\dot{N}a^2\ddot{S}i+2\ddot{A}l\ddot{S}i+[\dot{N}a, \dot{C}a]\dot{C})+3\dot{H}$, containing one atom more of water than the formula given by Whitney for the cancrinite of Litchfield, Maine.

Carnallite [III].—Heintz gives the composition of *carnallite*, as found by Siewert:

MgCl	KCl	NaCl	ĊaS̈	Ḣ
36·03	27·41	0·23	1·14	36·33—38·01

—Kopp's Jahresbericht, 1858, 739.

Cassiterite [p. 118, V, VI, VII].—In a recent letter (Boston, Jan. 8th, 1860) to Prof. B. Silliman, Jr., Dr. C. T. Jackson mentions having received from Los Angelos in California a so-called silver ore, which on examination proved to be oxyd of tin mixed with some peroxyd of iron. On assay it gave 60·5 per cent of metallic tin. From the size of the masses of ore, Dr. Jackson is led to suppose that a vein of workable magnitude exists, some specimens being eight inches in thickness.

CERITE [p. 312].—Rammelsberg (Pogg., cvii, 632) has published several analyses of this species. The mean of the results gives—

$\dot{S}i$	$\dot{C}e$	$\dot{L}a$, $\dot{D}i$	$\dot{C}a$	$\dot{F}e$	$\dot{H}$
19·18	64·55	7·28	1·35	1·54	5·71=99·61

from which he draws the formula $(\dot{C}e, \dot{L}a, \dot{D}i, \dot{C}a, \dot{F}e)^3\ddot{S}i+Aq$, or $\dot{R}^3\ddot{S}i+Aq$. An important fact observed by Rammelsberg is, that when cerite is treated with chlorhydric acid it is partially decomposed, leaving an insoluble residue of a different composition from that contained in the solution. He remarks that it appears as if cerite was composed of a mixture of silicates, differing in not being equally acted upon by acids.

CINNABAR [p. 48, II, IV, V].—Hugo Müller has analyzed and described tetrahedral crystals of cinnabar, from Asturia, Spain, which he supposes to be pseudomorphs of either tetrahedrite or chalcopyrite.—Quar. Jour. Chem. Soc., xi, 240.

CHLORITE [p. 294, IV, V].—For analyses of a chlorite-like substance from the melaphyr-porphyry of Ilfeld by Streng, see Zeitschrift d. deutschen geolog. Gesellschaft, x, 136.

CHRYSOCOLLA [p. 309, II].—P. Herter gives analyses of two varieties of chrysocolla from the crystalline slates of Ober and Nieder-Rochlitz in Transylvania, in Zeitsch. d. deutschen geolog. Gesellschaft, ix, 372. In the same paper Herter mentions a mineral which he considers a new species. It occurs in a geode of quartz. It is amorphous; brittle; color dark pistachio-green to liver-brown and dirty yellowish-green; has a strong pitchy lustre and an almost conchoidal fracture. Sp. gr.= 2·991. Contains $\ddot{S}i$ 14·24, SbO_5 24·68, $\ddot{A}s$ 7·24, $\dot{C}u$ 31·49, $\dot{P}b$ 0·68, $\dot{A}g$ 2·05, $\dot{F}e$ 8·38, $\dot{C}a$ 2·16, $\dot{M}g$ 0·56, $\ddot{A}l$ 0·21, $\dot{H}$ 8·03=99·71. Another specimen gave but 16 per cent copper, showing the composition to be variable. In matrass gives water. Fuses easily in the forceps coloring the flame emerald green, with soda on charcoal gives a metallic bead. The centre of several of the masses was found to contain tetrahedrite, of which the above substance is a product of decomposition.

CLAYITE, *W. J. Taylor* (Proc. Acad. Nat. Sci., Philad., Nov. 1859.).—This mineral is a sulphid of lead, with about twenty-five per cent of arsenic, copper and antimony, and appears to be intermediary between galena and cupro-plumbite. It is from Peru, and occurs in small monometric crystals, a combination of the tetrahedron with the dodecahedron. It is also found amorphous, forming a coating a thirty-second of an inch thick on a layer of quartz. Color and streak blackish-gray; sectile; hardness about 2·5. B.B. on charcoal fuses easily, giving reactions for lead, arsenic and antimony, and with soda a brilliant metallic globule which becomes lustreless on cooling. Carefully selected crystals gave—

	S	As	Sb	Pb	Cu	Ag
1.	8·22	9·78	6·54	68·51	7·67	*tr.*
2.	8·14	——	——	67·40	5·62	——

No. 2 was not entirely free from extraneous matters. Prof. Taylor gives the formula (Pb, Cu)(S, As, Sb). A confirmatory result was obtained on a specimen of the amorphous variety. The mineral was received from Joseph A. Clay, Esq., of Philadelphia, having been sent to him by his brother, Hon. J. Randolph Clay, United States Minister in Peru, and it is named in honor of these gentlemen. [The amount of sulphur is extremely small, and the presence of arsenic and antimony seems to indicate an analogy with *steinmannite*, which has recently been shown to be a galena, containing some twenty or more per cent of the sulphids of arsenic, antimony and zinc, although this composition varies exceedingly.—G. J. B.]

COAL [p. 26, II, IV, VI].—O. Matter has analyzed (Jour. f. prakt. Chem., lxxvii, 39) the so-called Bog-head coal from Torbane-Hill in Scotland, with the following result:

C	H	N	O	S	$\dot{H}$	$\ddot{S}i$	$\ddot{A}l$	$\dot{F}e$	$\dot{C}a$
60·81	9·18	0·78	4·39	0·32	0·39	13·19	9·50	1·22	0·27=100·05

CONDURRITE [p. 36, V].—An examination of this mineral by C. Winkler (B. u. H. Zeit., xviii, 383) confirms the results obtained by v. Kobell and Rammels-

berg, showing this mineral to be a mixture of arsenolite, cuprite, copper-glance, and arsenid of copper.

COPPERAS [p. 380].—A cupriferous variety of copperas from a mine of chalcopyrite in Turkey has been analyzed by Pisani (Comptes Rendus, April 18, 1859). Color like that of cyanosite, on exposure assumes an ochreous tint. Composition—

Ċu	Ḟe	S̈	Ḣ
15·56	10·98	29·90	43·56

giving the formula (Ḟe Ċu)S̈+7Ḣ, or copperas in which a portion of the iron is replaced by copper.—(L. E. and D. Phil. Mag., [4], xvii, 409.)

COPPER NICKEL [p. 52, VI].—According to G. Rose and Nöggerath the copper-nickel from Sangerhausen crystallizes in the hexagonal form.—(Zeitschrift d. deutschen geolog. Gesellsch., x, 91; Verhandl. d. naturhist. Ver d. Rheinlande, xv, xv.)

CYANOLITE, CENTRALLASSITE, CERINITE, *H. How*, (Ed. new Phil. Jour., x, 84).—Prof. How has described three new species of silicates occurring in a reniform nodule in the trap of the Bay of Fundy, one mile east of Black Rock. The nodule was about half the size of a fist. It was covered with a green chlorite-like coating, and on breaking it presented a curious internal structure; immediately beneath the coating was a narrow band of a yellowish-white mineral resembling wax (*cerinite*), then a portion having a stellated appearance and a highly pearly lustre (*centrallassite*), while the centre was principally made up of a bluish-gray opaque mineral in rounded spots (*cyanolite*). A careful separation of the constituents showed—1. *Cyanolite*, comprising the centre of the nodule, was amorphous; hardness=4·5; sp. gr.=2·495; fracture flat-conchoidal, even; streak white, lustre dull, color bluish-gray; sub-translucent in thin pieces, and the powder transparent under the microscope. Decomposed with chlorhydric acid, affording slimy silica, but does not gelatinize either before or after heating. In matrass becomes white and gives off water. B.B. in platinum forceps fuses only on the thin edges, with soda and borax gives transparent beads, with salt of phosphorus a translucent glass. Analysis gave—

	S̈i	Ä̇l	Ċa	Ṁg	K̇	Ḣ
1.	74·15	0·84	17·52	*tr.*	0·53	7·39 = 100·43
2.	72·52	1·24	18·19	*tr.*	0·61	6·91 = 93·47

Analysis No. 2 was made on a specimen not perfectly free from *centrallassite*. Disregarding the small amount of alumina and potash in No. 1, we have the oxygen ratio Ċa : S̈i : Ḣ as 1 : 7·85 : 1·31 or 4 : 31·40 : 5·2, from which Prof. How draws the formula $Ċa^4S̈i_{10}+5Ḣ$=S̈i 74·26, Ċa 18·36, Ḣ 7·37. Considering the water as basic the ratio of the oxygen in all the bases to that of the silica is as 1 : 3·2, approximating to that of Edelforsite, Ċa S̈i or 1 : 3. The name *cyanolite* is in allusion to the blue tint which distinguishes this mineral from its associates.

2. *Centrallassite* occurs in spherical concretions between the cyanolite and the rind. The concretions when broken have a lamellar structure and consist of plates diverging from a centre; the plates have a pearly lustre, but the mineral passes into an opaque white form. Centrallassite has a white, sometimes yellowish, color; translucent, transparent in thin plates; brittle; lustre sub-resinous; hardness=3·5; sp. gr.=2·45—2·46. In matrass yields water, becomes opaque and silvery-white. B.B. fuses readily, with spirting, to an opaque glass, with the fluxes gives a clear bead. Decomposed by chlorhydric acid without gelatinizing. The result of two analyses were:—

S̈i	Ä̇l	Ċa	Ṁg	K̇	Ḣ
59·05	1·00	27·86	0·20	*undet.*	11·40
58·67	1·28	27·97	0·13	0·59	11·43 = 100·07

The oxygen ratio of the mean of these analyses for the lime, silica and water is 1 : 3·91 : 1·27=4 : 15·64 : 5·08, from which Prof. How deduces the formula $Ċa^4S̈i_5+5Ḣ$=S̈i 59·06, Ċa 29·20, Ḣ 11·74. From two determinations of the water and an estimation of the silica, the opaque mineral was proved to have the same composition as the transparent variety.

3. *Cerinite.* The narrow band enveloping the two preceding minerals (an eighth of an inch in thickness) was an opaque mineral, translucent in very thin fragments; amorphous; lustre sub-resinous, resembling white or yellowish-white wax; H.=3·5. B.B. fusible without intumescence. It was imperfectly decomposed by chlorhydric acid. Two analyses gave—

	$\ddot{S}i$	$\ddot{A}l$	$\ddot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{H}$	
1.	58·13	12·21	1·01	9·49	1·83	0·37	15·96	=99·00
2.	57·02	13·11	1·27	10·15	1·91	*undet.*	15·42	=98·88

The iron and potash in No. 1 were dissolved out by chlorhydric acid—a fusion with carbonate of soda was made to complete the decomposition. The loss in the analyses is supposed by Prof. How to be due to alkali not determined. The ratio between $\dot{R}$, $\ddot{R}$, $\ddot{S}i$, $\dot{H}$ is as 1 : 2 : 9 : 4, and gives the formula $3\dot{C}a\ddot{S}i+2\ddot{A}l\ddot{S}i^{3}+12\dot{H}$= $\ddot{S}i$ 58·06, $\ddot{A}l$ 14·60, $\dot{C}a$ 11·96, $\dot{H}$ 15·38.

Datholite [p. 334, I—IV, VI].—J. D. Whitney has described a peculiar variety of this mineral, which occurs in nodules in the Minnesota mine, Lake Superior (this Jour. [2], xxviii, 13). The mineral is quite compact and breaks with a conchoidal fracture; it is perfectly white and opaque, resembling in physical character the purest and most close-grained marble. H.=4·5; sp. gr. 2·983. Analysis by C. F. Chandler gave—

$\ddot{S}i$	$\ddot{F}e, \ddot{A}l$	$\dot{C}a$	$\ddot{B}$ (loss)	$\dot{H}$
37·41	0·35	25·11	21·40	5·73 = 100.

Q. Sella has in his collection, a crystal of datholite from Baveno, 4½ inches long by 3¾ inches broad, and 1½ inches in thickness.—(Wien Akad. Berichte, xxix, 239.)

Diaspore.—See under *Natrolite.*

Embolite [p. 93].—F. Field has given analyses of three varieties of chloro-bromid of silver from Chañarcillo, in the province of Atacama (Quar. Jour. Chem. Soc., x, 239):

	Ag	Br	Cl	
1.	68·22	16·84	14·92	= 99·98
2.	66·94	19·82	13·18	= 99·94
3.	61·07	33·82	5·00	= 99·89

No. 1 had a pale green color, and its formula is $2AgCl+AgBr$. No. 2, of a darker color, and of more frequent occurrence, is identical with Breithaupt's *embolite,* as analyzed by Plattner, $3AgCl+2AgBr$. No. 3 was of a very dark green color, sometimes having a purple tint, its formula is $AgCl+3AgBr$.

Under the names *megabromite* and *mikrobromite* Breithaupt has described two new chloro-bromids of silver (B. u. H. Zeit., xviii, 449). I. *Megabromite.* Lustre adamantine; color siskin to pistachio-green, changing on exposure to the light, to blackish-gray; streak pale green. Crystalline form cubic; cleavage cubic, though not always distinct; fracture conchoidal and uneven; slightly malleable and sectile. H.=2·75—3; sp. gr. 6·230—6·234. Occurs in compact limestone. Analysis by T. Richter:

Ag	Br	Cl	I
64·19	26·49	9·32	*tr.*

The relations of Ag, Br and Cl are as 2·26 : 1·26 : 1, or as 9 : 5 : 4=$4AgCl+5AgBr$. It bears a very strong resemblance to embolite in physical characters.

II. *Mikrobromite.* Lustre adamantine; color between asparagus and greenish-gray, on exposure becomes ash-gray and opaque; streak white, translucent; crystalline form, cubic; fracture irregular, and without any regular cleavage; very sectile and malleable. H.=2·5—3. Sp. gr. 5·75—5·76. Occurs with native silver in a yellowish-red compact limestone at Copiapo in Chile. Two analyses by Richard Müller gave—

	Ag	Br	Cl
1.	70·28	12·35	17·37
2.	69·81	12·44	17·75

The ratio of Ag, Br and Cl is as 4 : 1 : 3, giving the formula $AgBr+3AgCl$.

[In this connection it may be interesting to notice the remarks of Domeyko upon the chloro-bromids of silver (Elementos de Mineralogia, 202). He says: "the chloro-bromids vary in color, from grayish-green or yellow, to asparagus and pistachio-green. In general the specimens that have a yellow color have more bromine, and consequently less silver, than those of a gray or pearly green color." Analyses of three specimens of the yellow variety from the mines of Chañarcillo gave Domeyko:

	AgCl	AgBr		Ag	Br	Cl
1.	51·00	49·00	=	66·53	20·85	12·62
2.	52·80	47·20	=	64·84	20·09	13·07
3.	51·00	49·00	=	66·53	20·85	12·62

The ratio of the atoms of Ag, Br and Cl in Nos. 1 and 3, are as $2\frac{1}{2} : 1 : 1\frac{1}{2}$ or 7 : 3 : 4, giving the formula 4AgCl+3AgBr. No. 2 has the ratio $2\frac{1}{2} : 1 : 1\frac{1}{2}$ or 5 : 2 : 3, giving the same formula as Breithaupt's embolite, 3AgCl+2AgBr. Domeyko also gives four analyses of the grayish green variety which occurs in masses an inch or more in thickness. Of these Nos. 4, 5 and 6, as analyzed below, were from Chañarcillo and 7, from Quillota. All the specimens examined were remarkably homogeneous and pure, some of them as translucent as wax.

	AgCl	AgBr		Ag	Br	Cl	Ratio,
4.	72·9	27·1	=	70·44	11·53	18·03	9 : 2 : 7
5.	65·6	34·4	=	69·14	14·63	16·23	7 : 2 : 5
6.	81·4	18·6	=	71·94	7·92	20·14	20 : 3 : 17
7.	66·4	33·6	=	69·28	14·30	16·42	15 : 4 : 11

If we now recapitulate these together with Field's analyses and the analyses of the three species of Breithaupt, in the order of the increase of bromine, we have:

	Ag	Br	Cl	
Pearly green, Chañarcillo, (6),	71·94	7·92	20·14	Domeyko.
" " " (4),	70·44	11·53	18·03	"
Mikrobromite, Copiapo,	69·84	12·39	17·77	Müller.
Pearly green, Quillota, (7),	69·28	14·30	16·42	Domeyko.
" " Chañarcillo, (5),	69·14	14·63	16·23	"
Light green, "	68·22	16·84	14·92	Field.
Embolite, "	66·94	19·82	13·18	"
" "	66·86	20·08	13·05	Plattner.
" " (2),	66·84	20·09	13·07	Domeyko.
Yellow, " (1 and 3),	66·53	20·85	12·62	"
Megabromite,	64·19	26·49	9·32	Richter.
Dark green, Chañarcillo,	61·07	33·82	5·00	Field.

Here then are ten distinct chemical compounds formed by the union of various proportions of AgCl and BrCl, and so far as known, they all crystallize in the monometric system. As both *bromyrite* (AgBr) and *kerargyrite* (AgCl) crystallize in the monometric system, and as Cl and Br are isomorphous and may replace each other in an infinite number of proportions, it is well to ask, where we shall stop in the making of new species. The five varieties which we have quoted from Domeyko, together with the two varieties analyzed by Field, deserve to rank as species quite as much as embolite, megabromite, and mikrobromite. The varieties of chloro-bromid of silver seem to shade insensibly into each other—the specific gravity increases, and the color deepens in proportion with the increase of bromine. We have already the name embolite admitted in the science, and if the native chloro-bromid should be found which has the ratio 2 : 1 : 1 or AgCl+AgBr, it would have the same right to be ranked a species as dolomite, but we protest against making ten or more species of Ag (Br, Cl), for the same reasons that we should protest against making distinct species of all of the varieties of dolomitic limestone.—G. J. B.]

Fergusonite [p. 350, III].—R. Weber has analyzed *fergusonite* from West Greenland (Pogg. Ann., cvii, 590):

Cb	Sn	Zr	Y	Ce	Fe	U	
48·84	0·35	6·93	38·61	3·05	1·48	0·35	= 99·61

This, although differing somewhat from the former analysis by Hartwall, proves the mineral to be distinct from the Norwegian *tyrite*.

FRANKLINITE [p. 166, I, VII].—The chemical composition of this mineral has been carefully studied by Rammelsberg (Pogg. Ann., cvii, 312), and the causes of error, and the disagreement in former analyses pointed out. His analyses, partly from massive, and partly from crystallized specimens, gave:

G.=5·21		1.	2.	3.	4.	5.
	Fe	64·28	65·32	64·92	63·40	64·64
	Mn		13·08	13·87	13·28	13·81
	Zn			25·09	26·83	25·51
				103·88	103·51	103·96

Analyses 4 and 5 were made by Schulz in Rammelsberg's Laboratory. The mean is Fe 64·51, Mn 13·51, Zn 25·30 (excluding Zn in No. 4) =103·52, equal to

	Fe	Mn	Zn	O	
	45·16	9·38	20·30	25·16	= 100·00
Equivalents,	4·8	1·	1·8	9·3	

The atomic proportion between the metals and the oxygen is 7·6 : 9·3=1 : 1·2=5 : 6 or R^5O^6. Rammelsberg proves by experiment that at least a *portion* of the manganese must be sesquioxyd, and after the consideration of several hypotheses in regard to the oxydation of the iron and manganese, which show the impossibility of the composition of the mineral corresponding to the spinel formula, he is led to assume the *whole* of the manganese to exist as sesquioxyd. He then calculates the oxygen remaining, after deducting that united with the zinc and manganese, as belonging to the iron. This gives—

	a	Oxygen.		*b*	Oxygen.	
Mn	13·51	4·13	13·61	13·51	4·13	12·38
Fe	31·64	9·48		27·50	8·25	
Fe	29·55	6·55	11·55	33·31	7·38	12·38
Zn	25·30	5·00		25·30	5·00	
	100·00		25·16	99·62		24·76

Column *b* is a correction of the calculation so as to make the oxygen of the protoxyds and sesquioxyds equal, and corresponds to the amount of iron found in the analyses. The oxygen thus calculated differs but 0·4 per cent from the total oxygen.

The calculation of the compound R^5O^6 in which Mn : Zn : Fe=1 : 2 : 5 gives Fe 45·24, Mn 8·92, Zn 21·02, O 24·81, or Fe 64·66, Mn 12·80, ZnO 26·19=103·65, corresponding very well with the results of the analyses, and giving the formula $(FeZn)^3(FeMn)$ or $\dot{R}^3\ddot{R}$. Rammelsberg considers the isomorphism of $\dot{R}\ddot{R}$ with $\dot{R}^3\ddot{R}$ as a necessary consequence of the isodimorphism of the oxyds $\dot{R}$ and $\ddot{R}$, already shown in his recent investigations on hornblende, augite, and the different varieties of specular and titanic iron. [An analysis of *franklinite*, made by the writer in the autumn of 1858, gave results agreeing very nearly with the composition obtained by Rammelsberg, viz. Fe 65·05, Mn 14·77, Zn 23·30, insoluble 0·30=103·12.—G. J. B.]

GALENA [p. 36, II—IV, VII].—The so-called *steinmannite*, already referred to galena by Kenngott (Uebersicht, 1855, 109), has recently been analyzed by Schwarz (*Reuss*, Wien. Akad. Ber., xxv, 561), with the following results:

PbS	As_2S_3	SbS_3	ZnS	FeS	
76·48	9·25	0·77	11·38	2·10	= 99·98

Another specimen gave 2 p. c. less lead, a trace of zinc, and scarcely any arsenic, but contained a large amount of antimony. In a third sample a small portion of silver was found. From these facts Prof. Reuss draws the conclusion that sulphid of lead is the only constant constituent, and that *steinmannite* is an impure galena.

GARNET [p. 190, I—VII].—Analyses of *lime-iron-garnet* from the Schischimsk Mts. (1.), and from Achmatowsk (2.), and the *grossular* from the Slüdianka River (3), made under the direction of N. v. Iwanow in the Mining-Department Laboratory in St. Petersburg gave (Kokscharow, Mat. Min. Russlands, iii, 79):

		$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{M}n$
1.	G.=3·798.	35·21	*tr.*	34·11	30·96	*tr.*	*tr.*=100·28
2.	———	37·22	6·04	24·81	31·07	0·49	*tr.*= 99.63
3.	G.=3·427.	40·99	14·90	10·94	32·94	0·98	—=100·75

An extended notice of the occurrence of *garnet* in Russia, with description of interesting crystals is given by Kokscharow in his Materialen zur Mineralogie Russlands, iii, 1—40.

GERSDORFFITE [p. 58, VII].—Dr. Genth has detected crystals of this mineral occurring as an incrustation upon partially decomposed galena and blende at Phœnixville, Pa. The crystals are cubes with octahedral planes, and sometimes, though rarely, pentagonal dodecahedrons are found.—This Jour., [2], xxviii, 248.

GLASERITE [p. 365, III].—Prof. W. J. Taylor refers to *glaserite*, with a query, a sulphate of ammonia and potash from the Chincha Islands. It occurs in concretions a half or three-quarters of an inch in diameter. Color yellowish-white; structure crystalline; taste pungent and bitter; opaque; permanent in air. Hardness 2. B.B. blackens and fuses with difficulty, giving a white bead. The results of two analyses were:

	$\ddot{S}O_3$	NH^4O	KO	NaO	
1.	48·40	5·37	43·45	1·68	= 98·90
2.	48·30	5·10	46·49		= 99·89

Both specimens contained traces of organic matter. The composition gives the formula $(NH^4O, KO, NaO)\ddot{S}O^3$, which differs from that of glaserite only in having a portion of the potash replaced by ammonia and soda (Proc. Acad. Nat. Sci. Philad., Nov., 1859).

HALLOYSITE [p. 251].—Nöggerath has described as a variety of opal, a mineral occurring in a soft gelatinous state in trachyte in the opal mine at Czerweitza in Hungary (Verhand. d. naturhist. Ver. d. Rheinlande, xv, cii). Upon exposure to the atmosphere the mineral hardens, and its characters approach those of jasper-opal. Analysis by Landolt showed the mineral to lose 5·30 p. c. by drying over sulphuric acid, and the dried substance gave:

$\ddot{S}i$	$\dot{A}l\dot{F}e$	$\dot{C}a$	$\dot{H}$	
46·96	36·56	*tr.*	16·10	= 99·62

[Assuming the iron to be an unimportant ingredient, this composition corresponds to the varieties of *halloysite* from Anglar and Housscha (Min., p. 151, anal. 1, 2).—G. J. B.]

HAYESINE [p. 394, III, IV].—Analyses of very pure *hayesine* by Reichardt (Kopp's Jahresbericht, 1858, 737):

	$\ddot{B}a$	$\dot{C}a$	$\dot{N}a$	$\ddot{S}$	Cl	Insol.	$\dot{H}^b$	$\dot{H}^c$
1.	52·05	11·56	*tr.*	0·53	0·94		33·53	1·38
2.	50·42	12·10	*tr.*	1·07	1·21	0·67	33·67	0·87

(*a*) by the difference. (*b*) expelled at a red heat. (*c*) expelled at 100° C.

No. 1 was a specimen from the German importers, and 2 was received direct from Lima. Reichardt gives the formula $\dot{C}a\ddot{B}^4+10\dot{H}$. An analysis of *hayesine* by F. W. Helbig (Dingler's Polytech. Jour., cxlvii, 319) gave $\ddot{B}$ 46·46, $\dot{C}a$ 14·03, $\dot{N}a$ 5·17, $\dot{H}$ 32·61, NaCl 1·89, $\dot{M}g$ and $\ddot{S}i$ traces. Additional analyses of the commercial article are given in Barreswil's Réperetoire de Chimie Appliquée, i, 215.

HEMATITE [p. 113, II, III, IV, VII].—A specimen of tabular crystalline hematite from Vesuvius analyzed by Rammelsberg (Pogg. Ann., cvii, 453) gave $\dot{F}e$ 98·05, $\dot{M}g$ 1·40=99·45. It contained no protoxyd of iron.

Analyses of hematite from the Lake Superior region by Prof. J. D. Whitney (this Jour., [2], xxviii, 13):

	I.			II.		III.	
	a.	*b.*	*c.**	*a.*	*b.*	*a.*	*b.*
Insoluble,	1·02	·80	·54	7·92	7·96	1·99	2·05
Iron,	69·41	70·22	69·96	64·42	64·01	68·81	
Oxygen and traces, lime, &c.,	29·57	28·98	29·50	27·66	28·03	29·20	

* Mean of three closely-agreeing determinations.

I. from the Jackson Mountain, II. from the Cleveland, and III. from the Burt or Lake Superior Mountain.

HOMICHLINE [VII].—This mineral was referred to *barnhardtite* in the last supplement, but the recent analysis of Richter, published by Breithaupt in the B. u. H. Zeitung, xviii, 321, gives its composition as Fe 25·81, Cu 43·76, S 30·21, and the formula $(Cu^2S)^3$, Fe^2S^3+2FeS. Sp. gr. 4·47—4·48, (Breithaupt.) [The identity of crystalline form of this mineral with chalcopyrite, together with its less degree of hardness, and the difficulty of obtaining it pure and free from admixture with chalcopyrite, would seem to indicate that it might be a product of decomposition of this latter species, or perhaps a mixture of this species, with some of the richer sulphids of copper, such as erubescite or copper-glance. It is interesting in this connection to note Dr. Genth's remarks upon the occurrence of barnhardtite with copper-glance and chalcopyrite at the Pioneer Mills mine (this Journal, [2], xxviii, 248). —G. J. B.]

HORNBLENDE [p. 170, I—IV, VI, VII].—A. Knop has published a description and several analyses by himself and W. Hoffman of an interesting soda hornblende from the serpentine rock at Waldheim in Saxony (Ann. d. Chem. u. Pharn., cx, 363). Color leek-green; translucent; occurring in veins of an inch in thickness and resembling actinolite. H.=5. Sp. gr. 2·957.

	$\tilde{Si}$	$\dot{\tilde{A}}l$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	$\dot{M}g$	$\dot{N}a$	
1.	58·71[a]	1·52	5·65	0·25	11·53	10·01	12·38=100·05	Knop.
2.	58·45[a]	1·92	5·53	0·51	10·28	11·12	12·61=100·42	Hoffmann.
3.	58·45[a]	1·74	5·79	0·32	10·76	10·83	12·93=101·12	"

(*a.*) Mean of two determinations.

It lost 0·5 per cent by ignition. [The analyses give too much silica for the hornblende formula, but this may be accounted for on the supposition that the mineral was partially decomposed, when treated by hydrochloric acid to free it from adhering carbonates. The large percentage of soda is remarkable.—G. J. B.]

IODYRITE [p. 95, 506].—An analysis of iodyrite from Delirio's Mine, Chañarcillo, afforded F. Field, Ag 45·98, I 54·02=AgI.—(Quar. Jour. Chem. Soc., x, 241).

IRIDOSMINE [p. 19, I].—Analyses of *iridosmine* from various localities by Deville and Debray (Ann. Chim. et Phys., [3], lvi, 481).

	Ir	Rd	Pt	Ru	Os	Cu	Fe	
1. Columbia,	70·40	12·30	0·10	—	17·20	—	—=100·	
2. "	57·80	0·63	—	6·37	35·10[a]	0·06	0·10=100·06	
3. California,	53·50	2·60	—	0·50	43·40	—	—=100·	
4. Australia,	58·13	3·04	—	5·22	33·46	0·15	—=100·	
5. Borneo,	58·27	2·64	0·15	—	38·94	—	—=100·	
6. Russia,	77·20	0·50	1·10	0·20	21·00	*tr.*	—=100·	
7. " G.=18·9,	43·28	5·73	0·62	8·49	40·11	0·78	0·99=100·	
8. " G.=18·8,	64·50	7·50	2·80	—	22·90	0·90	1·40=100·	
9. " G.=20·4,	48·94	1·65	0·14	4·68	48·85	0·11	0·63=100·	
10. " G.=20·5,	70·36	4·72	0·41	—	23·01	0·21	1·29=100·	

(*a.*) In this analysis, the osmium was determined directly, in the others by difference.

IRON [p. 17, II, VII].—F. A. Genth describes in this Journal, [2], xxviii, 246, a specimen of what appears to be telluric iron. It is said to occur near Knoxville, in Tennessee, although its exact locality is not known. It contains neither carbon, phosphorus or sulphur, and its peculiar appearance together with its being assoeiated with a silicate of magnesia, iron and lime, render it probable that it may be a genuing specimen of *native iron*. Dr. Genth describes the mass examined, to have been about one and a half inches square, and three-eights of an inch in thickness. The

iron had a grayish-white color, a hackly fracture, and broke easily into fragments, which though crystalline, did not show any distinct planes. It was soft, scarcely scratching fluor-spar. Lustre eminently metallic. Readily dissolved by nitric acid. Composition:—

Fe	Ni	Co	Mg	Ca	Si	
99·790	0·140	*tr.*	0·022	0·121	0·075	= 100·148

A similar mineral has been received by Dr. Genth from Northern Alabama, and it is exceedingly desirable that more definite information should be obtained in regard to the locality and mode of occurrence of this problematical substance.

KERARGYRITE [p. 92, IV].—A specimen of chlorid of silver from the "Republicana Mine," Chañarcillo, analyzed by F. Field (Quar. Jour. Chem. Soc., x, 239) contained:

Ag	Cl		
75·27	24·73	=	AgCl.

LABRADORITE [p. 237, II, VII].—Vom Rath gives as the composition of the *labradorite* from the gabbro of Marmorera in Graubündten:

	Si	Al	Fe	Ca	Mg	K	Na	
G.= 2·840.	55·45	22·12	4·28	9·68	1·30	1·64	5·73	=100·20

This mineral lost 2·76 p. c. on ignition.—Zeitsch. d. deutschen geolog. Gesellschaft, ix, 246.

LIBETHENITE [p. 420].—Analyses of *libethenite* from Congo in Portuguese Africa by Hugo Müller (1.) (Quar. Jour. Chem. Soc., xi, 242); from Libethen (2.) by Bergemann, and from Nischne-Tagilsk (3.) by Chydenius (Kopp's Jahresbericht, 1858, 726):

	Cu	P	As	H	Fe	O	
1. Congo,	67·21	28·76		4·03			=100·00
	66·76	29·02		4·22			=100·00
2. Libethen,	66·29	26·46	2·30	4·04			= 99·09
3. Nischne-Tagilsk,	64·47	29·48	*tr.*	3·68	1·77	0·82	=100·22

LILLITE, *Reuss* (Wien Akad. Ber., xxv, 550).—This name has been given by Reuss to a mineral which occurs at Przibram in Bohemia. In physical characters it resembles glauconite, and appears to be a product of the decomposition of pyrites. It is an amorphous, lustreless, earthy substance, having a hardness = 2, and sp. gr. 3·043. Color blackish-green, in very fine powder under the microscope is leek-green by transmitted light. Material selected as pure as possible gave Payr on analysis:

Si	Fe	CaC	FeS (insol.)	H	
32·48	54·95	1·96	0·63	10·20	= 100·22

On treating the mineral with nitric acid, Payr found after ignition and making allowance for the water, the sulphur of the pyrites, and the carbonic acid, that the iron in the mineral had absorbed 3·43 per cent of oxygen, so that deducting this from the 54·95 Fe we have 51·52 per cent of iron plus oxygen. This gives a total of 96·79, a loss of 3·21 per cent in the analysis. The author places the species near hisingerite and cronstedite. It corresponds very closely in chemical composition with the variety of hisingerite from Riddarhyttan in which Rammelsberg found (Min., p. 290):

Si	Fe	Fe	Ca	Mg	H
33·07	34·78	17·59	2·56	0·46	11·54

MAGNESITE [p. 447, II, III].—Analyses of the magnesite from Snarum and Frankenstein by T. Scheerer (Jour. f. prakt. Chem., lxxvi, 424):

	C	Mg	Fe	Ca	
Snarum (crystallized),	52·13	46·66	0·78	0·43	= 100·00
Frankenstein (amorphous),	55·34	47·43	—	0·22	= 100·00

The small amount of mechanical mixture, amounting in the Snarum specimen to 0·05--0·1405 per cent, and in the Frankenstein specimen to 0·048 p. c., have been subtracted from the above.

MAGNOFERRITE.—Rammelsberg (Pogg. Ann., cvii, 451) gives this name to the octahedral iron which occurs interlaminated with hematite, in the fumaroles formed at Vesuvius after the eruption of 1855. His former analyses showing the presence of a considerable amount of magnesia are contained in Suppl. VII. Two additional analyses of portions selected out by means of the magnet from the finely pulverized mineral gave:

		Ḟe	Ṁg	Ċu	Insol.	
1.	G.=4·568.	82·91	13·60	0·99	2·51	= 100·01
2.	G.=4·638.	83·30	13·41	0·59	2·00	= 99·30

which, excluding the oxyd of copper and insoluble portions, gives (1.) Ḟe 85·92, Ṁg 14·09=100·01, and (2.) Ḟe 85·51, Ṁg 13·77=99·28.

The former analyses (Suppl. VII) thus calculated are: *a*. Ḟe 86·96, Ṁg 12·59= 99·55; *b*. Ḟe 84·20, Ṁg 16·00=100·20; *c*. Ḟe 84·35, Ṁg 15·65=100. Analysis *a* was made from selected crystals, *b* was a portion extracted by the magnet from the associated hematite, while *c* was a specimen thus selected from one of the older Vesuvian hematites. Rammelsberg considers the composition of the crystals as $\dot{M}g^m\dot{F}e^n$ in which probably $m=3$ and $n=4$, the regular (monometric) form being due to the isodimorphism of Ṙ and R̈.

MARGARODITE [p. 223].—An analysis of an authentic specimen of margarodite from the original locality at Pfitsch gave Hlasiwetz (Kenngott's Uebersicht, 1858, 67):

S̈i	Ä̇l	Ḟe	Ċa	K̇	Ṅa	Ign.	
45·48	33·80	6·25	0·48	7·31	6·22	0·36	= 99·90

Kenngott remarks that this composition may be represented by the formula $\dot{R}\ddot{S}i + 2\dot{R}\ddot{S}i$, but adds that this is of little value, as on closer examination with the magnifier, the specimen proved to be an intimate mixture of a mica with granular quartz and minute crystals of feldspar. [The mica from Lane's Mine, analyzed by Smith and myself, and referred by Dana to margarodite, is distinctly foliated and apparently perfectly homogeneous. It is identical in composition with the so-called margarodite from St. Etienne analyzed by Delesse (Min., p. 224).—G. J. B.]

MARIONITE (*Elderhorst*), see *Zinc-bloom*.

MEGABROMITE (*Breithaupt*), see *Embolite*.

MIKROBROMITE (*Breithaupt*), see *Embolite*.

MISPICKEL [p. 62, 509, I, II, III, V].—Analyses of mispickel from Sahla in Sweden by J. Potyka (Pogg. Ann., cvii, 304):

	S	Fe	As	Sb	Bi	
G.=6·095.	19·13	34·78	43·26	1·29	0·14	= 98·60

These results give the received formula, $FeAs_2+FeS_2$, differing from the analysis by Behncke (Suppl. III), which corresponded to $3FeS^2+2Fe^2As^3$. Potyka shows the want of agreement between the analyses to be due to the fact that mispickel suffers partial decomposition by simply boiling in water. The sp. gr. of small fragments he found to be 6·043—6·047, that of the powder boiled for some time in water was only 5·819 to 5·874, and on examination of the water appreciable quantities of sulphuric acid, iron and arsenic were found in solution.

C. v. Hauer obtained in two analyses of mispickel from Kindberg in Styria:

S̈i	Ä̇l	Ċa	Fe	As	S	
5·0	1·0	0·3	30·8	43·2	18·9	= 99·2
0·7	0·3	*tr.*	32·7	45·0	21·0	= 99·7

—Jahrb. d. k. k. geolog. Reichsanstalt in Kopp's Jahresbericht f. 1858, p. 678.

According to Daubrée (Compt. Rendus, 1858, xlvii, 959) the lignite of the tertiary formation at Lobsann (Lower Rhine) contains from 0·002—0·0008 per cent of arsenic, and on dissolving the bituminous limestone from the same locality a fine amorphous residue (amounting to about 2 p. c.) is obtained, which gives the reactions of mispickel.

MOLYBDATE OF IRON [p. 144, I, II].—The so-called *molybdate of iron* described by D. D. Owen has recently been examined by Dr. Genth (this Jour., [2], xxviii, 248), and from the varying proportions of the iron—in one case 35 p. c., in another 24·3—he questions whether the substance may not be a mechanical mixture of molybdine and limonite.

MOSSOTTITE.—See *Aragonite.*

NAGYAGITE [p. 65].—Folberth has analyzed the foliated-tellurium from Nagyag. It occurs in six-sided tables in a pearl-gray quartz, and has a specific gravity =6·68. Treatment with sulphid of carbon extracted 25 p. c. of the amount of sulphur. Two analyses gave (Verhandl. d. siebenbürg. Ver. f. Naturwissensch., viii, 99, in Kenngott's Uebersicht f. 1856–7, 179):

Pb	Au	Sb	S	Te	Se
60·83	5·84	3·69	9·76	17·22	*tr.* = 97·34
60·27	5·98	3·86	9·68	18·04	*tr.* = 97·83

differing very materially from the previous analyses by Klaproth, Brandes and Schönlein.

NATROLITE [p. 327, VI, VII].—A variety of transluscent natrolite from Fassa in Tyrol, analyzed by Hlasiwetz, gave the following composition (Kenngott's Uebersicht, 1858, 72):

$\ddot{S}i$	$\dot{A}l$	$\dot{C}a$	$\dot{M}g$	$\dot{N}a$	$\dot{H}^a$	$\dot{H}^b$
43·34	27·43	3·60	0·40	9·00	10·30	0·90 = 99·97

(*a*) basic water. (*b*) hygroscopic water.

agreeing very nearly with the composition of galactite, which has been shown to be a variety of natrolite by Dana and Heddle (Suppl. I and III).

In an article upon *Spreustein* (natrolite) (Pogg. Ann., cviii, 431) Scheerer shows the cause of color, of the red and brown varieties of this mineral, to be due to mechanical impurities. A microscopical examination of several varieties showed that only the perfectly white specimens were entirely free from mechanical mixture. The white varieties were perfectly decomposed by chlorhydric acid giving a homogeneous jelly, while with the colored varieties the gelatinized mass always contained suspended more or less of an opaque white powder. If however, the decomposition was made with nitric acid, this powder retained the original color of the natrolite. A separation of the insoluble powder on some twenty grams of the mineral gave material to determine the character of this substance. The results of two analyses prepared from different varieties gave:

	$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{H}$
1.	1·58	76·75	6·77	14·70 = 99·80
2.	0·82	82·56	1·52	15·00 = 99·90

These give the formula $\ddot{R}\dot{H}$, and the powder is *diaspore* in which a portion of the alumina is replaced by iron. The quantity of this mineral in the specimens of natrolite analyzed by Scheerer varied from 4 to 7 p. c. This will explain the reason of the different analyses of Spreustein differing from each other, and also from pure natrolite. The following analyses may serve as examples. I. Crystallized colorless natrolite from Brevig analyzed by Dr. Sieveking in Scheerer's laboratory. II. Dark brownish-red Spreustein from an island of Brevigfjord, by Scheerer.

	$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{N}a$	$\dot{H}$
I.	47·16	26·13	0·53	0·53	15·60	9·47 = 99·42
II.	44·50	30·05	0·98	0·83	13·52	9·93 = 99·81

II. contained 6¾ p. c. of diaspore, which when subtracted from the above gave $\ddot{S}i$ 47·47, $\dot{A}l$ 26·83, $\dot{F}e$ 0·60, $\dot{C}a$ 0·88, $\dot{N}a$ 14·42, $\dot{H}$ 9·61=99·81; this is almost exactly the composition obtained for I. Scheerer adds that the so-called *brevicite* (Min., 327, anal. 14, 15, and 16) is nothing more than natrolite, which contains a considerable percentage of diaspore. For the further consideration of the disputed points upon the paramorphous nature of *Spreustein* (Paläo-Natrolith) see the original paper in Pogg. Ann., cviii, pp. 416—435.

NEPHELINE [p. 232, II].—P. v. Pusirewsky has analyzed the *elæolite* which is associated with cancrinite, zircon and other minerals in the Graphite-Mine at Mariinskaja in the Tunkinsk Mts. (Kokscharow Mat. Min. Russlands, III, 78), and J. P. Kimball has described the same variety of nepheline, occurring with sodalite at Salem, Mass. (this Jour., [2], xxix, 65):

	Si	Al	Fe	Ca	Mg	Na	K	Ign.
1. Mariinskaja,	44·94	30·29	0·72	1·15	0·15	21·80	1·48	—=100·53 P.
2. Salem, G.=2·63.	44·31	32·80	*tr.*	0·40	—	16·43	5·50	1·47=100·91 K.

Nickel and Copper, arseniuret of.—This ore, mentioned by T. Sterry Hunt (this Jour., [2], xix, 417) as a mixture of *domeykite* and *copper-nickel*, has since been thoroughly examined by both Prof. Hunt (Rep. Geol. Survey, Canada, 1853–6, p. 388) and Prof. J. D. Whitney (this Jour., [2], xxviii, 15) giving analyses which confirm the above conclusion.

NICKEL-GYMNITE [p. 286, VII].—An ore, apparently an impure variety of this mineral, is described by T. Sterry Hunt (this Jour., [2], xix, 417, and Rep. Geol. Survey, Canada, 1853–6, p. 389), as occurring with the nickel ores of Michipicoten Island, Lake Superior.

OLIGOCLASE [p. 239, I].—Vom Rath has given analyses of *oligoclase* from the granite of Albulaberge, and also of a compact *lime oligoclase* from the diorite of Piz-Rosag, in Granbündten (Zeitschr. d. deutsch. geol. Gesell., ix, 226, 259):

	Si	Al	Fe	Ca	Mg	K	Na
Albulaberge, G.=2·725.	62·01	21·16	2·54	3·53	0·78	4·33	5·94=100·29
Piz-Rosag, G.=2·835.	57·64	22·99	3·92	8·09	0·37	1·79	5·25=100·05

The analyses were made on the ignited mineral. The specimen from Albulaberge lost 1·05 per cent, and that from Piz-Rosag 1·32 per cent on ignition.

The leek-green *feldspar* associated with pyrrhotine at Bodenmais (Bavaria) has been analyzed by Potyka (Pogg. Ann., cviii, 366). It is triclinic, and has the characteristic striæ on the cleavage surfaces. Sp. gr. 2·604. Composition:

Si	Al	Fe	Ca	Mg	K	Na
63·12	19·78	1·51	0·66	0·13	12·57	2·11 = 99·87

This gives an oxygen ratio of $\dot{R} : \dot{R} : Si$, of 1 : 2·86 : 10·17 or nearly 1 : 3 : 10,—a ratio intermediary between that of oligoclase and orthoclase; the sp. gr. (2·604) is also between that of oligoclase (2·56) and orthoclase (2·67).

ORTHOCLASE [p. 242, II, III, V—VII].—J. D. Whitney has analyzed the interesting crystallized *orthoclase* which is associated with native copper, calcite and the zeolites in many of the Lake Superior copper mines (this Jour., [2], xxviii, 16). It occurs in distinct crystals of a reddish flesh color, having a striking resemblance to stilbite. The crystals are rarely as much as one-tenth of an inch in length. Composition:

Si	Al	Fe	Mn	K	Na
65·45	18·26	0·57	*tr.*	15·21	0·65 = 100·14

OSERSKITE (*Breithaupt*), see *Aragonite*.

PECTOLITE [p. 305, II, III, VI, VII].—Analyses of three specimens of the very pure variety of this mineral from Bergen Hill, N. J., by J. D. Whitney (this Jour., [2], xxix, 205):

	Si	Ca	Mn	Fe	Na	H[a]
1.	54·82	33·12	0·66	2·6	8·78	2·36
2.	54·76	32·88	1·16		9·17	2·03
3.	54·27	32·83	1·24		8·94	2·72

(*a*) By the difference.

The direct determination of the water on the substance dried at 80° C. gave, for 2, 3·03, and for 3, 2·75 p. c. These results agree very closely with the previous analysis published by Prof. Whitney (Min., anal. 5). Specimen 3, from the Wheatley Collection in Union College, was considered the purest, and the analyses gives

the oxygen ratio for Ḣ, Ṅa, Ċa, S̈i as 1 : 1·05 : 3·83 : 11·84 or nearly 1 : 1 : 4 : 12, or the formula, as expressed by Prof. Whitney, $\dot{N}a^2\ddot{S}i^4+4\dot{C}a^2\ddot{S}i^2+3\dot{H}=\ddot{S}i$ 54·22, Ċa 33·73, Ṅa 9·33, Ḣ 2·74. This corresponds much better with the results obtained than v. Kobell's formula, in which the oxygen ratio is 1 : 1 : 4 : 1¼.

Prof. Whitney calls attention to the relations of pectolite to spodumene, and also to wollastonite and pyroxene, the latter connection being more apparent when the formula is written $(\dot{C}a\frac{4}{6}, \dot{N}a\frac{1}{6}, \dot{H}\frac{1}{6})^2\ddot{S}i^2$ or $\dot{R}^2\ddot{S}i^2$.

Pennine [p. 295, II, IV, V].—A new analysis of *pennine* from Zermatt by Victor Merz, gave (Kenngott's Uebersicht f. 1858, 63):

S̈i	Ä̇l	Ḟe	Ṁn	Ṁg	Ḣ	
33·26	11·69	7·20	*tr.*	35·18	12·18	= 99·51

not differing materially from the previous results of Marignac, Schweizer and MacDonnel. For an extended discussion of the chemical composition of this mineral by Dr. Kenngott, see loc. cit., pp. 62—66.

Pholerite (p. 251].—F. A. Genth describes (this Jour., [2], xxviii, 251) *pholerite* as occurring in the coal mines of Schuylkill Co., Pa., in yellowish white scales which become of a snow white color, and pearly lustre, on being treated with chlorhydric acid. Under the microscope the scales appear to be clinorhombic, having the planes i-$\bar{i}$ and -1-i. The specimens examined were from Tamaqua near Pottsville. Analysis No. 1, was made on the original mineral, in Nos. 2 and 3 the substance had been previously treated with chlorhydric acid. Nos. 1 and 2, were decomposed by fusion with carbonate of soda—No. 3 by treatment with sulphuric acid.

	S̈i	Ä̇l	Ḟe	Ċa	Ṅa	K̇	Ḣ	
1.	46·93	37·90	0·18	0·93	*undetermined*		13·98	= 99·92
2.	46 98	39·65	—	—	0·11	0·06	13·69	= 100·49
3.	46·81	39·56	—	—	0·11	0·06	13·91	= 100·45

giving the formula $\dot{Al}^2\ddot{S}i^4+6\dot{H}=\ddot{S}i$ 47·06, Ä̇l 39·20, Ḣ 14·71.

Phosphochalcite [p. 425, II, VI, VII].—Bergemann has found arsenic acid in all the native phosphates of copper.—Analysis of the *phosphochalcite* from Linz gave:

	Ċu	P̈	Ä̇s	Ḣ	
Phosphochalcite,	69·97	19·89	1·78	8 21	= 99·85

(Abstract from Pogg. Ann., civ, 190, in Kopp's Jahresbericht, 1858, 726).

Pitchblende [p. 107, IV, V].—Hermann has given (Jour. f. prakt. Chem., lxxvi, 326) the name *uranoniobite* to the crystallized pitchblende from Strömsheien in Norway, previously described and analyzed by Scheerer (see Min., p. 108, anal. 5, under pitchblende). Scheerer remarks in his description (Pogg. Ann., lxxii, 568) that it is possible that the metallic acids found in the analysis may be due to admixture with a substance he calls *Niob-pelopsaures Uran-Manganoxydul* (columbate of uranium and manganese) with which the pitchblende is associated;—further investigation is needed to establish its claims to be considered a distinct species.

Hermann also gives a new analysis of the *pitchblende* from Joachimsthal:

	PbS	S̈i	Ä̇l	Ḟe	B̈i	C̈u	Ü	Ṗb	Ṁn	Ċa	Ṁg	Ḣ
G.=6·97.	2·84	2·45	0·33	1·88	1·23	52·37	28·84	0·74	0·14	5·78	0·41	2·59

with traces of arsenic. In the same paper Hermann communicates an analysis of the so-called *pittinite* (Pittinerz, Breithaupt) from Joachimsthal. The mineral occurs in amorphous opaque masses of a pitch-black color. It has an uneven and slightly conchoidal fracture, and a highly resinous lustre. Streak greenish-brown. H.=4. Sp. gr. 5·16. Heated in tube yields water containing traces of fluorine and ammonia; fused with soda on charcoal gives a globule containing lead and bismuth. Easily decomposed by nitric acid with separation of gelatinous silica on evaporation. Composition:

S̈i	C̈u	Ḟe	B̈i	Ṗb	Ċa	Ṁg	Ḣ	Insol.	
5·00	68·45	4·54	2·67	. 2·51	2·26	0·55	10·06	3·20	= 99·23

with traces of fluorine, ammonia, phosphoric and carbonic acids. [This substance is evidently very nearly related to pitchblende, and is probably a result of the alteration of that mineral. Hermann endeavors to show that silica is an essential constituent of pitchblende and allied uranium minerals, but as most of these substances are amorphous, and as their composition varies considerably, it seems possible that the silica may be due to admixture with some earthy silicate.—G. J. B.]

PITTINITE.—See *Pitchblende.*

PLATINUM [p. 12, I—IV].—Analyses of platinum from various localities, by H. St.Claire Deville and H. Debray (Ann. de Chimie, [3], lvi, 449):

	Pt	Ir	Rh	Pd	Au	Cu	Fe	X[a]	Sand	Pb(?)	Os & loss	
1.	86·20	0·85	1·40	0·50	1·00	0·60	7·80	0·95	0·95	—	—	=100·25
2.	80·00	1·55	2·50	1·00	1·50	0·65	7·20	1·40	4·35	—	—	=100·15
3.	76·82	1·18	1·22	1·14	1·22	0·88	7·43	7·98	2·41	—	—	=100·28
4.	85·50	1·05	1·00	0·60	0 80	1·40	6·75	1·10	2·95	—	—	=101·15
5.	79·85	4·20	0·65	1·95	0·55	0·75	4·45	4·95	2·60	—	0·05	=100·00
6.	76·50	0·85	1·95	1·30	1·20	1·25	6·10	7·55	1·50	0·55	1·25	=100·00
7.	51·45	0·40	0·65	0·15	0·85	2·15	4·30	37·30	3·00	—	—	=100·25
8.	45·70	0·95	2·65	0·85	3·15	1·05	6·80	2·85	35 95	—	0 05	=100·00
9.	59·80	2·20	1·50	1·50	2·40	1·10	4·30	25·00	1·20	—	0·80	=100·00
10.	61·40	1·10	1·85	1·80	1·20	1·10	4·55	26·00	1·20	—	—	=100·20
11.	77·50	1·45	2·80	0·85	*undet.*	2·15	9·60	2·35	1·00	—	2·30	=100·00
12.	76·40	4·30	0·30	1·40	0·40	4·10	11·70	0·50	1·40	—	—	=100·50

(a.) Iridosmine.

Nos. 1, 2, and 3 from Choco (Columbia), South America; 4, 5, and 6 from California; 7, Oregon; 8, Spain; 9, 10, Australia; 11, 12, Russia.

For analyses of platinum ore from Goenoeng Lawack in Borneo by Prof. Bleekrode see Pogg. Ann., cvii, 189.

PYRITES [p. 54, I, IV].—G. Rose has described a pseudomorph of *pyrites* after *pyrrhotine*, the crystals are six-sided prisms, two inches across and one inch in thickness (Zeitschrift d. deutsch. geolog. Gesellschaft, x, 98).

PYROMORPHITE [p. 400, II, IV].—Analyses of Russian *pyromorphite* by Struve (Kokscharow, Mat. zur Min. Russlands, iii, 42):

		PbCl	Ṗb	Ḟe, Ċr	Ȧs	V̈	P̈	
1. Beresowsk,	G.=6·715.	9·94	73·36	0·59	—	*tr.*	15 82	=99·71
2. Altai (Tomsk),	G.=5·537.	10·13	73·40	—	2·61	—	12·90	=99·04

PYROXENE [p. 158, I, II, V—VII].—Reuss has described a compact white *pyroxene* from Oberrochlitz in Bohemia. Under the microscope it shows a crystalline structure. The mineral is snow white when pure, but sometimes has a light green color from admixture with chrysocolla, malachite and allophane. H.=5·5—6. G.=3·398. Decomposed by chlorhydric acid with gelatinization. Analysis by v. Payr (Wien Akad. Ber., xxv, 557):

S̈i	Ċa	Ṁg	Ḟe	Ṁn	
55·03	20·72	15·71	4·84	3·16	=99·46

PYRRHOTINE [p. 50, I, II].—Analysis of *pyrrhotine* from Bernkastel on the Moselle by Baumert gave Fe 61·0, S 39·4, and no nickel (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xiv, s. lxxxv). For observations on nickeliferous pyrrhotine from Snarum see Müller in B. u. H. Zeitung, xvii, 304.

QUARTZ [p. 145, II—IV, VII].—Blum and Carius have described *quartz* as pseudomorph of *celestine*, from Girgenti. The crystals contained S̈i 98·80, ṠrS̈ 1·78 (Pogg. Ann., ciii, 628, in Kopp's Jahresbericht, 1858, 745).

REALGAR [p. 31, VI].—Analysis of *realgar* from Pola de Lena in Asturia, Spain, by Dr. Hugo Müller (Quar. Jour. Chem. Soc., xi, 242) gave S 30·00, As 70·25.

RIPIDOLITE [p. 296, I, V].—An interesting and peculiar variety of this mineral from Steele's Mine, Montgomery Co., North Carolina, has been described by Dr. F. A. Genth in this Journal, [2], xxviii, 250. Composition:

Si	Al	Fe	Fe	Mn	Mg	H
24·90	21·77	4·60	24·21	1·15	12·78	10·59

SAPONITE (Nicklès).—For a more extended description of this silicate noticed in the Suppl. VII, see Ann. de Chimie, [3], lvi, 46.

Saualpite.—A synonym for a variety of zoisite from the Saualp in Carinthia.

SCHEELITE [p. 347].—F. A. Genth has found *scheelite* at the Bangle Mine in Cabarras Co., and also at Flowe mine, Mecklenburgh Co., North Carolina. At the former place it occurs in granular masses three-fourths of an inch in diameter; it has a pale yellowish-brown color, and a distinct octahedral cleavage. Composition (this Jour., [2], xxviii, 252):

W	Sn	Cu	Fe	Ca	
79·52	0·13	0·08	0·18	19·31	= 99·22

The variety from Flowe mine was observed in crystals, in one case a modification of the octahedron 1, truncated by 1-*i*,—crystal about three-tenths of an inch in length; another specimen, half this size, had an orange color and was a combination of the planes $\frac{1}{2}$ and *i-i*.

Another variety from Flowe mine, forming what Dr. Genth calls *rhombic tungstate of lime*, occurs in small indistinct crystals—the largest one-quarter of an inch long. Each crystal has a nucleus of wolfram, and the following planes are given: *I*, *i-i*, $\frac{1}{2}$-*i*, 1, and 1-*i*; cleavage could not be observed. Dr. Genth does not believe these crystals to be pseudomorphs, and suggests that tungstate of lime is dimorphous,—a conclusion which, though extremely interesting, we hesitate to accept until the subject has been more fully investigated.

SERPENTINE.—Observations on the crystalline structure of serpentine by Websky in Zeit. d. deutschen geol. Gesellsch., x, 277.

SMITHSONITE [p. 447, I, III, VII].—For analyses of zinc ores from Arkansas by Dr. Elderhorst see First Geological Report of Arkansas, pp. 147–155.

SODALITE [p. 229, II, VI].—J. P. Kimball has published a description and analysis of sodalite, from an erratic block of compact syenite at Salem, Mass. (this Jour., [2], xxix, 67). The mineral was associated with elæolite, orthoclase, biotite, zircon, and albite (?) Occurs in crystalline, sub-translucent masses; cleavage indistinct; lustre greasy; color lavender-blue. Sp. gr. on three specimens 2·294, 2·303, 2·314. Chemical composition:

Si	Al	Fe	Na	Cl	
37 33	32·70	*tr.*	24·31	6·99	= 101·33

Calculating the chlorine to exist as chlorid of sodium we have:

Si	Al	Fe	Na	Na	Cl	
37·33	32·70	*tr.*	18·17	4·57	6·99	= 99·76

corresponding very closely to the analyses of the sodalite from Litchfield in Maine by Whitney (Min., anal. 5, 6). Dr. Kimball remarks that the sodalite from both Litchfield and Salem, is found in erratic blocks, but the absence of cancrinite as an associating mineral in the Salem specimens, would seem to favor their being derived from different sources.

STROMEYERITE [p. 48].—Prof. W. J. Taylor has described and analyzed a variety of *stromeyerite* occurring at Copiapo in Chile (Proc. Acad. Nat. Sci. Phila., Nov. 1859). It is found in small six-sided trimetric crystals not larger than one-eighth of an inch in diameter. Its hardness is 2·5—3. Lustre metallic; color dark steel-gray; streak nearly black and shining. Sectile, crystals brittle. It occurs in barytes in small cavities associated with quartz crystals, and upon the latter are implanted the crystals of stromeyerite, together with small crystals of pyrargyrite. Analysis gave:

S	Ag	Cu	Fe		
16·35	69·59	11·12	2·86	=	99·92

This composition differs materially from the published analyses of stromeyerite, although not more than the analyses of specimens from different localities vary

from each other. Cu and Ag appear to replace each other in this mineral in all proportions. The formula is (Cu, Ag, Fe)S.

TALKOID, *Naumann* (Mineralogie, 5te, Aufl. 255).—The sparry crystalline talc from Presnitz described by Scheerer (Pogg. Ann., lxxiv, 321, this Jour., [2], xiv, 39) has been named *talkoid* by Naumann. It is snow white and broadly foliated occurs with magnetite at Presnitz. Sp. gr. 2·48. Composition, according to Scheerer and Richter:

	$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	$\dot{M}g$	$\dot{H}$	
1.	58·46	0·09	1·09	——	0·61	32·83	6·56	= 99·64
2.	58·70	0·06	1·01	0·39	0·81	32·07	6·56	= 99·50

for which Naumann gives the formula $\dot{M}g^3\dot{S}i^5+\dot{H}$.

TANTALITE [p. 351, III—VI].—A. E. Nordenskiöld has analyzed tantalite from a new locality at Björtboda in Finland (Pogg. Ann., cvii, 374):

$\dot{T}a$	$\dot{S}n$	$\dot{F}e$	$\dot{M}n$		
83·79	1·78	13·42	1·63	=	100·62

the oxygen ratio between the bases and the metallic acids is 1 : 4·83, most nearly resembling the composition of the Tammela tantalite.

TENNANTITE [p. 84, II].—Vom Rath has published the following analyses of tennantite from Cornwall (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xv, s. lxxii, in Kopp's Jahresbericht f. 1858, 680):

	Density.	S	Cu	Fe	Zn	As	
1*a*.	4·652.	25·22	46·88	6·40	1·33	18·72	= 98·55
1*b*.	——	27·13	44·43	6·88	1·43	20·13	= 100·00
2.	4·69.	26·34	52·97	2·82	——	18·06	= 100·19

No. 1*a* is the direct result of the analysis—the mineral was associated with black oxyd of copper, and assuming the amount of this substance to be seven per cent, and averaging the analysis to one hundred, gives the result as in No. 1*b*. Analysis No. 2 was by Baumert. According to v. Rath, the ratio between the metallic sulphids and the sulphid of arsenic in tennantite is 5 : 4, while the analogous ratio in tetrahedrite is 4 : 3.

TOURMALINE [p. 270, II, IV, VII].—Jenzsch (Pogg. Ann., cviii, 648) has examined a crystal of *tourmaline* from Elba which he considers to be optically bi-axial. He suggests, from his investigations, that although the tourmaline crystals from Elba and Penig (Saxony) approach very nearly the hexagonal form, that they belong either to the trimetric or monoclinic system—a view previously suggested by Breithaupt's measurements. Breithaupt publishes a preliminary notice in the Berg und Hüttenmannische Zeitung, xix, 93, of a forthcoming monograph on this subject.

TRIPHYLINE [p. 406, 513].—F. Oesten obtained from the analysis of a very pure specimen of triphyline from Bodenmais in Bavaria (Pogg. Ann., cvii, 438):

	$\dot{P}$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	$\dot{M}g$	$\dot{L}i$	$\dot{K}$	$\dot{N}a$	$\dot{S}i$	
G.=3·545—3·561.	44·19	38·21	5·63	0·76	2·39	7·69	0·04	0·74	0·40	=100·05

This gives the oxygen ratio between the bases and phosphoric acid 15·34 : 24·77 = 3·09 : 5, and the formula, $\dot{R}^3\dot{P}$, the same as first proposed by Fuchs. Wittstein, in a recent note (Pogg. Ann., cvii, 511), calls attention to the fact that eight years since he published results giving the above formula, and says moreover, that a portion of the iron exists as sesquioxyd. Oesten has since (Pogg. Ann., cviii, 648) published proof that the specimen he examined was entirely unaltered, and that all the iron existed as protoxyd.

TYRITE (?) [I, III, IV].—Potyka (Pogg. Ann., cvii, 590) has analyzed specimens of supposed tyrite from Norway which prove to be a new columbate containing several per cent of potash, and distinct from the tyrite of Forbes. The chemical composition was found to be:

$\dot{C}b$	$\dot{Z}r$	$\dot{W}$	$\dot{S}n$	$\dot{P}b$	$\dot{C}u$	$\dot{Y}$	$\dot{C}e$	$\dot{F}e$	$\dot{U}$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{H}$	
43·49	0·80	1·35	0·09	0·41	0·35	31·90	3·68	1·12	4·12	1·95	*tr.*	7·23	3·71	=100·20

The ratio between the metallic acids and bases, exclusive of the water is, as 1 : 1·04 or $\dot{R}^3\ddot{C}b$. The mineral occurs implanted in red feldspar in small irregular masses having an uneven fracture, but no distinct cleavage. Lustre, sub-metallic; color black, in thin splinters reddish-brown and translucent on the edges; streak reddish-brown; hardness that of apatite (5). Sp. gr. in coarse powder =5·124 (16·6° C.). When hot water is poured upon fragments a crepitation or crackling takes place. B.B. with borax gives a reddish-yellow bead while hot, which on cooling becomes yellow; with salt of phosphorus is completely dissolved to a greenish-yellow bead while hot, becoming green on cooling. No reaction for manganese with soda. Treatment with concentrated sulphuric acid gave no reaction for fluorine. [This mineral corresponds in many of its physical and blowpipe characters with the *bragite* of Forbes (see Suppl. III). Possibly a thorough analysis of authentic specimens of bragite would show them to be very nearly related, if not identical. —G. J. B.]

Uranium, silicates of, see Hermann's paper in Jour. f. prakt. Chem., lxxvi, 320.

Uranoniobite (*Hermann*), see *Pitchblende*.

URANOCHALCITE, *Hermann*.—This name has been given by Hermann to a mineral from Joachimsthal (Jour. f. prakt. Chem., lxxvi, 321). It occurs in reniform amorphous masses having a metallic appearance. Fracture compact, and slightly conchoidal, with a feeble metallic lustre; brittle; opaque; color between steel-gray and pinchbeck-brown; streak black. H.=4. Sp. gr. 5.04. Heated in a closed tube the mineral at first gives off water, and then a sublimate of realgar, and finally metallic arsenic, leaving a black residue consisting chiefly of bismuth, uranium, copper, and iron. Treated with nitric acid the mineral is dissolved with separation of sulphur. On evaporation of the solution, silica separates in the gelatinous form. The analysis gave:

S	As	Cu	Ni	Fe	$\ddot{S}i$	$\ddot{B}i$	$\ddot{C}u$	$\dot{F}e$	$\dot{U}$	Ag	
5·79	7·23	10·21	0·97	2·31	4·40	36·06	14·41	11·95	3·27	2·40	*tr.*=99·00*

Hermann writes the formula $5(\dot{R}^4\ddot{S}i+4\dot{R}^2\ddot{S}i+10\dot{H})+R(AsS)$. [It is quite improbable that this composition is that of a simple mineral, and until further investigation we may reasonably doubt the homogeneousness of the substance analyzed.—G. J. B.]

Vanadinite [p. 362, II—IV].—Kokscharow considers the *vanadinite* crystals from Beresowsk to be pseudomorphs of *pyromorphite*. Struve found in the interior of each vanadinite crystal a portion of unaltered pyromorphite. The mean of two analyses gave:

	PbCl	$\dot{P}b$	$\dot{P}eCr$	$\ddot{V}$	$\dot{P}$
G.=6·863.	9·60	71·13	0·43	15·92	2·92

Struve represents this composition by the formula $PbCl+\dot{P}b(\dot{P}\frac{1}{4}, \ddot{V}2\frac{1}{4})$ or $(3\dot{P}b^3\dot{P}+PbCl)+5(3\dot{P}b^3\ddot{V}+PbCl)$.—(Kokscharow, Mat. Min. Russlands, iii, 44).

Vivianite[p. 415, III, IV].—For an article on the composition and formation of *vivianite* by Alphonse Gages, see L., E. and D. Phil. Mag., [4], xviii, 182.

Water [p. 110].—Analysis of water from the Dead Sea, by Dr. F. A. Genth.—Ann. d. Chem. u. Pharm., cx, 240.

Wolfram [p. 351, I—IV].—F. A. Genth has published (this Jour., [2], xxviii, 253) an analysis of the wolfram which forms the nucleus of the peculiar tungstate of lime crystals alluded to under scheelite. One crystal showed the planes *I*, i-$\bar{i}$, $\frac{1}{2}$-$\bar{i}$, and 1-$\bar{i}$. Sp. gr. 7·496 (at 25° C.). Composition:

$\ddot{W}$	$\ddot{S}n$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	
75·79	*tr.*	19·80	5·35	0·32	= 101·26

corresponding to variety II. (Min., p. 352), having the formula $4\dot{F}e\ddot{W}+\dot{M}n\ddot{W}$.

* The original gives 100, but owing to a typographical, or other error the analysis adds up only 99.

WULFENITE [p. 349, II, V].—The massive wulfenite from Garmisch, is a mixture of molybdate of lead, with carbonate of lead and other substances, as shown by Wittstein's analysis (Kopp's Jahresbericht, 1858, 721):

Ṗb	C̈a	M̈g	F̈e	M̈o	S̈i	P̈	Loss (C̈ & trace V̈)
33·80	10·85	3·57	2·80	20·00	16·20	0·04	12·74

ZINC-BLOOM [p. 460, 513, VII].—Dr. Elderhorst has described a hydrous carbonate of zinc from Marion County, Arkansas, as a new species under the name *marionite* (First Geol. Rep. Arkansas, p. 153). The chemical composition he found:

Ż̇n	C̈	Ḧ		
73·26	15·01	11·81	=	100·08

[This is identical with analyses 1*a*, of zinc-bloom from Santander in Spain, by Peterson and Voit, published in the last supplement. This analysis gave Ż̇n 73·1, C̈ 15·1, Ḧ 11·8. These analysts found that zinc-bloom undergoes a change on exposure to the air, thereby losing both carbonic acid and water. A specimen of 1*a*, exposed to the air for three months was found to contain Ż̇n 74·73, C̈ 13·81, Ḧ 11·45. Other analyses by Braun are quoted in the last supplement. Peterson and Voit (Ann. d. Chem. u. Pharm., cviii, 50) give the formula for zinc-bloom Ż̇n$_5$, C̈$_3$, Ḧ$_8$, which is the same as that given by Dr. Elderhorst for *marionite*;—it is an interesting fact that this is also the composition of the precipitate, produced by adding an equivalent of carbonate of soda to a zinc salt at the boiling temperature. Marionite may be considered as zinc-bloom, and the earlier analyses of this species by Smithson and Berthier, are undoubtedly less correct than those of Karsten and the more recent ones by Peterson, Voit, Braun, and Elderhorst.—G. J. B.]

Terreil mentions the occurrence of zinc-bloom at Santander in oolitic grains (L'Institut, No. 1347).

ART. XXXIV.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

IT is proposed in the present article to investigate the dimensions of the great comet of 1858, at certain specified dates, upon the theory developed in this Journal, (vol. xxvi; No 79), and compare the theoretical determinations with the results of observation. Resuming the equation of the approximate orbit of a particle emitted from the nucleus, obtained in the investigation alluded to, viz.

$$\frac{\sqrt{2pr}}{k\sin a}\left(1-\sqrt{1-\frac{k\sin a}{pr}z}\right)=\sqrt{\frac{2x}{k\cos a}}. \quad . \quad . \quad . \quad (1.)$$

in which the axis of z coincides with the original direction of motion, a denotes the angle of inclination of this initial line of direction to a line perpendicular to the radius-vector, r the radius of the nucleus, p the acceleration due to the repulsive force of the nucleus at its surface, and k the opposite acceleration produced by the sun's repulsion; let us pass to a new system of rectangular axes, x' and z', of which the axis of z' is coincident with the radius-vector of the orbit of the comet. Effecting the transformation of coördinates, reducing, and denot-

ing by H the distance of the vertex of the cometary envelope from the nucleus $\left(=\frac{pr}{k}\right)$, we have

$$x'^2 - 2H\sin 2a \,.\, x' = -2H\sin 2a\cot a \,.\, z'. \quad . \quad . \quad . \quad . \quad (2.)$$

Putting $H\sin 2a = K$,

$$x'^2 - 2Kx' = -2K\cot a \,.\, z'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

Let $z'=0$, and we obtain for the half-breadth of the envelope, $\frac{b}{2}=2K$; and thence, for the coördinates of the vertex of the curve described by the particle, $X'=\frac{b}{4}=K$, and $Z'=h=\frac{K}{2}\,.\,\text{tang}\,a$. Transferring the coördinates to this point, we get for the equation of the curve, referred to its vertex,

$$x''^2 = 2K\cot a \,.\, z''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

This is the equation of a parabola, of which the parameter, $2p_1 = 2K\cot a = 4h\cot^2 a$; and the distance from the focus to the vertex $= \frac{K}{2}\cot a = h\cot^2 a$.

It is also the equation of the curve that would be described by a particle if it were projected from the nucleus with a certain velocity, and subsequently repelled by the sun alone. From which it appears that the path pursued by a particle repelled from the nucleus is very nearly the same, and, for the purposes of the present investigation, may be regarded as the same, as that which would be followed if the particle were simply projected from the nucleus. If we had occasion to trace accurately the trajectory of the particle in the vicinity of the nucleus, another investigation would become necessary. It should also be observed, that in the case of any particle, which, on its return from its excursion toward the sun, comes into proximity to the nucleus, the parabolic projectory becomes materially modified by its repulsive action, and equations (3 and (4) are inapplicable.

We may conclude from the result just obtained that, so far as the form and dimensions of the nebulous envelope are concerned, the theory of a repulsion exerted by the mass of the nucleus does not differ materially from that of the projection of the cometary matter by an instantaneous force from its surface; which, it appears, has been advocated and discussed by Bessel.

Other determinations relative to the envelope of the comet may be effected by the following formulas; in which $Z=$ the greatest distance attained by a particle, in the initial direction of motion; $Y=$ the actual distance from the nucleus, of the particle when in this extreme position; $\varphi=$ the angle included between Z and Y; $\beta=$ the inclination of the tangent drawn to

any point of the curve followed by the particle, to the radius-vector of the orbit of the comet; $v =$ the velocity of the particle at the vertex of its parabolic path; and $v' =$ its velocity at any other point of the curve;

$$H=\frac{pr}{k}. \qquad Z=\frac{H}{\sin a}. \qquad Y=\frac{H}{\sin^2 a}. \quad . \quad . \quad . \quad . \quad (5.)$$

$$\varrho=\text{co.}\,a. \qquad \text{tang}\,\beta=\sqrt{\frac{H\sin 2a\cot a}{2z''}}=\sqrt{\frac{p_1}{2z''}}. \quad . \quad . \quad . \quad (6.)$$

$$v=\sqrt{kp_1}. \quad . \quad . \quad (7.) \qquad v'=\frac{v}{\sin\beta}. \quad . \quad . \quad . \quad . \quad (8.)$$

$$v'=\sqrt{k(p_1+2z'')}=\sqrt{k\left(p_1+\frac{x''^2}{p_1}\right)}. \quad . \quad . \quad . \quad (9.)$$

More accurately, we may obtain the velocity v'' at right angles to the radius-vector, for any point of the actual curve, from the following equation:

$$v''=\cos a\sqrt{2pr\left(1-\frac{r\cos a}{x'}\right)}. \quad . \quad . \quad . \quad . \quad . \quad (9^a.)$$

in which $x' =$ distance of the point from the radius-vector. The distance from the nucleus to any point of the trajectory of the particle, whose coördinates are known, may be readily obtained from the polar equation of the curve.

Equs. (1) to (9^a) have been obtained on the supposition that the nucleus is at rest; or, in other words, they refer to the relative motion of the cometary particle and nucleus, on the supposition that the two have the same velocity, and a constant direction of motion through space. Strictly speaking, there is not a perfect accordance between the two motions, even during the short interval of time that the particle remains within the limits of the envelope; but no material modifications of the theoretical results are required on this account, in investigating the form and dimensions of the envelope. But when we undertake to follow the cometary particle, after it has left the region of the envelope, and is receding from both the nucleus and the sun, under the influence of the solar repulsion, it will no longer answer to neglect the orbitual motion of the nucleus.

The general problem, to find the relative positions of a repelled cometary particle, and the nucleus of a comet, after any interval of time, appears to have been first effectually solved by Bessel. This important problem has recently been taken up independently, and solved anew by Prof. Peirce; who has shown that the orbit of the repelled particle is a hyperbola convex towards the sun, and has verified the supposed law of variation of the sun's force of repulsion. In pursuing the line of investiga-

tion in hand, we are led to take a point of view somewhat different from that occupied by either of these eminent astronomers. It is now proposed to determine both the true and apparent positions of the receding particle, after the lapse of any interval of time, directly from the initial velocity and direction of motion; in order to take account of the various circumstances of the original motion of the different particles supposed to proceed from the nucleus. The following formulas will serve for this purpose. Equs. (12) to (15) have been deduced from the general equations of motion of a body around a centre of attraction, by changing the sign of the force, and adapting them to convenient computation. Equ. (17) for calculating the true anomaly of the particle in its hyperbolic orbit, from the time, was independently investigated. It is sufficiently accurate for our purpose, and the calculation can be more readily effected with it than by the intervention of the eccentric anomaly. The constants which enter into the equation can be determined by very simple formulas, for any comet the elements of whose orbit are known, and for any position of the comet in its orbit; their values having been determined by other means for the perihelion of any one comet. They depend upon the initial circumstances of motion of the particle emitted from the nucleus. Equ. (16) was deduced from equ. (17).

If any particle, on leaving the sphere of influence of the nucleus, is subject to a diminished attraction from the sun, it will describe a hyperbola concave toward the centre of attraction, and will recede from the nucleus, though less rapidly than if it were effectively repelled by the sun. Equs. (22) to (25) serve for this case. There will be occasion to make use of them when we shall undertake to determine all possible particles that at any assumed date may go to make up the concave outline of the tail.

New Haven, March 28th, 1860.

(*To be continued.*)

Art. XXXV.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th*, 1859.—4th Article.

In the three preceding numbers of this Journal we have given observations of the Aurora of Aug. 28th to Sept. 4th, from almost every part of North America between the parallels of 13° and 48° north latitude. We now present a summary of observations of the same aurora in Europe, with some reports from Asia, and accounts of a simultaneous auroral exhibition in the southern hemisphere.

1. *Observations at Christiania, Norway,* (lat. 59° 54′), *by* Prof. CHRISTOPH HANSTEEN.

1859, Aug. 28th. At 10 P. M. only an indistinct coruscation behind the clouds in the north.

Aug. 29th, $12^h\ 10^m$ A. M. perfectly bright, almost as at full moon; the air dim with cirro-stratus, nevertheless the aurora shone through everywhere with strong radiating and flaming motion, very irregularly and unsteady. Corona was often formed; best formed at $12^h\ 17{\cdot}5^m$. Altitude 71° 37′ from south, azimuth 9° 57′ east. At $12^h\ 18{\cdot}5^m$ a purple-colored beam shot in east to γ Andromedæ. At $12^h\ 21{\cdot}5^m$ altitude of corona 72° 27′; azimuth 14° 55′ east. At no time were there regular bows. There was always a vacant space over the south horizon, but often of a suspicious character. It continued after 1 A. M. without essential variation in strength or character.

Aug. 29th, evening, rain—heavens covered.

Aug. 30th, " " " "

Aug. 31st, $11\frac{1}{2}$ P. M., lightning and thunder in southwest.

Sept. 1st, heavy rain—thunder.

Sept. 2d, radiating and strong flaming aurora, $12\frac{1}{2}^h$.

Sept. 3d, radiating aurora over the whole northern heavens to a little south of zenith; rather dimly. It continued to illuminate the heaven after it was almost covered. At $1\frac{3}{4}^h$ A. M. very clear behind the skies everywhere.

Sept. 4th, 10 P. M., radiating aurora in the north to 30° altitude. Later in the night, vehemently flaming with broad flames.

Sept. 5th, 10 P. M., elegant radiating aurora which dilated from the whole northern horizon to south of zenith, mostly behind a veil of cirro-stratus. At an altitude of 45° it was partly flaming. At 12^h it had nearly ceased.

Sept. 6th, at 10 P. M., an arc from 6° to 8° broad, the lower edge of which had an altitude of 5°.

The following table shows the state of the Bifilar magnetometer between Aug. 28th and Sept. 6th:—

1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.
	h m			h m			h m	
Aug. 28	9 23 A. M.	704·09	Aug. 30	9 15 A. M.	670·10	Sept. 2	4 27 P. M.	1438·91
" "	2 10 P. M.	764·98	" "	1 55 P. M.	751·18	" "	5 30 "	1104·69
" 29	9 28 A. M.	243 0	" "	5 30 "	732·08	" "	6 30 "	1020·41
" "	9 43 "	349·3	" 31	9 16 A. M.	667·72	" 3	9 21 A. M.	650·12
" "	10 9 "	638·78	" "	1 55 P. M.	737·95	" "	1 58 P. M.	926·13
" "	10 15 "	531·81	Sept. 1	9 21 A. M.	678·02	" 4	9 19 A. M.	670·05
" "	10 27 "	709·11	" "	2 5 P. M.	711·26	" "	4 28 P. M.	1034·2
" "	10 30 "	670·18	" 2	9 21 A. M.	609·70	" "	4 45 "	959
" "	10 33 "	723·43	" "	9 49 "	780·07	" 5	9 14 A. M.	705·15
" "	1 55 P. M.	941·50	" "	11 29 "	616·87	" "	9 15 P. M.	1060·49
" "	5 35 "	801·42	" "	2 25 "	1381·77	" 6	9 11 A. M.	641·85

The greatest difference observed during this period was 1195·91 parts of the scale. One division of the scale corresponds to $\frac{1}{1570}$ of the horizontal intensity. Hence the variation of the horizontal intensity from Aug. 29 to Sept. 2 amounted to nearly $\frac{1}{13}$ of its whole value.

The inclination of the magnetic needle was observed as follows:

Aug. 29th,	10h	21m	A. M.	71°	31′·5	Sept. 2d,	10h 23m	A. M. 71° 29′·0
	5	23	P. M.	71	19·8		4 16	P. M. 70 26·9
							6 26	P. M. 71 5·8

The mean inclination of the needle in 1859 was 71° 18′.

The effect of this aurora upon the telegraph lines in Norway was much greater than in France and Germany. The effect was noticed from the opening of the stations at 7 A. M. On the 29th communication was interrupted till 11 A. M. on almost all the lines; and likewise Sept. 2d, but with a long repetition after 2 P. M. Sept. 3d, only towards 8½ A. M. During the remaining parts of those days, the perturbations were more or less uninterrupted, nevertheless communication could be maintained in some degree. Strong currents caused simultaneous attractions of all the armatures. The galvanometer showed strong deviations, sometimes with slow, sometimes with sudden movements, from one side to the opposite.

The intensity of the currents was greatest upon the longest lines going towards the north, on which sparks and uninterrupted discharges were from time to time observed. Pieces of paper were set on fire by the sparks of these discharges. In Bergen, where the line to Stavanger runs in a north and south direction, the current was at times so strong, especially Sept. 2d and 3d, that it was necessary to connect the lines with the earth in order to save the apparatus from destruction. The phenomena appeared less strong in Christiansand, in the southern part of Norway, where the lines run east and west.

2. *Observations made in different parts of England; extracted from the London Times.*

A. Durham (lat. 54° 46′).

Sept. 1, aurora; Sept. 2, vivid white aurora; Sept. 3, aurora. Sept. 4, faint aurora.

B. Preston (lat. 53° 45′), by R. C.

Sept. 2d, there was a brilliant auroral display, continuing from 11 to 12 o'clock, and a second appearance, though not so brilliant, at a little before 2 o'clock on the morning of Sept. 3d. During the first display, the whole of the northern hemisphere was as light as though the sun had set an hour before, and luminous waves rolled up in quick succession as far as the zenith, some of a brilliancy sufficient to cast a perceptible shadow on

the ground. To the northwest there was a large patch of light of a deep crimson hue, while the waves of light were white, as also were the streamers which occasionally shot across the northern part of the sky. It was the most brilliant aurora that has been witnessed here for many years.

C. Nottingham (lat. 52° 57′), by E. J. Lowe.

On the evening of Aug. 28th and morning of Aug. 29th there was an unusually brilliant auroral display. From $8^h\ 40^m$ P. M. until 9 P. M. Aug. 28th, curtains of red light were visible near the zenith. By $11^h\ 40^m$ P. M. the glare of orange light in the north was powerful enough (even through much cloud) to make the hands of a watch visible. At $12^h\ 25^m$ A. M. the light was so strong that it gave the impression of daylight. At $12^h\ 45^m$ an opening in the clouds near the zenith disclosed the cupola which was situated exactly on Alpha Andromedæ. At $1^h\ 15^m$ A. M. magnificent rays of light met two degrees east of Alpha Andromedæ. At this time three-fourths of the sky was covered with aurora. At $2^h\ 30^m$, there being more clear sky, a splendid mass of aurora was visible, forming an ever changing cupola close to Gamma Trianguli. All the coruscations moved slowly eastward. At $3^h\ 15^m$ the cupola was formed near Gamma Andromedæ.

Sept. 3, strong aurora near the horizon.

Sept. 4, aurora.

D. Grantham (lat. 52° 55′).

Aurora Aug. 28th, 29th, 30th, 31st, and Sept. 3d.

E. London (lat. 51° 37′).

Aug. 28th, at $11^h\ 30^m$ P. M., auroral light in the north. At $0^h\ 15^m$ A. M. Aug. 29th it assumed the form of a luminous arch, similar to daybreak, and in the southwest there was an intense glare of red covering a very large extent; at $0^h\ 20^m$ streamers; at $0^h\ 25^m$ the streamers rose to the zenith and were tinged with crimson at their summits; at $0^h\ 45^m$ frequent coruscations; at $1^h\ 0^m$ the arch which had partially faded was re-formed, the body of light being very strong, but not sufficient to enable one to read any but very large print; at $1^h\ 30^m$ light equally strong, but outline indistinct; at 2 A. M. much less light and very indistinct. Continued till $2^h\ 30^m$ A. M.

Sept. 3, aurora.

F. Clifton (lat. 51° 27′), by William C. Burder.

Aug. 28th, about $10^h\ 45^m$ P. M., commenced a brilliant auroral display. At first there were several fine streamers, some of them white, and some faint crimson, extending from near the horizon almost vertically to α and β Ursæ Majoris. From that time till midnight there were generally very beautiful streamers, but without lateral motion, most of them being not quite vertical, but inclining slightly towards the east at the top. There was

also always a general light, extending at midnight from northeast to west, and sometimes bright enough to enable a person to read the time on the face of an ordinary watch.

The aurora was repeated Sept. 1st, Sept. 2d, and Sept. 3d.

G. Aldershot (lat. 51° 15′).

Magnificent display of aurora Aug. 28th, and till early morning Aug. 29.

H. Brighton (lat. 50° 50′).

Aug. 29th, about half past one o'clock, a fine aurora occupied more than one-half the sky. It had the appearance of an irregular hemisphere of white light fringed with a band of crimson from twenty to thirty degrees broad, stretching from southwest to northeast by east.

3. *Observations at St. Valery, France*, (lat. 50° 10′ N., long. 1° 37′ E.), *by* H. LARTIGUE, *from Comptes Rendus*, T. XLIX, p. 367.

Near St. Valery a white light of considerable intensity was noticed in the north at $11^h\ 40^m$ P. M. Aug. 28th. A red column, with sides nearly parallel, and 4° or 5° in breadth, rose from the N.N.W. nearly to the zenith, but disappeared after a few minutes. About $12^h\ 10^m$ the white light near the horizon had increased in intensity; a large part of the heavens was colored red, and the exhibition attained its greatest brilliancy at $12^h\ 20^m$. Magnificent columns and brilliant rays, changing from red to green and white, rose to the zenith, sometimes passed beyond it, and occupied the entire space between Aquila and the meridian, and a few minutes later extended to the constellation Auriga. The light was bright enough to allow objects to be seen at a distance of one mile, as during a clear night with a full moon. The illumined portion of the sky increased till $12^h\ 40^m$. After this time the brightness diminished near the meridian, but the east and west portions continued red. At $1^h\ 15^m$ the vertical columns again appeared very brilliant, and nearly as extensive as at $12^h\ 40^m$, but they soon disappeared. The red light grew fainter, and disappeared entirely at 2^h. The white light which marked the commencement of the phenomenon continued three-quarters of an hour longer.

4. *Observations at Paris, France*, (lat. 48° 50′), *by* M. COULVIER GRAVIER, *from Comptes Rendus*, T. XLIX, p. 338.

The aurora was first noticed at Paris at 2^h on the morning of August 29th, and it soon rose to a great height above the horizon. About $2^h\ 45^m$ the vertex of the grand arch had reached the trapezium in Cetus, being 150° from the northern horizon, and it extended from Monoceros to 10° south of θ Aquilæ; having an amplitude of more than 200°. The vertex of the small arch rose to η Draconis, being a height of 26°; and it

extended from Cerberus to Leo Minor, having an amplitude of more than 100°. The exhibition continued until the morning twilight. A motion of translation from W.SW to E.SE. was suspected, but the motion was not very appreciable. When the aurora appeared in its greatest brilliancy, the substance which composed it appeared to be in a state of great agitation; and the rays exhibited a red color, sometimes like that of iron heated to redness and sometimes to a white heat. The space occupied by the small arch was, as usual, of a greenish color; the centre near the horizon being black, and the whole destitute of rays. The aurora exhibited the greatest brilliancy between the W. and N.E. points of the horizon. A few cirrus clouds were noticed during the exhibition; they were all black, without any reflection of the light of the aurora, proving that this light emanated from a region much above that of the clouds.

Magnetic effects of the Aurora; from the Comptes Rendus, T. XLIX, p. 473.

On the 26th of August some anomalies were noticed in the motions of the magnetic instruments at the Observatory of Paris, the declination having changed 22′ between 9½ A. M. and noon.

Aug. 28th at 5 P. M. the motion of all the magnetic instruments was very irregular. Between midnight and 1 A. M. of Aug. 29th the horizontal intensity varied 0·0074. At 9 A. M. of the 29th the horizontal intensity had diminished by 0·01, while the vertical component had increased 0·0013.

During the forenoon of the 29th the declinometer was very much disturbed, and at 11 A. M. it oscillated 41′ on each side of its mean position. Towards evening the disturbances disappeared; but a fresh disturbance commenced on the 1st of September, at $11^h\ 30^m$ A. M. About 4 P. M. Sept. 2d, there commenced a new magnetic storm, more violent than that of Aug. 29th. The magnets were carried beyond the range of their scales, showing a change of the horizontal intensity exceeding 0·014, but as the observations were only recorded photographically, the extreme range could not be determined.

Effect on the Telegraph Wires, from the Comptes Rendus, T. XLIX, p. 365.

From the evening of Aug. 28th until the morning of the 29th the needles of the magnetic telegraph at Paris were almost constantly in motion, as if a permanent current was passing through the telegraph wires. Business was therefore entirely interrupted, and could not be resumed until 11 A. M. Aug. 29th. The same effect was noticed on the telegraph lines from 4^h to 8^h on the morning of Sept. 2d, although no aurora was noticed on that day. Business was again interrupted, the needles were disturbed, and the bells were rung.

The galvanometers were violently deflected, sometimes to the right and sometimes to the left. The needles were turned from zero 10° or 20°, remained there stationary for a short time, then suddenly moved to 30° or 50°, then returned and were deflected in like manner on the other side of the zero point. The effect was more powerful and longer continued on the lines from Paris to Bordeaux, Marseilles and northward, than it was on the east and west lines. During the night of Aug. 29th some intelligible signals were received from Strasbourg.

During the day, Aug. 30th, the telegraph operators experienced frequent interruptions. On the afternoon of Sept. 1st some difficulty was experienced in telegraphing; but Sept. 2nd, at 4^h 50^m A. M., there was a general disturbance on all the lines, first on those to Bordeaux, Toulouse, Marseilles, London and Brussels, and a few minutes later on those to Basle, Strasbourg, Havre and Brest. At 7 A. M. *bright sparks* were noticed on the conductors of the lines to Bordeaux and Toulouse. The line to Strasbourg was less affected than the others. About 3 P. M. telegraphic communication was resumed on all the lines; but during the evening and the next morning it frequently happened that the communication was difficult.

Observations of Ozone.

Regular observations are made at Versailles on the amount of ozone in the atmosphere. During the auroras of Aug. 29 and Sept. 2, the quantity of ozone was decidedly greater than usual. The following table shows the sums of ozone collected during each period of six days, from Aug. 4, to Sept. 8, 1859:—

	Morning.	Evening.
From Aug. 4 to Aug. 10	64·0	55·6
Aug. 10 to Aug. 16	87·0	59·0
Aug. 16 to Aug. 22	82·0	60·0
Aug. 22 to Aug. 28	65·0	55·0
Aug. 28 to Sept. 2	97·0	64·0
Sept. 2 to Sept. 8	81·0	58·0

5. *Observations at Brussels*, (*lat.* 50° 51′), by M. QUETELET, from L'Institut of Feb. 1, 1860.

At 12^h 35^m A. M. Aug. 29th, the sky was overcast with a light and uniform veil, with the exception of the northern horizon, which presented a slight appearance of twilight. Soon there appeared in the N.W. a rosy light, which, in a few seconds, assumed enormous dimensions. It rose to an altitude of 60°, and illumined all that portion of the sky. The rosy light rapidly extended, and soon changed to purple, presenting the appearance of a vast conflagration. There was a constant oscillatory movement, and the light varied from a bright yellow to

the deepest red. Near the horizon the sky presented a greyish and dirty appearance. There were faint traces of an obscure segment, whose centre was on the magnetic meridian. Bright rays of a yellowish white shot up from this part of the horizon, traversed the rosy light in the N. W., and terminated in a bundle at a distance of 90° from their origin.

About $12^h\ 45^m$ A. M. the twilight which illumined all the northern region became more intense; the general tint continued of a yellowish white, but on the eastern and western borders passed into a yellowish green. Then there appeared on the N. N. E. a second rosy light, but less decided than that of the N. W. This was also traversed by yellow rays; but the latter were much more brilliant and broader than those which traversed the light in the N. W. Those rays also terminated in a bundle at a distance of 43° from their origin.

Subsequently the aurora presented frequent alternations of brightness, but the general appearance continued the same until 2 o'clock, when observations were suspended.

At 9 A. M. Aug. 29th a disturbance of the magnetic instruments was noticed at the Observatory. The following table shows the extreme indications of the instruments for each hour, from 9 A. M. to 9 P. M. of Aug. 29th. Between 9^h and 10^h A. M. the fluctuations of the horizontal intensity were too great to be observed by the fixed telescope.

Hour.	Declination. Max.	Min.	Horizontal Inten. Max.	Min	Hour.	Declination. Max.	Min.	Horizontal Inten. Max.	Min.
h. h.	d.	d.	d.	d.	h. h.	d.	d.	d.	d.
9 to 10	50·12	58·63	?	?	3 to 4	53·54	57·55	9·63	4·93
10 to 11	49·33	53·60	1.07	−2·84	4h 30m	57·96	"	8·83	4·60
11 to 12	51·77	55·89	1.89	−0·85	"	57·02	"	5·54	
12 to 1	51·68	53·58	6·50	2·53	6h 30m	56·60	"	3·78	
1 to 2	52·68	53·32	6·47	5·00	8h	55·43	"	6.12	
2 to 3	53·13	53·85	6·04	5·40	9h	55·73	"	5.85	

About midnight Aug. 28th—29th, the employés in the telegraph office at Brussels noticed signals from their bells, such as often occur during a storm. The employés in the offices at Mons, Antwerp, Gand and Ostend were also awakened by their bells, and enquired what was wanted. Communication with Paris, London, and Berlin were interrupted till $1^h\ 30^m$. Paris and London inquired of our operators if they saw a light in the heavens. The effect ceased at $1^h\ 30^m$ on all the lines except the submarine line from Ostend to Dover, which was charged with electricity throughout the entire morning. It was not till $3^h\ 30^m$, and after nearly doubling the battery, that communication was re-established.

September 2, between 5^h and 6^h A. M., there was a second disturbance on all the telegraph lines, and communication between

Brussels, Paris, and London was interrupted. The following observations were made at the Observatory of Brussels:—

Date.	Declination Max.	Declination Min.	Hor. Inten. Max.	Hor. Inten. Min.
	d.		d.	
Sept 2, 9 to 10 A. M.	54·75	57·65	10·43	6.25
10 to 11 "	53·62	59·40	9·82	3·36
11 to 12 "	53·72	58·15	0·30	4·64
12 to 1 P. M.	52·06	66·24	8·05	7·36
1 to 2 "	49·34	62·81	?	?
2 to 3 "	43·40	57·87	17·79	0·00
3 to 4 "	48·32	58·40	15·53	10·12
4 to 5 "	51·15	55·00	14·01	10·48
5 0m	54·75		10·24	
6 0	54·44		8·87	
7 5	56·67		7·80	
8 24	56·59		7·21	

Date.	Declination. Max.	Declination. Min.	Hor. Inten. Max.	Hor. Inten. Min.
Sept. 2, 9h 0m P.M.	60·62		5.64	
10 0	53·47		8·04	
Sept. 3, 9 to 10 A. M.	57.17	58·23	4·89	4·09
10 to 11 "	54·56	56·53	5·21	4·46
11 to 12 "	53·73	54 96	7·08	5·55
12 to 1 P. M.	50·59	53·63	10·64	7·40
1 to 2 "	51·03	53·15	11·28	8·88
2 to 3 "	51·63	51·90	10·42	8·75
3 to 4 "	48·37	51·52	13·75	8·36
4 30m	51·52		12·50	
5 0	53·50		14·83	
9 0	57·58		7·31	

6. *Effects of the Aurora upon the Telegraph Lines of Wurtemburg; from Poggendorff's Annalen*, Band 108, p. 506.

During the night of Aug. 28th, from 11h 15m P. M. to near noon of the 29th, there was remarked from time to time on all the telegraph lines proceeding from Stuttgard, an extraordinary attraction of the armatures, which continued from 20 to 40 minutes, and generally appeared first on the line to Heilbronn, after about 5 minutes on the Ulm line, next on the line to Carlsruhe, and last on the Tubingen line. This attraction was repeated every 5 or 10 minutes, and, towards morning, every 2 or 3 minutes. After 5 o'clock only bell signals could be obtained from the local stations, as the armatures were held fast. During this period the deflections of the galvanometers were very remarkable. In a single minute the needles changed their position 5 or 6 times even to 40° west. While on the Ulm line the deviation was easterly, on the Bruchsal line the deviation was westerly.

The cause of this phenomenon is found in a brilliant aurora which was everywhere observed from 9 P. M. Aug. 28th till towards morning of the 29th.

7. *Effects of the Aurora upon the Telegraph Lines of Prussia; from Poggendorff's Annalen*, Band 108, p. 504.

The electrical currents on the conducting wires exhibited themselves in violent deflections of the galvanometers. The needles swung violently from 30° to 70° to one side, returned slowly to zero, and then moved slowly to the other side. On the line proceeding from Berlin westward, the disturbance commenced between 1 and 2 o'clock on the morning of Aug. 29th, when all connection with the stations ceased. Notice had previously been received of disturbance at the easterly stations, Konigsberg, Kowno, Riga and Petersburg. During the day of

the 29th, on the western lines, communication was uninterrupted, while on the eastern lines it was occasionally suspended.

On the 2nd of September, when at 7 A. M. almost all the lines were in use, the disturbance occurred on all the lines, and interrupted communication from 5 to 40 minutes. The interruption was first experienced at Konigsberg 5 A. M.; at Stettin 5^h 55^m; Coblentz and Cologne 6^h 45^m; Berlin 6^h 50^m; Kowno and Riga at 7 A. M. About 9 A. M. the disturbance was greatest, and it declined till 9^h 45^m, when communication was resumed with most of the stations. At Stettin communication on all the lines was resumed at 9^h 24^m, and at Cologne at 10^h. At Konigsberg the disturbance still continued, and at Berlin it increased to 1 o'clock, so that all communication was suspended with the west. In the course of the day, news was received of disturbance at Hamburg, Breslau, Brussels, Paris, and Amsterdam. From the latter station came the intelligence that the submarine line to England was also interrupted by the aurora.

8. *Auroral Observations in Austria; communicated by* Prof. W. HAIDINGER, Vienna, to Prof. SILLIMAN.

The Aurora of Aug. 28th to Sept. 4th, was seen at the following places in the Austrian empire:

	Latitude.	Long.	
Bodenbach, Bohemia,	50° 47′	14° 10′	Aug. 29th, from 2 to 3 A. M.
Schossl, do.	50 27	13 30	From $11\frac{3}{4}$ Aug. 28th to $2\frac{1}{2}$ A. M. Aug. 29th.
Prague, do.	50 5	14 25	Aug. 29th, morning. Also faint in the night from Sept. 1st to 2nd.
Rzeszow, Galicia,	50 3	22 4	Sept. 2nd and 3rd, evening.
Biala, do.	49 50	19 5	Sept. 3, 8 o'clock.
Iristen, Austria,			Aug. 28, from midnight onwards.
Lintz, do.	48 17	14 15	September 3, 8 P. M.
Vienna, do.	48 13	16 23	Great magnetic disturbances were noticed from noon Aug. 28th to the evening of Aug. 29th. Also from Sept. 2 early in the morning to Sept. 3, morning; and from Sept. 4, morning, to Sept. 5th in the evening.
Kremsmünster, do.	48 3	14 7	Aurora, Sept. 3rd, from $8\frac{1}{4}$ to $8\frac{1}{2}$ P. M.
Schemnitz, Hungary.	48 27	18 50	From Aug. 28th, 10 P. M. to Aug 29th, 3 A. M. Also Sept. 3rd, 9 P. M.
Neutra, do.	48 17	18 50	Sept. 3, 9 P. M.
Mitterdorf, Styria,			Sept. 2, evening.
Laibach, Carniola	46 3	14 30	Sept. 3, after 8 P. M.

9. *Effects of the Aurora upon the Telegraph Lines of Switzerland; from the Comptes Rendus,* T. XLIX, p. 662.

The intensity and direction of the currents excited in the telegraph wires, during the aurora of September 2d, were determined by M. Hipp, at Berne, by the deviation of a magnetic needle, surrounded by a wire, making thirty coils. The regular current employed in telegraphing should have a sufficient force to deflect this needle 30°. M. Hipp found that the short lines gave no indication of a current, while the most marked effects were indicated by the longest lines, and especially by those which were directed from north to south, as the line from Zurich to Berne, Fribourg and Lausanne. The current on this line, directed from Zurich to Lausanne, would increase gradually, until the needle was deflected 42°. It would then slowly decline, and at the end of two or three minutes become zero. It would then change its direction, returning from Lausanne to Zurich, and attain a maximum of 30 degrees. The latter current, after continuing 60 or 90 seconds, became zero, and again changed its direction.

It appears from these observations that two currents succeeded each other on the telegraph wires, having a general direction from north to south, the one proceeding from north to south having a double intensity and a double duration, the other proceeding from south to north having a less intensity and a less duration.

M. Hipp obtained deviations of 58 degrees between Zurich and Berne, and of 64 degrees between Berne and Basle, indicating currents at least threefold the ordinary current employed in telegraphing.

10. *Effects of the Aurora of Aug. 28th and 29th upon the Telegraph Lines of Tuscany, by* M. CH. MATTEUCCI; *from the Annales de Chimie et de Phys.,* Tom. LVII, p. 419.

About 6 A. M., Aug. 29th, the disturbance became sensible on the telegraph lines. About 10 A. M. a current, which marked 25 degrees on the galvanometer, and equal to about 30 feeble elements of Daniell, traversed the upper wire of our telegraph lines, in the direction from Pisa to Florence. The current slowly increased, attained its maximum in about five minutes, and then rapidly declined. These periods were renewed a great number of times, and, during the intervals, telegraphic communication was held in the usual manner. About 3 P. M. the auroral effect upon our telegraph lines had ceased.

During the disturbance, on all the lines where there are several wires stretched one above the other in the same vertical plane, the strongest current was uniformly observed in the upper wire, while in the wire nearest the earth, the current was either feeble

or inappreciable. This extraordinary current was the most intense on the longest wires.

11. *Observations at Rome, Italy, (lat. 41° 54′), by* M. Secchi; *from the Comptes Rendus*, T. xlix, p. 347 and 458.

On the 29th of August we had a superb aurora. The sky at Rome was covered with a red veil, and was crossed by the most brilliant rays, in the form of luminous columns. The magnetic instruments were very much disturbed. The declinometer deviated 34′ from its normal position, and the inclination varied 42′. The instruments for measuring the horizontal and vertical force both passed beyond the range of their scales, showing that the variation of the horizontal force must have been at least 0·0135, and of the vertical force at least 0·0075. The disturbance continued for a long time during the forenoon, and the vertical magnet, which, before noon, was beyond the scale, in consequence of the elevation of its north pole, at one o'clock passed beyond the scale on the opposite side, from a depression of its north pole.

A still more remarkable disturbance of the magnetic instruments occurred on the 1st and 2nd of September. At 4 P. M. Sept. 1st, the vertical magnetometer passed beyond its scale, showing a diminution of vertical force.

Sept. 2, at 7 A. M., the magnets were very much disturbed. At $7^h \cdot 10^m$ the declinometer pointed 2° 50′ to the west of its ordinary position. After this the needle returned rapidly to the east, and at 7^h 30^m pointed 1° 23′ east of its mean position, thus describing an arc of 4° 13′ in less than half an hour. This disturbance is the more remarkable, as the greatest range heretofore observed at Rome was only 45′ or 50′.

The bifilar indicated a diminution of the horizontal component, amounting to 0·129, or about *one-eighth* of its mean value.

These disturbances continued with variable intensity all day. At 4^h 15^m P. M. the vertical magnet again passed beyond the range of its scale. At 9 P. M. the magnet was more tranquil, and at midnight they had all returned nearly to their normal condition.

The variations of the declinometer, the bifilar and the vertical magnetometer were not simultaneous, but their maxima occurred at different times. The great vibrations were cotemporaneous with the currents observed on the telegraph lines. The clouds observed in the heavens had the exact appearance of those of the aurora borealis when it occurs by day, and such as were noticed at Rome Aug. 29th.

Similar observations were made at Leghorn, where at 6^h 30^m A. M. Sept. 2nd the declination was 15° 10′, while at 6^h 30^m P. M. it was only 14° 18′. The inclination of the magnetic needle was also very much increased during the day.

12. *Observations from Western Asia.*

A. Yozgat (lat. 39° 45′), by FAYETTE JEWETT, M.D., American Missionary.

The auroral phenomena referred to in your circular were *not* observed at Yozgat. On the 28th of August, and for several days before and after that date, I was in Arabkir, a town nearly 300 miles almost east of Yozgat. The aurora was not noticed there. While I was at Arabkir, owing in part to the mildness of the temperature, and also to the peculiar clearness of the atmosphere, my attention was almost every evening directed to the study of the constellations. The natives, too, at that season, slept upon the roofs of their houses.

B. Kharpoot, (lat. 38° 40′), by Rev. C. H. WHEELER, American Missionary.

Aug. 28th and the following nights nothing unusual was seen here by me or by others of whom I have made inquiries. It is also a fact, so far as I know, that the usual displays of the aurora are less brilliant here than in New England.

C. Mosul, (lat. 36° 22′), by H. B. HASKELL, M. D. Missionary Physician.

No unusual appearance was observed Aug. 28th, 1859, either here, at Mardin, or Diarbekir. During the residence of American missionaries in Mosul (ten years) no auroral phenomena have been noticed.

13. *Observations in the Southern Hemisphere.*

Ship Southern Cross, (near lat. 50° S., long. 80 W.), from the Alta California.

On the night of Sept. 2d, during a tremendous gale, the rare spectacle of an aurora australis was witnessed. It commenced about half-past one o'clock in the morning, and increased in splendor until towards daylight, when it gradually faded before the light of day. The whole heavens were of a deep red, which color was reflected from the ocean. During the night a tremendous squall with hail burst upon the ship. Through the whole of this the flames assumed the same roseate hue; and when a spray flew over the ship, it fell to the leeward in ruddy showers. Between the squalls, in the clear places in the sky, the mysterious lights were seen shooting up in spiral streaks nearly to the zenith now flashing out with meteoric brilliancy, and now looming up against the horizon, as with the blaze of some terrible conflagration. During the gale, several times at night, brilliant balls of fire appeared flickering at the mast-heads, yard-arms, and other salient points. The captain and his officers say that they have never witnessed anything equaling this display for magnificence.

14. *Observations at Concepcion, Chili, (lat. 36° 46′), from the Mercurio of Valparaiso.*

An aurora was visible here on the nights of Sept. 1st and 2nd. It appeared at midnight in the south part of the horizon, and

was visible until two o'clock in the morning. It had a movement of translation from east to west. In appearance it resembled a cloud of fire, or a large ignis fatuus, which threw out some flame or vapor, and spread a light like that of the moon. For more than an hour the city was brilliantly illuminated by this heavenly light.

15. *Observations at Santiago de Chili, (lat. 33° 28′), from the Mercurio of Valparaiso.*

On the morning of Sept. 2nd, about two o'clock, the sky to the south of Santiago was brilliantly illuminated by a light, composed of blue, red, and yellow colors, which remained visible for about three hours. This phenomenon is very rare in Chili. The aurora was also seen in Valparaiso (lat. 33° 6′).

15. *Observations at Kapunda, South Australia, (lat. about* 35°), *by* J. B. AUSTIN; *from the London Times of Nov.* 14, 1859.

On Monday evening, Aug. 29th, just after dark, the aurora appeared like a large and brilliant pink cloud, extending about 25° or 30° above the horizon, and 60° or 70° in length. It continued visible for about twenty minutes, during the last five of which, splendid streamers of pink and white light were shooting vertically through it. It was seen almost throughout these colonies at the same time, and on four nights in the same week; but I saw it only twice, once Aug. 29th, and again on Friday, Sept. 2nd, when the most gorgeously brilliant display took place. It commenced immediately after sunset, and increased in splendor during the evening. For several hours, little was to be seen but a deep rich pink light over the southern part of the sky; but by degrees it extended, and, about nine o'clock, a huge pillar of fire appeared in the west, where it remained until midnight. After the moon went down, the brilliancy of the aurora increased, and from about half-past eleven till past twelve, a beautiful pale, soft, greenish-blue light, like the dawn of morning, extended itself above the southern horizon for about 100° or 110°, and about 18° or 20° in height. From this, streamers or radii of red, white and blue light shot upward to beyond the zenith, fully half the sky being covered with this splendid illumination, the light from which equalled that of the full moon in England. These radii converged towards a point about 15° north of the zenith, but did not themselves extend more than half that distance beyond the zenith. This was its last appearance, and a splendid finale it was. The powerful electric excitement in the atmosphere had an extraordinary effect on the telegraph wires, agitating the instruments violently in some places, and quite interfering with the transmission of messages.

ART. XXXVI.—*Geographical Notices;* by DANIEL C. GILMAN, Yale College Library. No XII.

REPRINT OF A TRACT, BY NICOLAUS SILLACIUS, (A. D. 1494), ON THE SECOND VOYAGE OF COLUMBUS.—Although the principal object of these "Geographical Notices" is to record the recent progress of our knowledge of the world, yet we cannot forbear to make mention of a remarkable publication which pertains to the discovery of the New World at the close of the fifteenth century.

Christopher Columbus, in his second voyage across the Atlantic, set sail from Cadiz, September 25, 1493. Soon after his return, Guglielmo Coma wrote from Spain to Nicolaus Sillacius in Pavia an account of the journey. These letters were translated by Sillacius into Latin, and such other information was added to them as could be gathered from current reports; and this whole account of the voyage of Columbus, was published under the title, "*De Insulis Meridiani atque Indici Maris nuper inventis.*" This curious tract has been almost forgotten for nearly four hundred years; and, at the present time, but two copies of the original edition are known to be in existence,—one belonging to the Marquis Trivulzio of Milan, and the other to James Lenox, Esq., of New York. The last named gentleman, with characteristic liberality, has made this pamphlet accessible to all scholars, by causing it to be carefully re-printed in the original Latin, with an English translation by Rev. James Mulligan, a biographical introduction, notes, and a bibliographical appendix, in which much important information is given in respect to the early printed account of the various voyages of Columbus. The whole work forms a quarto volume of about 180 pages, printed in a truly elegant style.

It cannot be expected that this tract will add very much to what is known from other sources of the great navigator and his voyages, but as a contemporaneous record of most important discoveries, the volume will always be prized not less by the geographer than by the historian and bibliographer.

VOYAGE AROUND THE WORLD OF THE AUSTRIAN FRIGATE NOVARA.—The reference which has been made in a previous page of this number of the Journal to the Austrian circumnavigatory voyage, furnishes us with an appropriate occasion for speaking of that exploring expedition.

The Imperial frigate "Novara," under the command of Commodore von Wüllerstorf, set sail from Trieste April 30, 1857, and returned to the same port August 26, 1859, having successfully completed a voyage of scientific observation around the world,

the first which was ever undertaken by the Austrian Navy. It is of course too early for the results of this expedition to be fully made public, but various accounts of the whole voyage, and of particular observations, have been given in the journal of the Geographical Society of Vienna, and in Peterman's *Mittheilungen*. *L'Institut* of Paris has also published a series of articles on the subject, communicated by M. Marschall of Vienna.

The "Novara" is a frigate of 1800 tons burthen, and 44 guns. It was manned by 354 men. The scientific corps, in addition to the commodore and other naval officers, consisted of the following naturalists, viz., Dr. Hochstetter, physicist and geologist; Frauenfeld and Zelebor, zoologists; Dr. Scherzer, ethnologist, having charge also of investigations in national economy, the botanist Jellinek and the artist Selleny.

Sailing, as we have stated, from Trieste, the expedition touched for longer or shorter periods at Gibraltar, (eleven days), Funchal, (nine days), Rio Janeiro, (three weeks), Table Bay, Cape of Good Hope, (twenty-four days), Island of St. Paul, (seventeen days), Point de Galle, (eight days), and Madras, (eleven days). Sailing from the last named port Feb. 10, 1858, from that time until August 11, a period of six months, the vessel was directed to various island and continental seaports of south-eastern Asia, including Nikobar, Singapore, Batavia, Hong Kong, Shanghai, etc. In September the island Puynipet was visited, and afterwards Sydney, (a month), Auckland, (seventeen days), Papeiti, on Tahiti, (eleven days), Valparaiso, (twenty-four days). Leaving the latter port May 11, 1859, the Novara reached Trieste at the end of the following August. The whole extent of the voyage was nearly forty thousand nautical miles.

Various letters and partial reports, submitted to the Academy of Sciences in Vienna, have already been printed, and a complete narrative of the voyage, and full reports of all the scientific observations which were made upon it, is soon to be prepared and printed.

DR. HAYES'S PROPOSED ARCTIC JOURNEY.—A meeting of the American Geographical and Statistical Society was held in New York, March 22, for the purpose of encouraging Dr. Hayes in respect to his proposed voyage to the Northern ocean. Dr. Hayes was present, and, in addition to his statements, an eloquent exhibition of the importance of this expedition, together with an appeal for material aid, was made by Dr. Francis Lieber.

Hon. Geo. Folsom, E. H. Vielè, Esq. Profs. Mitchell and Silliman, and Dr. A. H. Stevens also took part in the meeting, and letters were read in approval of the undertaking from Profs. Bache, Henry, Guyot, Dr. Gould, etc.

The general purpose of Dr. Hayes has already been set forth

in this journal, in a paper from his own pen, "On the Practicability of Reaching the North Pole," (vol. xxvi, p. 305-23, Nov., 1858.) At the recent meeting it was stated that ten thousand dollars had already been subscribed in aid of his enterprise, and at least ten thousand more are needed to insure the sending forth of the expedition.

The various weighty problems which are proposed for solution, especially the determination whether or not there be an open Polar Sea, present the strongest claims to the liberal contributions of all who are interested in the promotion of geographical discovery, or in the progress of physical science.

JOURNAL OF THE AMERICAN GEOGRAPHICAL AND STATISTICAL SOCIETY.—We are informed that this Journal, which has heretofore been published monthly, will hereafter be issued quarterly each number comprising at least 128 royal octavo pages. The first number, announced for the month of April, will comprise nearly 150 pages, consisting in part of original articles by the following writers: Commander Matthew F. Maury, Prof. Alexander D. Bache, Prof. Arnold Guyot, E. George Squier, Paul B. Du Chaillu, Dr. David Livingstone, Joseph C. G. Kennedy, James Wyne, M.D., together with late geographical and statistical intelligence, and careful notices of new scientific works bearing upon the objects embraced in the Society's labors. The subscription price to those not members of the Society will be three dollars a year.

Letters in reference to any matters connected with the Journal should be addressed to DANIEL WILLARD FISKE, General Secretary of the Society, New York.

EXPLORATIONS IN THE AMOOR REGION.—We have already called attention to the great efforts which are making by the Russian government to ascertain the resources and characteristics of Eastern Siberia, and to bring the immense region drained by the Amoor and its tributaries into connection with the commerce of the world. To our own countrymen, these investigations are especially important, when we consider the probable effect which will be produced upon the commerce of the Pacific.

At a recent meeting of the Royal Geographical Society of London, a paper was read, presenting extracts (prepared and translated under the direction of Capt. R. Collinson) from various official Russian reports, respecting the districts adjacent to the Amoor river. These extracts from the writings of Messrs. Pescurof, Vasilief, Radde, Usoltzof, Paragchefski, etc., are printed in the Transactions of the Royal Geographical Society, vol. 28,—together with an original map compiled by J. Arrowsmith. As our space at the present time allows us to quote but one of these reports, we have selected that by M. G. Radde, upon the table lands east and

southeast of the great Lake Baikal—or, as he terms it, the Dáuro-Mongolian Frontier of the Trans-Baikal region.

"If by the word 'Steppe' be understood an extensive, treeless and arid plain, without any considerable undulations, that term cannot, in its full sense at least, be applied to the tracts now under consideration. Scientifically, and with regard to the formation of its surface, this region should be described as an elevated extent of country, intersected by many bare mountain ranges; the valleys and low plains between which are in some places strongly impregnated with salt, and produce exclusively *chenopodeæ* whilst in others they receive the waters of many small springs and atmospheric moisture in the shape of snow and rain, giving rise to innumerable small, turbid, and muddy lakes, seldom containing water fit for use, but more often contaminated with saline and alkaline solutions. An ordinary observer, one who has not penetratrated into the external structure of the earth's surface, or, what is of greater importance, into the properties of the soil from which he derives his sustenance, would see here only a contrast of conditions, namely, the contrast of the wooded surface to the treeless and bare, inducing him to call such a country a steppe. Whether the latter surface be level, or high and undulated, it would equally by him be termed a steppe; and only perhaps in distinguishing two contiguous regions would the mountainous and desert zone be designated as the 'high' steppe.

The Dáur country on the Mongolian frontier cannot, both with relation to its absolute height and its topographical features, be even approximately compared to a regular steppe; nor can any parallel be drawn between the chemical properties of their vegetable strata. Whilst in many regions, as for instance in the extensive Orenburg, Taurida, and Bessarabian steppes, the chernozem, so favorable to cultivation, penetrates the surface to 2 and 3 feet, there is a total absence of organic matters in the woodless valleys of the Dáurian frontier table-lands; and the soil of that extensive region has not undergone any considerable change for many centuries, owing to all the elevations, and frequently the valleys, abounding in siliceous ("jasper and flint") formations, which either do not admit of precipitation at all, or with great difficulty; added to which, the decomposition of hard rocky masses is materially retarded by the dryness of the atmosphere, and the want of snow and rain.

A further exposition will show that, leaving aside the peculiar stamp which characterises the organic nature of this region, the material and moral condition of its scanty population have succumbed to the influence of the physical conditions abovementioned. The greater part of this desert track, perfectly unsuitable for the production of grain, is apparently, like the inhabited regions of the Gobi desert on the south, destined by na-

ture for the nomadic life of the wild and superstitious Mongol, who, spurning the ties of a fixed abode, scours the level plain on his fleet steed.

With respect to geographical position, the Dáurian frontier-steppes occupy a narrow zone between longitude 112° 30′ and 119° E.; their chief extension is from west to east, and they are only in a few places intersected by the parallel of 50° N. latitude. If the treeless elevations are alone to be denominated steppes, the boundary of the Russo-Dáurian steppes must be drawn southwards from Nijni-Ulhun frontier station, as the mountains on the banks of the Onon, extending farther west, are covered all over with dense forests; on the east, on the other hand, from the above station, and between Akshinsk and Mogoitu, along the right bank of the Onon, extends a forest of tall trees, the predominating family of which, the pines suddenly disappears a few versts east of Mogoitu, and is succeeded by a straggling wood of birch, as far as Kubuhai.

The steppe district thus only crosses the Onon at Niji-Ulhun, occupying also a small zone, well irrigated with numerous small streams, on its left bank.

In its easterly extension, parallel to the course of the Onon, the steppe is not bounded on the north by this river, but by a very thick forest extending between the Onon and the desert, in some places 10 miles in breadth. This forest is worthy of notice for its historic associations as the sojourn of Chingis-Khan, and also in a botanico-geographical respect, forming, as it does, a natural boundary between the river and the steppe, which is remarkable for its small breadth and its clearly defined limit on the south. The forest thins gradually towards the east, down the Onon, and terminates entirely at the place where the river bends abruptly to the north on meeting the western spurs of the Adoncholon mountains; farther in that direction, with a lesser fall, and often contracted between banks of granite, the river pursues its course as far as its confluence with the Shilka, through a wooded country more frequently overgrown only with bushes.

The frontier steppe, which has already a breadth of about 53 miles between the old Chindan fortifications and the Uldza river, extends towards the south along the confines of this pine forest, acquiring a greater width farther on. The Onon-Borza* rivulet, flowing from the northeast, and which likewise approaches the southern offshoots of the Adoncholon mountains at 116°, and after bending to the north unites after a course of twenty miles with the Onon at Ust-Borzinsk, belongs at its western middle course to the steppe region. In like manner, the more sloping southern

* This stream is called Onon-Borza in distinction to the three Borza rivulets which fall into the Argun.

declivities of the Adoncholon mountains, of which the summits alone are overgrown with stunted birch-trees (these are often, however, found in great density along the entire northern slope of that chain), are referable to the same region.* To the eastward, however, almost on the meridian of Tsagan-olu (116° 43′), two rows of woody elevations extend from east to north, intersecting each other at the most westerly lower range of the Buko-Hada, where the eastern branch terminates. The bare elevations running from this knot to the south expand the farther they extend, and form, near the frontier, the wooded table-land of Altangan, so called after one of its principal valleys.

The abovementioned mountains, which terminate in Buko-Hada, form first on their eastern, then on their northeastern extension, a water-shed between the affluents of the Onon and Argun; the Gazimur river takes its rise on its northern side. The Altangan table-land lies between two systems of saline waters; the lake of Tarei-nor is the largest representative of the western basins, whilst to the eastern belongs Ubudk, Tsagan-nor, Hara-nor, and many others. The culminating points of this region occur in the Steppe district, which here increases in width, being more than 67 miles in breadth between Tsagan-olu and Abagaitu. Having by barometrical measurement taken the height of Tsagan-olu at 2711 feet English, 500 more must be added for the mountain pass of Soktui. Only one valley, the largest and broadest of those occurring in the frontier steppes, on the Russian side, intersects the Altangan plateau from east to west, continuing on the other side of the mountains from their western slopes.† This is the valley of the Urulungui rivulet, which flows for 100–113 miles in a direction towards the Argun, and terminates there at Novo-Tsuruhaitui military station. At its lower course, the Urulungui flows gently along a winding channel, bordered at first occasionally by bushes of the willow, the precursors of a more luxuriant vegetation than that of the steppe. The region, however, between the Urulungui Argun, and Altangan plateau loses its vegetation more and more towards the south; on the frontier at Abagaitu it is intersected by parallel

* In Dáuria trees and bushes are only found on the northern slopes of mountains from two causes. The first is, that the southern slopes are much drier than the northern, which longer preserve the moisture of the soil, and so assist vegetation; whilst almost every plant withers in summer on the southern side of the mountain. The second cause is attributable to the circumstance that the fires which occur in the steppes in spring become sooner extinguished on the north side than on the opposite, where the snow leaves the ground earlier (being, in fact, perfectly dry by the end of February), thus offering no obstacle to the spreading of the fire. The limits of wood and bush vegetation are not governed in these regions by the rigor of the winter, but solely by the dryness of the soil and sultriness of the summer months.

† The Urulungui valley probably commences in the vicinity of the Chinese frontier, to the west of the Altangan plateau.

40° 35′ and is so unproductive and barren, that on that account alone, and without reference to its topographical features, it may be considered the extreme northeastern end of the Gobi desert, which extends to the lakes of Buir-nor and Dalai.

Broad, light-green, and low tracts, overgrown with reeds, and winding only along the very edge of the Argun, intersect the bare and rocky desert, the uniformity and character of whose vegetation is at last broken by the Urulungui rivulet, at Novo-Tsuruhaitui. Lower down from the mouth of that stream the valley of the Argun assumes another aspect; and the river itself, taking a bend to the northeast, visibly contracts and flows more rapidly. Here the chernozen soil of the valley with its diversified flora also makes its appearance, so that the Urulungui may not only be considered as the limit of the high Dáurian steppes, but also the sharply-defined natural boundary of their vegetation.

To the north of the Urulungui commences the district of the metalliferous deposits of the Nertchinsk mountain region, remarkable also for its vegetation, which, lower down in the valley of the Argun, is very rich in forms, particularly at Chalbuchi village. It is here that the Mongolian oak, the Corylus heterophylla and Betula dahurica, seen nowhere in Siberia, first occur. Lastly, possessing a sufficiently thick population, some portions of this region are highly favorable to the production of cereals; but it is less adapted to the depasturage of cattle than the steppes, on account of the many mountains by which it is intersected.

To describe in a few words the boundaries of the high Dáurian steppes, it suffices to say that their limit on the north is formed by a pine forest, extending along the right bank of the Onon, by the Onon-Borza rivulet and the Adoncholon mountains, together with the elevations at the upper courses of the Gazimur and Urulungui rivulets; on the southeast by the Argun; and on the south by the Chinese frontier laid down in 1727; the western extremity of the steppe being bounded by the forests on the right bank of the Onon,

The whole of this country, occupying an area of 380 square miles, attains an absolute height of 2200 (English) feet at its greatest depressions (namely, at Kulussutaefski military station at Bayrn-Tarei lake), and almost 3000 feet at its highest elevations. Numerous mountain chains, rarely however detaching isolated spurs, intersect it in various directions, forming broad valleys, with a saline soil, and which are often found to contain accumulations of precipitated Glauber salt and soda, but seldom any water-basins. Even where the latter occur, they never attain any considerable depth, and are mostly so shallow and level that after a snowless winter or hot summer they completely dry up and frequently

remain in that state for many years. The most striking example of this is afforded by the great Baryn-Tarei lake, lying south of the Kulussutaefski frontier station, which was found dry by Pallas in 1772: since then it filled with water, which again entirely evaporated five years ago, so that it now only presents a dry saliferous and muddy bottom, cracked in numerous wide fissures by the burning rays of the sun. With the exception of a few rills, generally filled only by snow water in spring, and remaining perfectly dry during the greater part of the year, a small number of spring morasses are alone to be found there. Not unfrequently such morasses occur in the vicinity of saline lakes; but often, having no efflux, they drain themselves, when, owing to the pressure of water beneath, the surface around their swampy edges rises several fathoms in winter with its icy covering. The ice remains in such places until the middle of summer; and even so late as the month of June have I seen on a freshwater morass near Kulussutaefski, in the neighborhood of Tarei lake, blocks of ice one inch thick, capped, as it were, with a layer of earth of the same thickness, overgrown with reeds.

With such a scarcity of water and so great an elevation, it is conceivable that the atmosphere of this region must be very dry. To the south of this frontier zone, at the same time, extends an immense desert, and on the north, the rain-clouds, being attracted by a dense forest, and arrested by elevated ranges, discharge their waters to superfluity over the wooded district of Nertchinsk; whilst some 7 to 14 miles to the south not a drop of rain or dew will fall for months together. At the village of Tsagan-olu, I witnessed, at the beginning and latter part of the month of June, examples of such an unequal distribution of moisture; whilst the heaviest rains and storms, continually interfering with my excursions, prevailed at midday in the forests only 5 miles to the north, buckwheat was being scorched 3 miles to the south of the village, and no rain had fallen since the middle of May at the frontier stations of Soktuisk, (40 miles farther to the south), and Kluichefski and Chindan (33 miles more westerly). It is to be regretted that scarcely any observations on the moisture of the atmosphere of this elevated region have hitherto been made, as, together with a better knowledge of the chemical properties of the soil, they might have led to some definite conclusion on the greater or lesser fitness of the country for agriculture. At the same time we find that almost useless experiments on the growing of corn have for many years been repeated with great perseverance at the military settlements on the frontier. In none of the extensive and remote regions of Russia, in the same latitude, are there, probably, presented so many local conditions unfavorable to agriculture as in the frontier steppes of Dáuria; and it is very doubtful whether, even with increased

labor, and the introduction of a better system of tillage, any regular or even moderate harvests can be obtained. Not only is there on one side the want of rain and snow, and the great elevation to influence the early autumnal frosts, but on the other the very properties of the soil offer still greater obstacles to cultivation; to be surmounted perhaps only by a Chinese density of population, and Chinese industry.

The very soil of these regions is of a twofold nature: a great part of the steppes, and all the mountain-chains in particular, are as if sown with flint, jasper, and chalcedony, deeply buried in a hard argillaceous sand, and forming also the upper vegetable strata, which present no traces of fertility; whilst all the depressions of the surface are impregnated with salt, and therefore produce only a few saline plants. The climate is at the same time unfavorable to the growth of any plant. Severe snowless winters prevent the cultivation of winter wheat, while the early autumnal frosts are generally prejudicial to crops, and impede the fallow tillage. Spring wheat and buckwheat are consequently alone sown; and even these crops perish in great part from the droughts in May and June, no shade being afforded to their roots by their thin foliage and feeble growth of stem, which rises only one foot from the ground. As a rare exception, a snowy winter will sometimes follow a series of dry years; but this, although acting beneficially on the fields, is of great injury, by its long continuance, to the cattle, which are not unfrequently entirely destroyed by the want of fodder. Under ordinary climatic conditions, the want of snow is the chief impediment to their safely passing the winter; so that, on the freezing of the few fresh springs, the animals suffer much more from thirst than from hunger, and from the first half to the end of December are often so reduced that, even with a sufficiency of food, they are unable to survive the second half of that month.

Appreciating the advantages which Eastern Siberia derives from the opening the Amúr to commerce, Mr. Radde proceeds to consider the present agricultural wealth of the Dáurian Steppes, and its future influence and development. The first part of the paper has already shown the unproductiveness of the country, and the great obstacles presented by its climate. Cattle-breeding and sheep-farming in particular, would alone appear to admit of some development, as the lowlands and steppes afford good pasturage; and the prejudicial climatic conditions might, with perseverance, foresight, and industry, be rendered less unfavorable.

Wool is the only article which Mr. Radde adduces as an export, and he considers there will be no difficulty in finding a mar-

ket for it in the United States. The frontier region of Dáuria and Mongolia is capable, the author thinks, of producing two millions of sheep; whilst cattle-rearing must for some years remain in its present state, owing to the scantiness of population, and the difficulty of making provision for the winter."

KHANIKOFF'S TRAVELS IN PERSIA.—Through the attentions of D. W. Fiske, Esq., General Secretary of the American Geographical Society, we have received the *Proces-Verbal* of the meetings of the Imperial Geographical Society of St. Petersburg, held Dec. 16, 1859, and Jan. 13, 1860.

At the latter sitting, M. Khanikoff presented an account of his researches in Persia, to which, in a former number of this Journal, a brief allusion was made. His remarks were chiefly directed to the Province of Khorassan, as will be seen from the following abstract which we translate from the *Proces-Verbal.*

The limits of this vast province, bounded on the north by a plateau which stretches in the direction of latitude from Hindou-Kousch to the southern extremity of the Caspian Sea, and toward the west by another plateau, making an angle of from 20 to 30 degrees with the meridian, are far less clearly defined toward the east. This traveller is of the opinion that Khorassan may be justly considered as bounded in this direction by the western slopes of Hindou-Kousch, which stretch from Hérat to Kandahar, as well as by the mountains which separate Séistan from Béloudchistan. The space thus enclosed presents four natural sub-divisions, to which M. Khanikoff gives the name terraces. The first embraces the salt desert lying between Kaschan, Koum, Bastam, Nichibour, and Tebbès. Its general inclination is directed from northeast to southwest, and its lowest point is on the line joining Bastam and Tebbès. The second comprehends the dry desert of Lut, and toward the north borders on the preceding; the mountains of Kirman are its southern limit; its general inclination is from north-northwest to south-southeast, and its lowest point is probably no more than 500 feet above the level of the sea. The lowest point of the third, which includes Séistan, is at the surface of Lake Hamoun, of which the waters are 1,545 feet above the level of the sea. Finally, the fourth terrace, which is the least extended, is bounded by the line which, on one side, joins Birdjand and Sebzar, and on the other stretches from the first of these villages to Toun, Haff, and Yezdoum; its general slope inclines from southwest to northeast. The limits of these four divisions of Khorassan are not everywhere well defined, but they are clearly indicated by the directions of the water courses and the inclinations of the ravines. The northern frontier of Khorassan coincides with the isothermal line of 12° Cent., a fact which gives plausibility to the conclusion, that over all the expanse of the northern plateau of cen-

tral Asia, from Orenbourg to Meshed, over a space of 20 degrees in breadth, the annual temperature seldom falls to 6° Centigrade; at the southern limit of the first terraces described above, date trees grow and produce fruit in abundance, from which we must conclude that the annual temperature here is not below 18° Cent.; hence, in this direction and in moving toward the equator two geographic degrees only, the mean annual temperature acquires an increase equal to that gained to the north of Meshed by a progress of 20 degrees along the meridian. M. Khanikoff calls the attention of the Geographical Society to this point, that the rapid elevation of the degree of the annual temperature cannot be explained by the astronomical and hypsometrical coördinates alone, of the regions where this increase has been observed. He thinks that one of the essential causes to which it should be referred is the dryness of the air, which rapidly increases from the southern shore of the Caspian Sea to the frontier of Béloutchistan, so that in the desert of Lut the atmosphere contains only $\frac{13}{100}$ths of relative humidity.

After entering into detail relative to the probable limits of the highest temperature, determined from the softening observed in the stearine which was in the baggage of the members of the expedition, M. Khanikoff has described some of the most striking atmospheric phenomena which he has himself studied in Khorassan. He mentions, among others, waterspouts and whirlwinds of dust, the dry mist, the atmospheric fluctuations, the mirage, and finally, observations upon the zodiacal light, which was seen by the expedition while traversing the space between Anarderré and Kirman. In conclusion, M. Khanikoff presented to the assembly the whole trigonometrical network, by aid of which the sketches which he has traced were drawn, adding the details of the circumstances which accompanied the operations. For want of time he reserved to another meeting the enumeration of the ethnographical labors of the expedition.

Art. XXXVII.—*Correspondence of Prof. Jerome Nicklès, of Nancy, France, dated Feb. 26th*, 1860.

French Academy of Sciences. Public meeting and distribution of the prizes.—This meeting was held Jan. 30; it was concluded with the eulogy upon Thénard, pronounced by Flourens, one of the Perpetual Secretaries. The following is a summary of the principal prizes awarded.

Prize for Astronomy.—This prize was awarded to Robert Luther, for the discovery of *Mnemosyne*, the only new planet of the year 1859. Mnemosyne is the 57th of the group of telescopic planets between Mars and Jupiter, and the 8th of those which are due to Mr. Luther. The

Academy has already four times awarded the prize to this astronomer for the discovery of the five planets, Thetis, Proserpine, Bellona, Leucothea, and Fides.

The prize for Mechanics was awarded to Mr. Giffard for the invention of a new feeding apparatus for steam boilers, which he calls an "*automatic injector.*" The report, made by Combes, gives great praise to this apparatus. The injector very advantageously replaces the feeding pumps of steam boilers. In addition to the fact that it avoids all loss of heat, other than that which results from the cooling of the exterior of the tubes in which the steam and hot water circulate, the absence of any movable solid parts, exposed to wear and derangement, the extreme facility with which the quantity of water supplied can be regulated between limits sufficiently narrow, &c., render it very valuable for locomotive machines. Accordingly several great railroad companies have already applied it to machines of this kind.

The automatic injector takes its origin from an observation made by Savart in 1832, in his experiments upon the fall of liquid veins; a current let fall from a vessel where the level is maintained at a given height, penetrates directly and quite unbroken into a vessel where the surface is less elevated.

The observations of Savart; the phenomena, long known, of the communication of lateral motion to fluids by which is explained the action of those blowing machines called *trompes;* the forcible drawing in of air through the intervals which separate the bases of the tuyeres of high furnaces; the effects of the blast pipe of locomotive engines, &c., have given Mr. Giffard a hint which has led him to the invention for which the Academy have just awarded a prize.

Mr. Giffard, who is a person of very earnest spirit, is known in France by his attempts at guiding balloons. Some years since he obtained evident results in directing them, since he succeeded in making his balloon move *against* the *wind*, at the Hippodrome in Paris. This fact was stated in a report signed by intelligent men. Although without means to continue his researches upon this point, Giffard was not discouraged. Instead of making a show of his misery, and representing himself as persecuted by science, or as a martyr to an idea, he left balloons for the time and set about the construction of locomotive machines; he thought of his injector, and put off his researches upon ballooning until by his labors he should again be furnished with means to continue them; complete success crowned his investigations; he has now placed himself in a condition to take up again his favorite pursuits.

Physical sciences.—A prize has just been divided between Daubrée, Dean of the Faculty of Science at Strasbourg, and Delesse, mining engineer at Paris. The question proposed for competition was in respect to the *Metamorphism of Rocks.* The report of the committee will not convey to the readers any more knowledge than they have already obtained from this Journal which has often made mention of the labors of Messrs. Delesse and Daubrée.

The *Prize for experimental Physiology* has been awarded to Pasteur for his researches in regard to *fermentation.* They bear upon *alcoholic fermentation, lactic fermentation,* and *tartaric fermentation,* and of their

isomeric compounds. The report of the committee has particular reference to the physiological side of the question; it was edited by Claude Bernard, an emimently distinguished physiologist. The following extract is from the report:

Following the example of Cagniard Latour, Pasteur considers the yeast of beer an organized body; he regards the modifications which it undergoes during alcoholic fermentation as of a nature essentially vital, and he shows that the chemical phenomena of fermentation are connected with a perpetual renewal of the yeast; whence it follows that, during the alcoholic fermentation, the sugar not only gives origin to chemical substances which disengage themselves or remain dissolved in the liquid, but at the same time, there is still a portion of the sugar which is taken up by the yeast in the form of cellulose, and another portion in the form of fatty matter, while the nitrogen of the old yeast serves to regenerate the new. Pasteur has made in this respect an experiment which, so to speak, reduces physiological conditions to the most simple relations which can connect living beings with mineral nature. He has shown, in fact, that the globules of yeast develop and multiply, and that the sugar ferments, when a quantity of the globules, so to speak, imponderable is sown in a medium composed at the same time of: 1st. A solution of pure candied sugar. 2d. An ammoniacal salt, the dextrotartrate, for example. 3d. Mineral substances containing phosphates. The ammonia is seen to disappear and to be transformed into the complex albuminous matter of the yeast, while at the same time, the phosphates give up their mineral constituents to new globules. The carbon which is one of the constituent elements of the yeast, is evidently furnished by the sugar. Before Pasteur, the lactic yeast was generally considered as organic matter in process of alteration, but not as organized matter. Our author has discovered and pointed out the special character of a lactic yeast, which is much more minute than the yeast of beer. During the lactic fermentation this yeast buds and multiplies, behaving in the matter of reproduction very much like the yeast of beer. In regard to the fermentation of tartaric acid and its congeners, Pasteur has arrived at very unexpected results, and which have a high chemical and physiological interest. Putting into conditions of fermentation with albuminous matter, and at a suitable degree of heat, the racemate of ammonia, which is formed by the union of the right- and left-handed tartrates of ammonia, and which has no effect on polarized light, it was seen that the phenomena of fermentation finally manifested themselves, and that new chemical products were formed at the expense of the racemate of ammonia. But it is remarkable that only the elements of the *right*-handed tartrate separate or ferment, to give rise to the products of fermentation, while under the same conditions the *left*-handed tartrate remains unaltered in solution in the liquid, which then acts very energetically upon polarized light. In this fermentation there is produced a yeast peculiar to the right-handed tartaric acid, which developes itself in presenting the characteristics of a mycodermic vegetable.

This example proves, in the plainest manner, the influence of the molecular dissimilarity of organic bodies, in the phenomena of fermentation. It is, indeed, impossible otherwise to interpret the marked differ-

ence which, in this respect, the right and left tartaric acids exhibit, since both have exactly the same physical properties, the same chemical composition, and they differ only in the interior arrangement, which gives to their constituent parts a rotatory power equal, but in opposite directions, and which corresponds to the dissimilarity which is reproduced in their aptitude or inaptitude, to be influenced by ferments.

In short, Pasteur regards the chemical phenomena of fermentation as being always correlative to the vital phenomena of organization, and to the development which takes place, at the same time, in the organized yeasts which have the power to excite it.

The committee judged that the author, in thus pursuing the physiologic study of the yeasts, in the direction which he had chosen, would bring new light to bear upon a series of organic products, which are related to the phenomena of nutrition and histogeny.

Transplantation of the Periosteum.—Honorable mention was also made of Mr. Ollier, in reference to his interesting experiments on the transplantation of the periosteum, preserving its property of renewing the osseous tissue. The author showed, that if a strip of the periosteum is detached from the bone of a living animal, and is transplanted to the subcutaneous cellular tissue upon the same animal, or upon another individual of the same species, the fragment of periosteum becomes encrusted, and continues to live in such a manner that vessels are formed in its substance, and communicate with those in its vicinity, as can be proved by careful injection after death: Ollier has likewise proved that, several hours after death, the possibility of this transplantation of the periosteum still remains.

Prize relative to the unhealthy Arts.—This prize was awarded to the inventor of a lamp suitable for giving light to laborers at work beneath the surface of water. It is a lantern, consisting of a thick cylindrical covering of glass fixed between two iron plates. A reservoir, containing a mixture of alcohol and turpentine ("burning fluid") is placed in the interior. Where the apparatus is plunged into water, the air necessary to support combustion comes to the bottom of the lantern through two iron tubes opening up into the atmosphere. The products of combustion are likewise removed by means of a tube fitted to the centre of the upper plate, which is also prolonged so as to open into the atmosphere, and of which the section is double that of both the tubes, through which the external air is supplied.

The inventor of this apparatus was a simple workman, named Guigardot. With this lamp it is possible to work under water to the depth of twenty metres. It has been used with success at the works of the monumental bridge built over the Rhine, at Strasbourg. It illuminates a circle of $2^{m}\cdot 50$ radius, even in turbed water.

Medical Prize.—In the preceding years we gave much praise to the committee in charge of the medical prize. They sometimes expended even 90,000 francs in prizes of all kinds, awarded even to chemists whenever their labors were important to one of the branches of the healing art. This year the committee have been economical, one knows not why, inasmuch as important works are not wanting. They have awarded only honorable mention to physicians for the labors of their profession.

Prize for Organic Chemistry.—The opposite state of things appears in the department of chemistry, which for the first time has awarded a prize to living chemists. A sum of 6000 francs was divided between Messrs. Wurtz and Cahours; to Prof. Wurtz for his researches upon glycol and its derivatives, and upon the new bases containing oxygen recently discovered; to Prof. Cahours for his labors in reference to the organic radicals. Be it understood, there is no question as to who is the discoverer of the composite radicals (Liebig), nor of the organo-metallic radicals (Lœwig).

Bréant Prize.—This prize is in reference to cholera and contagious diseases. It has not been awarded. The pieces sent for this *concours* were mostly mere letters, containing medical formulæ, all, according to their authors, infallible for the cure of the cholera, and all wanting, both in practical observations in regard to this dangerous disease, and in rational deductions as to the nature of its attack, and the symptoms which accompany and constitute it. The following are the principal questions proposed for the *concours* of 1861 and 1862:—

1st question. "Discuss carefully and compare with theory, the observations upon the tides, made in the principal ports of France."

2nd quest. "To complete in some important point the geometrical theory of polyhedrons."

3rd quest. "Establish the general equations of the movement of the earth's atmosphere, taking into consideration the rotation of the earth, the calorific action of the sun, and the attractive forces of the sun and moon."

4th quest. "Study of any question, at the option of the candidates, relative to optical phenomena."

5th quest. "At different points of the thermo-electric scale and for a difference of temperature reduced to 1° C. to determine the directions and compare the relative intensities of the electric currents produced by different thermo-electric substances."

6th quest. "Determine by experiment the causes which influence the difference of position of the optical and photogenic foci."

7th quest. "Comparative anatomy of the nervous system of fishes."

8th quest. "Study of the hybrid vegetables, with respect to their fecundity, and the perpetuity or non-perpetuity of their character."

9th quest. "Study of the mode of formation and of the structure of spores and of the other organs which contribute to the reproduction of fungi, their physiological office, the germination of the spores, and particularly with reference to parasitic fungi, their mode of penetration and developement in other living organized bodies."

Each of these prizes consists of a medal of the value of 3000 fr.

10th quest. "Essay upon carefully made experiments, to throw new light upon the question of so-called spontaneous generation."

11th quest. "Experimental study of the modifications which can be effected in the developement of the embryo of a vertebrate animal by the action of external agents."

12th quest. "Study of the distribution of the vessels of the latex in the different organs of plants, with particular regard to their relation to or connection with the lymphatic or spiral vessels, as well as with the fibres of the liber."

13th quest. "Determine experimentally what influence insects may exercise upon the production of the diseases of plants."

Each of these prizes consists of a medal of the value of 2500 fr.

Besides these prizes there are others: especially the Bréant prize (100,000 fr.) is to be awarded "to the person who shall have discovered the means of curing the Asiatic cholera, or shall have discovered the causes of this terrible scourge."

Now that we are upon the subject of prizes we will also say that a prize of 6000 fr. is offered by the society of Pharmacy at Paris, for "the question of the artificial production of quinine, or in default of this, of a substitute possessing equivalent anti-febrile properties." The prize for artificial quinine has been open since 1849.

Time strictly limited to July, 1861.

These concours are open to all scientists without distinction of country.

Obituary. Death of Poinsot.—This illustrious geometer died on the 10th of December last, at the age of 83 years. Born in 1777, he carried on his studies at the Polytechnic School from which he went out in 1796 in the character of an engineer of bridges and highways. He was successively, Professor at a Lyceum in Paris, and member of the higher council of public instruction. He had been a member of the Institute since 1813 where he took the place of the mathematician Lagrange. We shall speak no further of the titles, the dignities, and the decorations of Poinsot, since all these objects of human pride are but a vapor, and have added nothing to his merits, whom mechanicians place, in the history of mechanics, immediately after Archimedes, Galileo, Huygens and Newton. His principal claim upon the memory of posterity is founded upon "*La Statique*," the "*Memoire sur l'équilibre et le mouvement des systèmes*, 1806," and the *Théorie nouvelle de la rotation* and the *Mémoire sur les cônes circulaires roulants;* the last memoirs were in 1853.

In the *Elémens de Statique*, he brought to light his beautiful theory of couples and its application to the conditions of the equilibrium of machines.

Poinsot had a truly philosophic mind; he knew how to render the most abstract matter accessible and to bring it down to the most elementary ideas; this power was one of the characteristic traits of his genius. It is equally recognized in his work upon the precession of the equinoxes, that remarkable phenomenon discovered by Hipparchus, and explained two thousand years afterwards by d'Alembert.

Poinsot retained his intellectual activity to the close of his life. He never experienced the trials which are often met by men of science in their pathway. While yet young he saw the most illustrious judges proclaim his rare talents, and his life has always been happy and honored; Poinsot was not only a great savant, but moreover a good man.

Discovery of an Intra-Mercurial Planet.—The year 1860 has been inaugurated in France by an astronomical discovery, all the more remarkable because made in unusual circumstances. It is the discovery of another planet between the earth and the sun, verifying the conclusion to which Leverrier had arrived by the power of computation, that there existed one or more planets within the orbit of Mercury. This conclusion was announced to the Academy at the session of September 12,

1859, in a letter from Leverrier to Mr. Faye. Very soon reports were spread that the discovery of such a planet was no new thing. Among these reports, the most persistent was this, that such a planet had been observed towards the commencement of the year 1859, by an amateur astronomer, a poor country doctor. Leverrier, resolving to trace this story to its source, went personally to Orgères, a village in the Department of Eure and Loir, where this doctor resided. Great was the emotion of Dr. Lescarbault on seeing before him the illustrious director of the Observatory of Paris. He had little difficulty in satisfying Leverrier that the observation had really been made. He said, "On the 26th of March last, (1859), about four o'clock, in pursuance of my regular habit, I was exploring the sun's disc with my telescope, when suddenly I noticed near the border a small, well defined, round black spot, having a sensible motion. It advanced farther and farther on the sun's disc. Unfortunately just then occurred a call from a patient. I went down below, gave my advice to the patient, reascended to my observatory, the black spot continued its journey, and I saw it reach the opposite border of the sun and disappear, after being about an hour and a half on the disc."

The measurements made by Dr. L., with very simple means of his own, enabled Leverrier to determine that the chord of the sun's disc traversed by the planet was $9' 17''$; and it would have required $4^h\ 26^m\ 48^s$ to traverse the diameter of the sun. He estimates the angular diameter of the planet at about a quarter that of the planet Mercury when in transit. If this estimate is correct, the new planet is insufficient to account for the anomaly in the motion of the perihelion of Mercury; and there probably is, as M. Leverrier announced, a group of small planets between Mercury and the sun. Observers should give special attention to this subject at the next total solar eclipse.

The new planet has not been officially named, but it is already designated as *Vulcan*. Its return is looked for at Paris at the end of March or the beginning of April.

This discovery was not a matter of mere chance. Dr. Lescarbault, having observed the transit of Mercury, on the 8th of May, 1845, conceived the idea that if there were between the earth and the sun any planetary body besides Venus and Mercury, it must also sometimes cross the sun; and that by frequently observing its disc, such body might be detected thereon as a black point.*

Without fortune and without means of observation, Dr. Lescarbault was unable to obtain a telescope until 1853; and it was not until 1858 that he commenced systematic work. He made most of his auxiliary apparatus for himself, and went to work in astronomy very much as Scheele did in chemistry.

* In 1836 and 1837, M. Pastorff of Buchholz observed, several times, a pair of small, round black spots, of unequal size, passing, in a few hours, across the sun's disc, and each time in a different path. These must have been a planet with a satellite. Messrs. E. C. Herrick and Francis Bradley of New Haven, in 1847, endeavored to re-discover those bodies, by observing the sun's disc, twice a day, with a large telescope, and by exploring the vicinity of the sun with the telescope, armed in front with a pasteboard tube, blackened within. These efforts proved unsuccessful. (See this Journal, Nov. 1859, vol. xxviii, pp. 445–6.) The hypothesis of an intra-mercurial planet was also proposed, many years since, by M. Buys-Ballot, in his researches on a maximum and minimum of the solar heat.

M. Leverrier further informs us that the explanations which M. Lescarbault gave him entirely satisfied him that the alleged observations were real, and are entitled to a place in science; that the long delay in making them public is due solely to the modest reserve of the observer, and the quietude of a residence remote from the excitement of great cities. In support of his assertions, M. Leverrier has exhibited the proofs which he found in the domicil of Dr. Lescarbault, viz. a paper on which the latter had marked, on the 26th of March, 1859, the appearance of the black point on the solar disc, and a pine board on which Dr. L. had chalked his computations and his drawings.

Hardly had these announcements been made, when several other similar observations were called to mind. Mr. Scott, Chamberlain of London, writes in Galignani's Messenger of January 14, 1860, that an Englishman, Mr. Lloft, observed, January 6, 1818, a black point traversing the sun's disc, and that he (Mr. S.) had made a like observation in the summer of 1847, had thence concluded the existence of a third inferior planet, and had so published. His son, five years old, standing by, was placed at the telescope, and confirmed the observation by crying out, "I see a little balloon on the sun."

M. Wolff of Zurich has published several ancient observations, which he thinks refers to the transit across the sun of an intra-mercurial planet, viz. by (1,) Scheutzer of Crefeld, June 6, 1761; (2,) Staudacher, towards the end of February, 1762; (3,) Lichtenberg, November 19, 1762; (4,) Hoffmann, at the beginning of May. 1764; (5) Dangos, January 18, 1798; (6,)Fritsch, October 10, 1801; (7,) Stark, October 9, 1819. All these observers saw a round well-defined point, of the apparent diameter of Mercury, crossing the solar disc in a short period, varying from two to three hours. Using only the observations 5, 6 and 7, Mr. Wolff finds that they accord with the supposition of a planet having a revolution around the sun in 19·25 days, a result surprisingly near to that (19·7 days) deduced by M. Leverrier, from the observation of Dr. Lescarbault.

In settling the question, who is entitled to the credit of the discovery, it is clear that Dr. Lescarbault's observations are the first which have the scientific seal, and are of such a nature as to be subject of computation.

New Members Elected.—

Fizeau was elected 2nd of January member of the section of Physics in place of *Cagniard de Latour*—deceased.

Plana was elected March 5, foreign associate in place of *Lejeune-Dirichlet* deceased.

At the session of the *Académie* held March 19, Mr. J. A. Serret was elected a member in the section of Geometry in place of *Poinsot*, deceased.

Dausey has lately (March 12) communicated to the Academy a memoir, setting forth the geographical and physical observations of Hommaire de Hell made in 1846–48 in Turkey and Persia, where he died at Ispahan in August, 1848. His manuscripts were taken to France by Jules Laurent the well-known artist who accompanied the expedition. Dausey reports the barometric and astronomical determinations of much value.

Hypnotism and Magnetism.—French society has been almost carried away with a kind of scientific epidemic which may be compared to the

malady called "table tipping," which was so much in vogue a few years since. This epidemic is Hypnotism or nervous sleep. It was brought into notice by a French surgeon following a work of Dr. Braid (published 15 years since), in which this physician, describes under the name of Hypnotism, the nervous affection which is produced under the following circumstances:—When a brilliant object is placed directly before the face at a distance of from 8 to 15 inches, and the subject of the experiment is requested to fix his eyes steadily upon the object, in such a way as to produce a permanent contraction of the muscles of the eye and eyelid, there will be seen to come on, after the lapse of a few minutes, a state analogous to catalepsy. M. Broca, having successfully employed hypnotism in producing insensibility to pain, proposes it to surgeons as an anæsthetic agent capable, in many cases of being substituted in practice for ether or chloroform, "because," as he remarks to physicians, "this method, introducing as it does no substance into the system, appears to me absolutely harmless."

From the numerous experiments which have since been made, it results; 1st, that this kind of anæsthesia can be developed but rarely, and in persons whose nervous system is especially predisposed; 2nd, that hypnotism may give rise to attacks of epilepsy.

This kind of anæsthesia is therefore not less open to objection than the other. Its origin is more ancient than is supposed; it was established more than two centuries ago, under the name of *irradiation* or *phenomena of actinobolism*, by P. Kircher in his *Ars magna Lucis et Umbræ*.

It is concluded that in hypnotism is seen only a fact in the domain of animal magnetism, which is not yet a science. We will not enter further into the details.

Porous bodies.—Among the topics of scientific interest which awaken attention at present, is the research of Jamin, professor at the École Polytechnique, upon the equilibrium and movement of fluids in porous bodies. The new results at which he has arrived afford an explanation of the ascent of the sap in vegetables without the necessity of recourse to the vital force. It is apparently a question of capillarity only.

Jamin has applied the new facts which he has discovered to the construction of an apparatus composed entirely of inorganic materials, but showing in its structure a great analogy with vegetables. This apparatus has the property of raising water as trees do, to a height greater than that attained by means of atmospheric pressure, from a moist soil whence the water is constantly drawn to the factitious leaves where it is continually evaporated.

Reduced to its most simple form this apparatus is composed of a block of some well dried porous substance as chalk, lithographic stone, &c., or a porous battery cell filled with a powder well rammed in, white chalk for instance, oxyd of zinc, or even with earth. A manometer is imbedded in the interior of the mass, and the whole is plunged in a vessel full of water. The water immediately penetrates its pores and drives out the air, which collecting in the interior, exercises a pressure upon the manometer amounting with oxyd of zinc to *five* atmospheres and with starch it exceeds *six* atmospheres. This is not the limit of the greatest possible pressure; Jamin makes known the causes which diminish it in these cases and proves that the water is forced into porous bodies with a force which he calls π, and which is equal to that of a considerable number of atmospheres.

A tube 1·20 metres long filled with plaster and terminated at the summit by an evaporating surface is inserted by its base into a reservoir closed and filled with water; a vacuum is caused measured by 15 or 20 millimetres of mercury or by 200 or 270 millimetres of water; and the water appears even at the upper extremity of the tube, which proves *that porous bodies are able to raise water higher than can be done by atmospheric pressure.* These facts cannot be explained by the ordinary laws of capillary attraction, since these bodies are not formed of impermeable tubes, but of corpuscles in juxtaposition, separated by small empty spaces. Jamin has therefore submitted the problem to the calculus and has come to results, of which we mention the following:

If in a damp porous body, the water is compressed by a power of several atmospheres, it can congeal only at a temperature below 0° C.* Consequently the old wood is able to resist frost, while the young shoots being less dense are unable to do so.

Since water in filtering through a porous body is compressed as it enters and dilates again as it runs out, it should exhibit electric currents and many other phenomena.

The theory can not be applied to non-homogeneous porous bodies. In the extended memoir which he has prepared, Jamin discusses the complicated results which may be occasioned by irregularity of structure; he makes an application of it to wood, and shows that the interior pressure must be augmented in the denser tissues; that the air must come from the larger tubes, which cannot serve for the ascent of the sap.

It is plain that the evident tendency of all these experiments is to explain the ascent of the sap in vegetables by capillarity. The idea is not new, but it has not been hitherto fully admitted, notwithstanding the experiments which have been heretofore made.

Jamin gives it probability in showing by decisive experiments, that porous bodies exercise a capillary action superior to the pressure of the atmosphere; further, he gives the physical theory of capillarity in porous bodies and succeeds in calculating the phenomena of the movement of liquids in trees. This is thoroughly physiological. If the Academy of Sciences could award the great prize for physiology, for a work upon fermentation, when it is not yet known whether this purely chemical phenomena is the result of vital action as Cagniard de Latour maintains, or the manifestation of a mechanical effect as Liebig explains it, in his beautiful theory of fermentations,—if this work in chemistry deserved the great prize for experimental physiology for a stronger reason should they award this prize for the splendid physical researches of which we have just spoken.

Application of electric light in Medicine.—The ordinary processes of illumination are of very difficult application when the object is to employ artificial light in diagnoses or in certain cases in operative medicine, inasmuch as the illumination is insufficient, or the light is more or less colored, and is accompanied with heat. It is not the ordinary electric light here spoken of, but that produced by induced currents. The problem to be solved consists in finding a source of light with little or no heating effect, which can be compressed into tubes of small capacity and of forms adapted

* This fact has just been demonstrated by Mr. Sorby for water contained in capillary tubes of a small diameter.—J. N.

to circumstances, and which finally is of such whiteness as not visibly to alter the color of organic tissues illuminated by it. This problem has just been solved by Dr. Fonssagrive, physician of the naval school at Brest, with the coöperation of Messrs. Ruhmkorff and DuMoncel. The apparatus consists of an empty tube of Geissler, of very small diameter folded and turned upon itself after the manner of multipliers; this tube contains a gaseous mixture which gives a perfectly white light when it is traversed by an electric current produced by an induction coil.

Phosphorescence.—The electric lamp (*photophore*) of which we were just speaking calls to mind some observations upon phosphorescence which have just been made by Phipson; he has found that like cane sugar, the *sugar of milk* or *lactine* becomes luminous by concussion, and also by fracture. To evolve phosphorescence from the *nitrate of uranium*, it is sufficient to shake briskly a bottle containing a certain quantity of this salt in the crystalline state. The light is very vivid when the experiment is made with one or two kilograms of this substance. Calomel possesses the same property, although in a less degree.

Works of Arago.—The sixteenth and last volume of Arago's works has just appeared. It contains a great number of unpublished researches made by Arago during his career.

The sixteen triangles are here given which Messrs. Biot and Arago determined in the prolongation of the meridian of France to the island of Formentera. Arago alone, has measured besides, a seventeenth triangle having its summit at the enclosure of Galaro in the island of Majorca and resting in one direction upon Camprey in the island of Trizza, and the other upon the mole of Formentera, with the design of obtaining the length of an arc of the parallel nearly 3° from the extremity of the meridian, and to determine the curvature of that portion of the earth's surface. The results of these measurements, heretofore unpublished, are found in this volume.

Arago in the year 1853, the very year of his death, communicated to the Academy a memoir upon the figure and physical constitution of Mars, which is likewise contained in this volume, and is accompanied by more than 3000 micrometrical measurements of the diameters of Mars, Jupiter, Saturn and Uranus, which were taken from 1811–1847.

It is well known that Arago made numerous investigations in regard to the refractive power of atmospheric air, dry or humid, and of different gases and vapors. He labored in this department for nearly half a century, namely, in 1805 with Biot, in 1815 and 1816 with Petit, and in 1852 with Fizeau. Chemists and physicists will derive much advantage from the determination of the refractive powers not only of some simple gases, but also of compound gases such as oxyd of carbon, carburetted hydrogen, sulphuretted hydrogen, cyanogen, the vapors of sulphur and of carbon, of sulphuric ether and chlorohydric ether.

This volume also contains studies upon optics, atmospheric electricity, &c. United to the other fifteen volumes it forms a lofty monument to science as well as to one of its most noble representatives.

Bibliography.—The following works have just appeared at Paris.

Oeuvres d'Arago, Tom. xvi, 1859; chez Gide, 5 rue Bonaparte.—This volume is devoted to scientific notices bearing upon the personal labors of Arago, many of which have never before been published.

At the *Librarie centrale des sciences rue de Seine; Recherches sur le non-homogénéité de l'étincelle d'induction*, par M. Th. du Moncel, 1860.—M. du Moncel, who gives all his leisure to the study of electricity and its applications, presents in this brochure the result of his researches upon the electric spark, and especially the spark of induction, of which the non homogeneity was discovered by him in 1855.

At Baillière Bros., Paris & New York. *Traité èlementaire de Physique expérimentale*, Tom. 1. 12°, 1860. par M. Forthomme, professor of Physics at the Lyceum in Nancy.—This work, even by the confession of the author, contains nothing new, but is distinguished by its method. The most difficult questions in regard to gravity, hydrostatics and heat are explained in the first volume with great clearness, and thus rendered intelligible to persons little versed in these matters, which are so important in our day and have so many useful applications.

By *Lacroix & Baudry*, Quai Malaquais.—*Grands hommes et grandes choses, notices scientifiques sur les inventions et sur les decouvertes modernes et sur les auteurs, par Victor Meunier.* 8°, 1860—This work appears by numbers, once a week; its author, of whom we have often spoken, has acquired in France a great reputation as a popularizer. He established the *Ami des Sciences*, a journal for scientific discussions, which he has directed for six years, and is remarkable for the independence of his opinions and judgments. M. V. Meunier in this new publication proposes all the great scientific questions of the day.

Art. XXXVIII.—*Description of an Equatorial recently erected at Hopefield Observatory, Haddenham, Bucks;* by the Rev. W. R. Dawes.

(*From the Monthly Notices of the Royal Astronomical Society.*)

My observatory was furnished, in May last, with an equatorially-mounted telescope by Messrs. Alvan Clark and Sons, of Boston, U. S., which in several important points differs from any other in this country; and I therefore hope that a brief description of it may not prove uninteresting to the Royal Astronomical Society.

The form combines great firmness and compactness with considerable elegance of design. The massive part of its structure is of cast-iron, the base of which is firmly bolted down to a stone pier. The semicircular form of the upper part affords a secure position for most of the wheel-work of the driving-clock, of which the going-weight descends in a groove on the east side of the pier, and is not seen in the drawing. The space between the polar axis and the semicircular bed-piece is occupied at its lower part by the hour-circle. Immediately above this is a sector, which clamps on to the axis, and the wheel-work of the clock occupies the upper portion. The sector has a radius of rather more than 9 inches and an arc of 30°, or two hours of right ascension. This arc has a face of an inch and a half in breadth, between which and a cylinder 7 inches in circumference there is just room enough for two thin bands of sheet-brass, each of about three-fourths of an inch in width, to pass side by side. These bands are both keyed by the end into one groove in the cylinder, at such a distance that they cannot overlap or interfere with each other. They are then bent round the cylinder in

opposite directions, the end of one being fastened to one extremity of the arc of the sector, and the end of the other at the other extremity of the arc to a piece of brass which is acted upon by a screw and nut, for giving to both the bands a due degree of tension. The sector and cylinder thus move together without friction, irregularity, or lost time.

Upon the same arbor with the cylinder is the wheel, 15 inches in circumference, in the racked edge of which the driving-screw works. This arrangement gives the screw about the same driving-power as if it acted on the edge of a wheel nearly 40 inches in diameter, fixed on the polar axis.

I have every reason to be satisfied with the going of the driving-clock; and the cylindrical bob of the pendulum being screwed on to its steel rod, the rate is capable of adjustment to the greatest nicety. Great care has been bestowed by the makers upon the accurate dividing of the wheel-work; and I have much pleasure in acknowledging that its performance fully bears out my expectations, founded on the character given by the Messrs. Bond of the clock-work applied by the same makers to the great Munich equatorial in Harvard Observatory, which has been so successfully employed for the purposes of telescopic photography. While the speed of the clock is regulated by the vibrations of the half-seconds pendulum, the action of the pendulum on the wheel-work is rendered smooth and equable by an ingenious application of Bond's *Spring-governor;* and so perfectly successful is this contrivance, that with the thread of the micrometer bisecting a star, and a power of 800 or 1000 on the telescope, no interruption or jerk from the escapement is perceivable.

For producing a slow motion in right ascension, the driving-screw is mounted on a brass frame, which, being carried by a fine screw under the observer's control, acts as a slipping-piece through nearly five minutes of time.

A firm clamp, close to the cradle of the telescope, fixes the declination-axis, and is accessible to the observer both at the eye-end, and also during the setting of the declination-circle. A slow-motion screw acts on an arm extending from the clamp to the bottom of the cradle to which the screw is attached.

To permit the adjustment of the polar-axis to the latitude and meridian of the place, the upper part of the cast-iron bed-piece is made with a groove which receives loosely a projecting keel on the portion bolted down to the pier. The form both of the groove and of the keel being semicircular, the upper portion is moved upon the lower by the stout screw which is seen in the drawing, and the polar-axis is thus easily raised to the required angle. The adjustment to the meridian is performed by the screws on each side of the groove in the upper piece pressing

against the keel in the lower, which has play enough in the groove to allow of a moderate degree of azimuthal motion.

To facilitate the finding of objects in Mr. Clark's "Two-eye-piece Micrometer," when their distance exceeds the field of one of the eye-lenses, the finder is furnished at its eye-end with a small position-circle divided into degrees. The thick wires of the finder being placed in the direction of the objects to be measured, the reading of the position-circle indicates the approximate setting for the micrometer, whereby the two objects may be immediately found by their respective eye-lenses. The aperture of the finder being two inches, it will show a star of the $9\frac{3}{4}$ magnitude of Struve's scale.

The object-glass of the telescope has a clear aperture of $8\frac{1}{4}$ inches, and a focal length of about 110 inches. The materials were furnished by Chance and Co., of Birmingham. The figure is excellent to the circumference, and the dispersion but little over-corrected. Its performance fully supports the character of Mr. Alvan Clark's object-glasses, and I believe it to be capable of everything which can be performed by such an aperture. It clearly divides γ^2 *Andromedæ*, and shows the smallest companions among the stars of the Pulkova Catalogue.

Haddenham, near Thame, November, 1859.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Fraunhofer's Lines.*—KIRCHHOFF has communicated a preliminary notice of very remarkable investigations on the spectra of colored flames. These investigations have given an unexpected clue to the origin of Fraunhofer's lines, and justify some remarkable conclusions as to the constitution of the atmosphere of the sun and perhaps also of the more brilliant fixed stars.

Fraunhofer remarked that in the spectrum of the flame of a candle there are two bright lines which correspond with the two dark lines D of the solar spectrum. The same bright lines are obtained more easily and stronger from a flame into which common salt has been introduced. The author produced a solar spectrum and allowed the sun's rays before they fell upon the slit to pass through a powerful salt flame. When the sunlight was sufficiently weakened two bright lines appeared instead of the two dark lines D; when however the intensity of the sunlight exceeded a certain limit, the two dark lines D were seen with much greater distinctness than without the presence of the salt flame.

The spectrum of Drummond's light usually contains the two bright sodium lines, when the luminous portion of the lime cylinder has not been exposed to ignition for a long time; if the lime cylinder remains fixed, the lines become weaker and finally vanish. Under these circumstances, a

salted alcohol flame placed between the lime cylinder and the slit, produces two dark lines of extraordinary sharpness and fineness, which exactly correspond to the lines D of the solar spectrum. In this manner these lines are artificially produced in a spectrum in which they do not actually occur.

If we introduce chlorid of lithium into the flame of Bunsen's gas lamp, the spectrum exhibits a very bright, sharply defined line, which lies in the middle between Fraunhofer's lines B and C. If we allow solar rays of moderate intensity to pass through the flame to the slit, we see the line bright upon a dark ground; but with a greater intensity of the sunlight a dark line occupies its place, which has precisely the character of Fraunhofer's lines. When the flame is removed, the line vanishes completely.

The author concludes from these observations that colored flames in whose spectra bright sharp lines occur, diminish the intensity of rays of the color of these lines, when these pass through them, to such a degree, that in place of the bright lines, dark ones occur, whenever a source of light of sufficient intensity is placed behind the flame. He further concludes that the dark lines of the solar spectrum which are not produced by the earth's atmosphere result from the presence in the ignited sun's atmosphere of those substances which produce in the spectrum of a flame bright lines in the same place.

We may assume that the bright lines corresponding with D in the spectrum of a flame always arise from the presence of sodium; the dark lines D in the solar spectrum allow us therefore to conclude that sodium is present in the sun's atmosphere. Brewster has found in the spectrum of the flame of saltpetre bright lines in the position of Fraunhofer's lines A, α, B; these lines indicate the presence of potassium in the sun's atmosphere. From the author's observation that no dark line in the solar spectrum corresponds to the red lithium line, it would follow that lithium is either not present at all in the sun's atmosphere or is present only in very small quantity.

The investigation of the spectra of colored flames has in this way obtained a new and high degree of interest. The author promises to pursue the subject in connection with Bunsen, and states that they have already obtained results which render it possible to determine the qualitative constitution of complicated mixtures from the appearance of the spectrum of their blowpipe flames. In pursuing together the investigation of Kirchhoff's discovery of the influence of flames upon rays of light, a remarkable fact has appeared which promises to be of great importance. Drummond's light requires a salt-flame of low temperature in order that the lines D may appear dark. The flame of alcohol and water is adapted to this purpose, but the flame of Bunsen's gas lamp is not. In this last case, the smallest perceptible quantity of salt causes the bright lines to appear. The authors reserve the developement of the consequences of this remarkable fact.—*Pogg. Ann.*, cix, p. 148, *January*, 1860.

Note.—Professor Stokes, in a letter to the editors of the L. and E. Phil. Mag. directs attention to the fact that Foucault, in 1849, published a short paper in l'Institut containing observations exactly analogous to those of Kirchhoff. Foucault's experiments were made by transmitting solar light through the galvanic arc, and appear to have escaped attention until their second and independent discovery by Kirchhoff.

2. *On the direct conversion of lactic into propionic acid.*—The conversion of lactic into propionic acid was first observed by Ulrich who obtained chloropropionic acid by distilling lactic acid with perchlorid of phosphorus. Lautemann has succeeded in a more direct transformation of lactic acid by the agency of iodid of hydrogen. When concentrated lactic acid is diluted with half its volume of water and the cooled liquid is saturated with iodohydric acid gas, iodine is set free and if the liquid is heated in a closed tube to 140° C. iodine is separated in quantity. The filtered liquid neutralized with potash and then distilled with dilute sulphuric acid yields propionic acid mixed with some iodine and iodohydric acid which are easily separated by carbonate of silver. The reaction which yields propionic acid is expressed by the equation

$$\underset{\text{Lactic acid.}}{C_6H_5(HO_2)O_4} + 2HI = \underset{\text{Propionic acid.}}{C_6H_6O_4} + 2HO + I.$$

The same result may be still more easily obtained by distilling lactic acid with biniodid of phosphorus when propionic acid passes over colored by free iodine. Lautemann considers these results as confirming Kolbe's view that lactic acid is propionic acid in which one equivalent of hydrogen is replaced by one of the compound radical HO_2.—*Ann. der Chem. und Pharm.*, cxiii, 217.

3. *On the Formation of Alanin from Lactic Acid.*—Strecker's discovery that alanin may be converted into lactic acid by the action of nitrous acid is familiar to chemists. Kolbe has succeeded in effecting the reverse process, that is, in obtaining alanin from lactic acid. Lactate of lime is to be converted into chlorid of propionyl by distillation with perchlorid of phosphorus and this by treatment with absolute alcohol into chloropropionic ether. The ether is then to be treated with a concentrated solution of ammonia in a closed tube for several hours. The solution on evaporation gives a mixture of sal-ammoniac and alanin which are easily separated. The reaction in this case is expressed by the equation—

$$C_6H_4ClO_3,\ C_4H_5O + 2NH_3 + 2HO = C_6H_4(NH_2)O_3,\ HO + NH_4Cl + C_4H_5O,\ HO.$$

Ann. der Chem. u. Pharm., cxiii, 220.

4. *On the Constitution of Lactic Acid*—Kolbe has communicated a very interesting discussion of this subject, maintaining his view that lactic acid is monobasic and is to be regarded as oxypropionic acid. We must however refer to the original paper which does not admit of condensation.—*Ann. der Chem. und Pharm.*, cxiii, 223.

5 *Contributions to the Chemistry of the Platinum-metals.*—Claus has continued his investigations of this most difficult subject and has obtained many new and interesting results, the most important of which we shall here state.

In the present paper, the author confines his attention chiefly to ruthenium and its analogies with osmium. The hydrated deutoxyd of ruthenium has the formula $RuO_2 + 5HO$ and may be obtained either by digesting the corresponding chlorid with carbonate of potash and washing the oxyd with boiling water, or by precipitating the sulphate of the oxyd with potash or soda. The sulphate in question may be prepared by oxydizing the sulphid with nitric acid. The oxyd when freshly precipi-

tated is a slimy ochre-colored substance which dries to a rust colored mass resembling hydrated sesquioxyd of iron. It dissolves in acids with a bright yellow color, and gives on heating with chlorhydric acid, a beautiful red solution of the chlorid. When heated to 300 C. the oxyd loses a portion of its water, and when more strongly heated, explodes in consequence of the instantaneous separation of the remaining water.

The author describes in full the double salts $RuCl_2$, KCl and $RuCl_2$, NH_4Cl and gives their reactions with the usual reagents. Ruthenium forms, like osmium, an acid containing four equivalents of oxygen, as well as the ruthenic acid RuO_3 already described by Claus. To this acid Claus gives the name of hyper-ruthenic acid; it may be prepared by passing a current of chlorine into an alkaline solution of ruthenic acid, prepared by fusing metallic ruthenium with caustic potash and saltpetre.

The hyper-ruthenic acid, being volatile, distils over, and may be collected in a receiver. The new acid is a golden yellow crystalline and volatile substance. On gentle heating, it melts into golden yellow drops, which again solidify as a crystalline mass. The acid is very volatile and evaporates at ordinary temperatures; its gas has a golden yellow color, and the acid has a peculiar smell which resembles that of nitrous acid. The gas irritates the lungs and produces a cough, but does not attack the eyes like osmic acid. It has little taste, but is somewhat astringent, though not acid. Its boiling point lies not far above 100° C.

The acid is very easily reduced. In the moist state its solution is decomposed after a few hours, with formation of the sesquioxyd. Alcohol and the greater number of organic bodies easily reduce it. Potash dissolves the acid slowly, but the solution passes after a time into one of ruthenate of potash. The author gives the relations of this body to other reagents in detail, but for these we must refer to the original memoir.

When a solution of $RuCl_2, NH_4Cl$ is evaporated to dryness with an excess of ammonia, the chlorid of a new base is formed, the formula of which, according to Claus, is $2NH_3 . RuCl + 3HO$. The salt has an Isabel yellow color, and crystallizes in rhombic tables, which are soluble in water and yield on heating pure metallic ruthenium. When this chlorid is digested with an excess of freshly prepared oxyd of silver, it yields a solution of the oxyd. On evaporation, this solution gives a rather porous, yellow, crystalline mass; this base appears to be even more caustic than caustic potash itself. The author promises a more complete description of this interesting base, as well as additional contributions to the chemical history of iridium and rhodium.—*Journal für prakt. Chemie.* vol. lxxix, 28, *January* 31*st.*

6. *Synthesis of new Bases containing Oxygen.*—Wurtz has observed the remarkable and important fact that oxyd of ethylene $C_4H_4O_2$ combines directly with ammonia, forming very well defined and powerful bases. When concentrated aqueous ammonia is mixed with oxyd of ethylene and the mixture is allowed to stand, combination ensues with a strong evolution of heat. On evaporation an alkaline syrup is obtained which gives with chlorohydric acid a solution from which colorless rhombohedra separate. These have the formula $C_{12}H_{15}NO_6 + HCl$: the platinum salt is $C_{12}H_{15}NO_6, HCl + PtCl_2$. The mother liquor of these rhombohedra contains another chlorid, the platinum salt of which has the formula—

$C_8H_{11}NO_4$, $HCl+PtCl_2$. The formation of these bases is indicated by the equations—

$$3C_4H_4O_2+NH_3=(C_4H_4O_2)_3H_3N=C_{12}H_{15}NO_6$$
$$2C_4H_4O_2+NH_3=(C_4H_4O_2)_2H_3N=C_8H_{11}NO_4.$$

These results may perhaps lead to the discovery of the true constitution of the complex organic alkaloids containing oxygen. They appear to show that Berzelius' view that the alkaloids are conjugates of ammonia may be true in some cases at least.—*Comptes Rendus*, xlix, 898.

7. *On a new series of Alcohols.*—Wurtz has also found that oxyd of ethylene unites with water to form new alcohols which he terms diethylene and triethylene alcohols. The reactions involved are represented simply by the equations—

$$C_4H_4O_2+2HO=C_4H_6O_4.\quad \text{Glycol.}$$
$$C_4H_4O_2+2HO=C_8H_{10}O_6.\quad \text{Diethylene-alcohol.}$$
$$3C_4H_4O_2+2HO=C_{12}H_{14}O_8.\quad \text{Triethylene-alcohol.}$$

Oxyd of ethylene also unites directly with glycol so as to form the diethylene and triethylene alcohols. The equations are

$$2C_4H_4O_2+C_4H_6O_4=C_{12}H_{14}O_8.$$
$$C_4H_4O_2+C_4H_6O_4=C_8H_{10}O_6.$$

All these substances behave like alcohols. The diethylene alcohol was also discovered by Lourenço and called by him the intermediate ether of glycol.—*Comptes Rendus*, xlix, 813.

8. *Researches on the Platinum metals;* from a letter of Dr. Wolcott Gibbs to one of the editors.—"The completion of my researches on the platinum metals has been delayed much longer than I expected. This has arisen partly from the intrinsic difficulty of the subject and partly from its expansion in particular directions in a very unexpected manner. A brief sketch of the results already obtained—imperfect as they are—may perhaps interest you.

The osmium-base of which Dr. Genth and myself published a brief notice about two years since in the Journal, proves to be the type of a very extensive series of compounds which promise to be of much theoretic interest. You will remember that the chlorid of that base is produced when osmite of potash, KO, OsO_3, is added to a solution of sal-ammoniac. I now find that new complex bases are formed when the osmite is added to solutions of the chlorids of narcotin-ammonium, cinchonin-ammonium, &c., &c.; in short, almost all the complex alkaloids which I have yet tried give analogous bases containing osmium in the radical. The new bases are very easily decomposed with evolution of osmic acid. They are more stable in the presence of an excess of chlorhydric acid and give crystalline double salts with the chlorids of gold and platinum.

These however are not the only or even the most remarkable basic compounds which I have discovered. Many of the ammonia-metal bases already described are capable of forming new bases into which osmium enters either as a conjugate body or as replacing hydrogen. When, for instance, osmite of potash is added to a solution of the chlorid of pallad-diamin, $2NH_3.PdCl$, a yellowish brown solution is formed

which on addition of chlorhydric acid gives a beautiful yellow crystalline precipitate insoluble in cold water and containing palladium, osmium and the elements of ammonia.

When osmite of potash is added to a solution of chlorid or sulphate of luteocobalt, $6NH_3 . Co_2Cl_3$ or $6NH_3 . Co_2O_3 . 3SO_3$, a buff-yellow precipitate is thrown down, which on addition of HCl gives a wine-yellow solution. This solution after a short time deposits beautiful crystals of the chlorid of a new base containing osmium, cobalt, and the elements of ammonia. The chlorid gives well crystallized salts with the chlorids of platinum, gold and mercury. Its solution is decomposed by gentle heating, osmic acid being evolved while a black powder is thrown down.

The other ammonia-cobalt bases give analogous compounds which however are decomposed almost as soon as formed. It is my intention if possible to examine the relations of osmite of potash to one or two of the arsenic and antimony bases, as for example to the chlorids having the formulas $(C_4H_5)_4AsCl$ and $(C_4H_5)_4SbCl$.

When ammonia is added to a solution of osmite of potash, the red color of the latter passes immediately to wine-yellow. Fremy supposes that an osmiamid is formed here having the formula $OsO_2 . H_2N$. I find that the product is a new osmium base, the chlorid of which is formed at once by neutralizing the yellow solution with HCl. This chlorid has probably the formula $\left.\begin{matrix} H_3 \\ Os \end{matrix}\right\} NCl$, though it may be $\left.\begin{matrix} H_3 \\ OsO_2 \end{matrix}\right\} NCl$. In like manner I am still doubtful whether the true formula of the other ammonia-osmium base is $2NH_3 . OsO_2 . O$ or $2NH_3 . OsO + 2HO$. Experiments now making will decide this point. Meantime I may say that I do not agree with Claus in considering the formula $2NH_3 . OsO_2 . O$ improbable since we have many analogous cases, as for instance in xanthocobalt and flavocobalt, the formulas of the chlorids of which are, you will remember,

$$NO_2 . 5NH_3 . Co_2 O . Cl_2 \text{ and } 2NO_2 . 5NH_3 . Co_2 O . Cl_2.$$

The chlorid $2NH_3 . OsO_2 . Cl$ or $2NH_3 . OsCl + 2HO$ is decomposed by boiling with chlorhydric acid, giving sal-ammoniac and a new chlorid which is perhaps identical with that just mentioned. This is easily explained by the equation

$$2NH_3 . OsCl + HCl = NH_3 . OsCl + NH_4Cl,$$

and the decomposition would then be exactly analogous to that of pallad-diamin under the same circumstances

$$2NH_3 . PdCl + HCl = NH_3 . PdCl + NH_4 Cl.$$

I hope soon to decide these questions by experiment and analysis, but the analyses are very difficult and tedious, and I have to feel my way and find out new methods for almost every determination.

You will see from the above that osmium is likely to become one of the most interesting of the elements, and that it is capable of forming an extraordinary number and variety of compounds. I am also very busy with the remarkab e class of double nitrites which I described at the meeting of the Am. Association for the Advancement of Science in August last. There are still some difficulties to be overcome, but I am

confident that I shall be able to effect a perfect separation of all the metals of the platinum group. The white iridium salt appears to have the formula $Ir_2O_3.2NO_3+3KO.NO_3$, but it contains a small quantity of chlorine which may be essential. Its stability and insolubility are very remarkable, and will I think prove of great value in separating iridium from the other metals. The corresponding salts of the other metals of the group appear to be all soluble. The ruthenium salt is soluble even in alcohol and ether, and gives with sulphid of ammonium a magnificent red solution. This is by far the most delicate test for ruthenium yet discovered as the reaction is peculiar to that metal. Claus' beautiful reactions with sulphocyanid of potassium and sulphydric acid are much inferior for qualitative purposes."

New York, March 30, 1860.

TECHNICAL CHEMISTRY.

9. *Solution of Cellulose in Ammonio-oxyd of copper.*—Some time since ERDMANN, in his *Journal für praktische Chemie* (lxxvi, 386) expressed the opinion that cellulose is not really dissolved by cuprate of ammonia, as stated by SCHWEIZER (*ibid*, lxxii, 109), but only swollen to a sort of thin mucilage like the well known limpid "solution" of starch.

This view was based upon the fact that when a clear solution of cellulose in NH_3 CuO is diluted with a large excess of water, the cellulose separates entirely in the course of a few days.

In defense of his original statement Schweizer now urges that the cellulose must be really dissolved by ammonio-oxyd of copper: since its fibres are unquestionably destroyed when this reagent comes in contact with them—as may be distinctly seen with the microscope; and since the cellulose precipitated from a solution of cotton in the above mentioned reagent no longer exhibits any trace of definite structure.

A solution of cotton in NH_3 CuO may also be filtered perfectly clear—although this operation is somewhat difficult when large quantities of the liquid are operated upon. The solution is moreover capable of passing through the cell membranes of plants as shown by CRAMER (*ibid*, lxxiii, 6).

The destruction of the solvent power of the cuprate of ammonia by dilution appears to depend upon alterations which this compound itself undergoes under certain circumstances. It often happens that a solution of ammonio-oxyd of copper which at first dissolved cotton with the greatest ease gradually loses this power even when kept in carefully closed vessels completely filled with the liquid. It is moreover well known to chemists that solutions of ammonio-oxyd of copper, and of the ammonio-copper salts, undergo decomposition when diluted with large quantities of water;—hydrate of copper being precipitated.

This decomposing influence which water exerts upon solutions of the compound of ammonia and oxyd of copper is in the opinion of Schweizer the cause of the gradual precipitation of cellulose from such solutions when these are largely diluted.—*Journal für praktische Chemie*, lxxviii, 370; compare also CRAMER, *ibid*. lxxiii, 1, *et seq.*

10. *Decoloration of Indigo by Sesquioxyd of Iron.*—According to KUHLMANN, when a solution of blue indigo is acted upon at the temperature of 150° (C.) by hydrated oxyd of iron its color is, almost immediately, completely destroyed. The same thing occurs with a number of other coloring matters.—In noticing this fact BARRESWIL suggests that persul-

phate of iron may perhaps be applied in calico-printing as a discharge for indigo and also in bleaching blue rags for paper making.—*Répertoire de Chimie Appliquée*, Oct. 1859, p. 429.

[The observation that salts of the sesquioxyd of iron have the power of bleaching indigo and other organic coloring matters was first made by Prof. H. WURTZ of Washington and published two years since in this Journal (vols. xxv, 378, and xxvi, 52)—also in the Proc. of Amer. Assoc. 1858 and in several foreign journals—to him unquestionably belongs whatever credit may attach to the discovery—F. H. S.]

11. *Aluminum Leaf*—A Parisian gold beater, DEGOUSSE, has succeeded in obtaining leaves of aluminum as thin as those from gold and silver. The aluminum must be reheated repeatedly over a chafing dish during the process of beating. This leaf is less brilliant than that of silver but it is not so easily tarnished as the latter. It is easily combustible, taking fire when held in the flame of a candle and burning with an exceedingly intense white flame.

According to FABIAN, (Dingler's polyt. Journal, cliv, 438,) the chemical lecturer will find aluminum leaf to be well adapted, for exhibiting the characteristic properties of the metal. It dissolves, for example, with surprising rapidity in a solution of caustic alkali.

[A specimen of this leaf accompanies the description of it in *Répertoire de Chimie Appliquée*, Oct. 1859, p. 435, also Nov., p. 488.]

12. *Critical and Experimental Contribution to the Theory of Dyeing.*—Under this title a somewhat extended treatise by Prof. BOLLEY of Zurich has appeared in the L. E. and D. Philosophical Mag. [4] xviii, 481, Supplement to Dec. 1859.

Two questions have long been agitated among chemists interested in the theory of dyeing. (1.) In what part of the colored fibre is the coloring matter situated? Does it merely adhere to the surface, or does it penetrate the entire substance of the cell-walls of such fibres as cotton and flax? Or lastly, in the case of such fibres is it stored up in the interior of the cells? (2.) What is the nature of the union between the dye and the fibre? Is it a chemical combination, or is it due to mere surface attraction? After comparing the various theories which have been advanced during the last century and discussing the merits of each, the author records the results of his own experiments, from which it appears that wool and silk in all cases where they have not been dyed with colors in a mere state of suspension* seem to be impregnated with the dye throughout their entire mass; while in the case of cotton, by far the larger portion of the coloring matter adheres to the surface of the fibre, the penetration of the cell-walls by the dye being either very slight or altogether wanting.

That the theory of W. Crum (L. E. and D. Phil. Mag,, April, 1844,—compare this Journal, [2], xxviii, 125), in accordance with which the tubular form of the cotton fibres is an essential condition to their taking a dye, is unfounded, appears from the fact that the amorphous cotton-gelatine precipitated from its solution in cuprate of ammonia (see this Journal [2], xxvii, 118) may be mordanted and dyed like ordinary cotton. In like

* In which case the coloring matter only adheres as a crust to the surface of the fibre.

manner sulphate of baryta and other pulverulent mineral bodies may be mordanted and dyed with decoctions of dyewoods.

With regard to the nature of the force which binds the coloring matter to the fibre—whether or no it be chemical attraction? Bolley concludes that there is no sufficient reason for accepting the view, principally developed by Chevreul [and by Kuhlmann, Comptes Rendus. Tomes xlii, xliii *et* xliv], that dyeing is a direct consequence of chemical affinity. He believes that the power possessed by fibres of attracting certain bodies—whether salts or coloring matters or both—from their solutions, belongs to that class of phenomena which results from the action of finely divided mineral or organic bodies (charcoal or bone black for example,) on such solutions. The distinction between the action of charcoal and of fibres in thus removing saline matters, or dyes, from their solutions is one of *degree* only, the nature of the operation being identical in either case.

A given weight of well prepared animal charcoal can, as a rule, deprive a larger quantity of liquid of its color than an equal weight of wool or silk. Neither wool or silk can remove *all* the color from a solution as charcoal can, their effect extending only to a certain degree of dilution beyond which the particles of coloring matter resist their attraction. Dyes which may have been taken up without a mordant by wool or, especially, by silk may be removed again by long washing in water, a fact which is not true in the case of charcoal, or only to a very slight extent. The attraction of coloring matters for water is therefore more completely overcome by charcoal than by animal fibre; but even the cleanest vegetable fibres, as, unmordanted and completely bleached cotton, possess a certain power of attracting coloring matter. That cotton should have less effect in this matter than wool or silk is not surprising in view of the great difference in the structure of cotton fibre as compared with that of the two substances last mentioned. It is well known that wool and silk in consequence of their physical constitution belong to the class of strongly absorbent or hygroscopic substances, *i. e.* in consequence of a certain porosity or looseness of their particles they swell up when moist and become easily penetrated by a liquid throughout their entire mass; on the other hand the cell-walls of cotton fibres are denser, less penetrable and at the same time thinner and therefore unable to contain the same quantity of liquid.

It has been often urged that since fibres, especially those of animal origin, not only exert an attraction for salts &c. but also possess the power of decomposing some of them, their action must be chemical But in this respect the behavior of charcoal is similar to that of the fibres. So too with regard to the increased attraction for color exhibited by mordanted cotton which is on a par with the fact observed by Stenhouse that the decolorizing power of wood charcoal is considerably increased by precipitating alumina upon it.

According to the Author mordants act by producing insoluble colors (lakes). Their behavior towards coloring matters in solution must be ascribed to chemical affinity, with which however the fibres themselves have nothing to do.

The so-called substantive dyes become insoluble from some other cause than the addition of a mordant, for example oxydation of protoxyd of iron, or of white indigo.

That common alum with which wool or silk has been impregnated is able to attract coloring matter from solutions and precipitate it on the fibres depends not upon the strength of the chemical affinity of these fibres for the coloring matter, but upon the fact (experimentally proved by Bolley) that they become saturated with the alum which cotton does not.

13. *Cellulose Digested by Sheep.*—The researches of several German chemists* have proved that the cellulose of plants is by no means so indigestible a substance as was at one time supposed, but that on the contrary it is digested in considerable quantities, by the ruminants at least, especially when a portion of the food of the animal consists of some substance rich in oil.

In order to ascertain to what extent the digestibility of cellulose may depend upon its state of aggregation, SUSSDORF and A. STŒCKHARDT have undertaken a series of experiments, of which only a very brief abstract can be here given. From their results it is evident that even the most compact kinds of cellulose can be in great measure digested by sheep. The experiments, commenced in July, 1859, were upon two wethers respectively five and six years old. These were fed: 1st, upon hay alone; 2d, upon hay and rye straw; 3d, hay and poplar wood sawdust which had been exhausted with lye; in order that the sheep should eat the sawdust it was found necessary to add to it some rye-bran and a small quantity of salt; 4th, hay and sawdust from pine wood mixed with bran and salt; 5th, hay, spruce sawdust, bran and salt; 6th, hay, paper-maker's pulp from linen rags and bran; after several unsuccessful attempts to induce the sheep to partake of the pulp when mixed with dry fodder it was at last given to them in a sort of paste or pap prepared by mixing bran with water. The experiments were continued until November, with the exception of a short intermission during which the animals were put to pasture in order that they might recover from the injurious effects—probably due to the resinous matters of the spruce wood,—of the fifth series of experiments.

The animals, as well as their food, drink and excrements were weighed every day. The amount of cellulose in the excrements was also daily determined by analysis. The composition of the food ingested having been previously ascertained.

It appeared that when the animals were fed: (1.) with hay (35 lbs. per week), 60 to 70 per cent of the cellulose contained therein was digested, *i. e.* it did not appear as such in the solid excrements. In this experiment the animals gained 7½ lbs. in 18 days. (2.) With hay 14 lbs., and straw 7 lbs. (per week), 40 to 50 per cent of the cellulose of the straw was digested. The animals having lost 2½ lbs. in 11 days. (3.) With hay 10½ lbs., poplar saw-dust 5¼ lbs., bran 7 lbs. (per week), 45 to 50 per cent of the cellulose of the poplar wood was digested. The animals having gained 2½ lbs. in 13 days. (4.) With hay 10½ lbs., pine wood saw-dust 7 lbs., bran 10½ lbs. (per week), 30 to 40 per cent of the cellulose of the pine wood was digested. The animals having gained 10 lbs. in 24 days. (5.) With hay 9⅓ lbs., paper-maker's pulp 7 lbs., bran 14 lbs. (per week),

* For a portion of these interesting results, see: *Agriculturchemische Untersuchungen und deren Ergebnisse angestellt u. gesammelt bei der landwirthschaftlichen Versuchstation in Mœckern.* Leipzig, WIGAND, 1852-57; also *Die landwirthschaftlichen Versuchs-Stationen.* Dresden, WERNER, 1858-59.—F. H. S.

80 per cent of the cellulose of the paper pulp was digested. The animals having gained 7 lbs. in as many days.

These experiments are to be continued, and more particularly with a view of ascertaining whether any nourishing effect is to be attributed to the cellulose.—STŒCKHARDT'S *Chemischer Ackersman*, 1860, No. 1, p. 51.

II. GEOLOGY.

1. *Notes on the Geology of Nebraska and Utah Territory*, (in a letter to one of the Editors from Dr. F. V. HAYDEN, dated Fort Laramie, March 3d, 1860.)—It will be seen by referring to the several memoirs, published in connection with my associate, Mr. Meek, and the second edition of a geological map of Nebraska and Kansas, that the great Lignite Tertiary Basin covers a vast area in the northwest. We find by personal observation that it occupies the greater portion of the country bordering on the upper Missouri, Yellow Stone and Big Horn rivers, that it extends far up into the Wind river valley and west along the North Platte road to the Sweet Water mountains, the Cretaceous rocks being exposed here and there by local upheavals, only except along the base of the mountains.

The lignite beds, which are well developed south of Fort Laramie extending along the base of the Laramie mountains to the Arkansas and southward, furnishing the coal or lignite in the vicinity of Denver City and probably forming a part of the same basin.

I have, in a former paper, suggested that fresh-water deposits near Fort Bridger are probably on a parallel with the estuary beds of Judith river, which at that time were not positively known to be Tertiary. The facts now in my possession show, with a good deal of certainty, that they form the lower portion of the great Lignite Basin. These estuary deposits, which occur in a number of localities in the west and northwest, as along the Grand and Cannon Ball rivers, at the mouth of the Judith on the Missouri, near the mouth of the Big Horn on the Yellow Stone, seem to have ushered in the tertiary epoch of the West, which had already been foreshadowed in Cretaceous formation No. 5,* by the Tertiary character of the Mollusca. We have already, in a former paper, noted the fact that a large portion of the fossils peculiar to the Cretaceous formation No. 5, are closely similar to true Tertiary types and in most of the localities the transition from No. 5 to the estuary beds is scarcely perceptible. On the North Platte, especially at Deer Creek, No. 5, which is very largely developed in this region, is not unfrequently thrust up through the overlying lignite beds, charged with its characteristic fossils. Along the bluff banks of the stream, where the beds are but slightly disturbed, the order of sequence of the strata is so perfect that I would not have been in doubt where to draw the line of separation until we came to the first seam of lignite, and even then I would have considered several beds of the Lignite formation as the upper portion of No. 5 had I not found in these lower lignite beds *Unios* and other fresh-water shells, together with impressions of leaves identical with those occurring so abundantly in the Upper Missouri and Yellow Stone Tertiary strata, and furthermore these beds on the

* The Cretaceous series of Nebraska has been divided into five formations, which for convenience have been numbered from the base in ascending order, 1, 2, 3, &c.

North Platte have now been traced continuously over the intervening country from the mouth of the Yellow Stone river to the Platte. I have ascertained the fact that the lignite beds along the North Platte are a continuation of those on the Upper Missouri, and that they extend in their full developement far up into the Wind river valley and along the Platte road to the Sweet Water mountains. As yet I have seen no indications of lignite in any of the divisions of the Cretaceous period except in formation No. 1 near the Big Sioux river on the Missouri and in a series of sandstones and shales near Fort Benton which we have referred to the same rock. As we proceed south and southwest in this region No. 1 seems to disappear gradually, and along the Laramie mountains I cannot determine its existence at all.

The geographical extension of the great Lignite Basin seems to me to be one of the most interesting questions in the geology of the West at the present time. Very little is known as yet of its limits and from the interesting facts collected by Dr. Engelmann and from other sources it must occupy a large area to the southward and westward from this point, and we already know that it extends far northward into the Hudson's Bay vicinity.

In regard to the White river Tertiary Basin its boundaries have been published with a good degree of accuracy. Its limits north of the Platte river are now well known, and as I have already stated in a former paper, one of the upper members of that basin is revealed along this river, and these, in their southern and southwestern extension, pass by a gradual transition into the Yellow Marl or superficial deposits of the Quaternary period. That the White river Tertiary beds are of later date than those of the Lignite Basin, is clearly shown by the former having been observed resting conformably upon the latter in several localities.

2. *Note on Prof. Newberry's criticisms of Prof. Heer's determination of species of North American Fossil Plants*, in a letter to Prof. Asa Gray, Cambridge.—*Dear Sir:* When I offered for publication in this Journal, the translation of part of a letter from Prof. O. Heer, concerning some fossil plants of the Tertiary, I was far from supposing that any of the statements of my learned friend would not appear satisfactory to every one interested in the study of our American Palæontology. Much less could I foresee that those statements would be construed in a manner that I do not think quite justifiable. As Prof. Heer's letter was published without his knowledge and sanction, I am forced, much to my regret, to defend his position against Dr. Newberry, a personal friend also, and a true and faithful pioneer in the field of our botanical palæontology.

I know nothing about the discussion on the Cretaceous formations and fossils, except what has been published in this Journal. And although last year, during my connection with the State Geol. Survey of Arkansas, I had the opportunity of examining well exposed strata at different stages of the Cretaceous, I was unable to find there any fossil plants, and therefore I have never seen as yet an American Cretaceous plant. Thus I can take my arguments only from the statements of Dr. Newberry himself. It is unnecessary to recall the five points in discussion.

The two first statements are, even from the assent of Dr. Newberry, satisfactorily explained by the insufficiency for exact determination of

sketches made from incomplete specimens. Nevertheless, it is but right to remark in favor of the opinion of Dr. Heer that *Populus Leuce* has not the leaves toothed as Dr. Newberry says, but only *subdenticulate*, following Unger's figure and description. Moreover, species of *Populus* bear toothed and entire leaves on the same branches.

The third assertion, about the value of the genus *Ettingshausenia*, concerns only the author of the genus and has nothing to do with the determination of the species. There was no call for a critique upon Prof. Heer on this account.

The fourth statement is the only essential one; it is: "that *excepting the so-called Credneria and Ettingshausenia, all the genera enumerated in Dr. Newberry's letter are Tertiary and not Cretaceous.*"

Nobody will consider as identical the genera *Salicites* and *Salix*, *Alnites* and *Alnus*. Then, from the eleven genera enumerated by Dr. Newberry, two only are found in the list of Hiehle, containing seventeen genera. These two genera are *Populus* and *Acer*, the true characteristic genera of the tertiary of Europe. The first is represented already in that formation by thirty-three species; the second by forty-five or more, and except the species admitted by Hiehle, not a single one has been found in the Cretaceous. From the nine other genera of Dr. Newberry which are not mentioned as Cretaceous by Hiehle, not one has been found by anybody else in that formation. Hence Prof. Heer was right to say that, except the two which he named, all the genera enumerated in the letter of Dr. Newberry were of the Tertiary and not of the Cretaceous. Every botanist without exception would have come to the same conclusion.

Therefore I cannot understand how the accusation of ignorance or of partiality could be brought against the celebrated Professor of Zurich. How it could be thought that a naturalist who has spent his life, without regard to personal interest, in the constant pursuit of his favorite science (the Botanical Palæontology of all the formations), could be supposed to ignore not only the fossil flora of the Cretaceous, but what Hiehle has published in the Palæontographia. This work is in every library and Prof. Heer has quoted it all along in the three volumes of his admirable Fossil-flora of the Tertiary! And how a man of such high moral standing as Prof. Heer, respected everywhere for his faithfulness and devotion to science, could be accused of giving his opinion or rates of determining the plants from a *judgment biased by erroneous oral testimony*, is still more inconceivable to me.

In the letter of Prof. Heer, there is not a word which could be construed as censurable or offensive to anybody. When the most learned Botanists take again and again species and genera for re-examination, new determinations, for a constant changing of names, of relations, of affinity, even with specimens of living plants; when such Palæontologists as Heer, Corda, Unger, Göppert, Braun, &c., who have on hand the largest libraries, numerous specimens, and every facility for comparing them are forced every day, either by new discoveries or by a more careful study, to acknowledge mistakes and to change their nomenclature; when the same fossil leaf is for different authors a *Populus*, a *Salix*, a *Laurus*, a *Ficus*, a *Quercus!* it cannot be expected that in America, where the science of Botanical Palæontology is scarcely born,

where the age of the strata from which the fossil plants are taken is mostly uncertain, where we have no possibility of comparing specimens, and not a single library where we can find all that has been published on palæontology, we can come at once by some kind of divination to the correct determination of a fossil species. And most commonly the examination has to be made on pieces of broken specimens of leaves, of which the general outline and the details of nervation are obliterated. We have thus to begin and to break the way, and the only means of doing it with advantage to our successors is to publish our fossil species as fast as we can get them, figuring them carefully and determining the species as well as we can without caring for any foreign opinion.

About the fifth statement of Prof. Heer, I can not admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous ought to be closely related to the European. The Tertiary of North America, of which numerous specimens have recently been collected in Mississippi, Tennessee and Kentucky, has some of the genera of Europe, *Sabal*, *Phœnicetes*, *Laurus*, &c., represented by peculiar species different from those of Europe, with a large number of *Terminalia*, *Magnolia*, two genera scarcely represented in the European tertiary. But this question cannot be examined now.

Contrary to the supposition of Dr. Newberry, it is certain that the fossil plants obtained from three different stages of the Tertiary in Mississippi, Tennessee and Kentucky, indicate a warmer climate than that of the same latitude at our time. And it is certain also that already two marine fossiliferous beds (very rich indeed) have been discovered, the one near Cairo in Illinois, by Prof. A. H. Northen; the other near Oxford, by Prof. Hilgard, and perhaps a third one in Tennessee by Prof. Safford,—three State Geologists of the three different States. I have spent some time last fall in the examination of the formations; and besides the specimens of fossil leaves which I have collected, I have now for examination the rich collection of the Mississippi University of Oxford, and the private collections of fossil plants of both Prof. Hilgard and Prof. Safford. It is from these materials partly examined that I have taken the above conclusions.

Respectfully Yours, LÉO LESQUEREUX.

Columbus, Ohio, April 3, 1860.

III. BOTANY AND ZOOLOGY.

1. *Florula Ajanensis*, by REGEL and TILING. Moscow, pp. 128, 4to, 1858. (Extr. Trans. Imp. Soc. Mosc.)

Primitæ Flora Amurensis, by C. J. MAXIMOWIEZ. St. Petersburg, 1859. (Extr. Mem. present. Acad. Im. Sci. St. Petersb.), pp. 504, tab. 10, 4to.—The first-named work, with the *Florula Ochotensis* of Trautvetter and Meyer in Middendorf's Journey, gives an account of the botany of the confines of Northeastern Siberia bordering on the Ochotsk Sea. The latter, a bulky volume, contains the first fruits of the botanical explorations consequent upon the colonization of the lower part of the Amoor River by the Russians. These districts nearly abut upon the northern part of Japan, and therefore possess for us a peculiar botanical interest. They share notably, though not as largely as Japan, in those North American types which present so curious a problem in geographical dis-

tribution. Having been supplied with an almost complete suite of the specimens upon which these works are based, through the generosity of the directors of the Imperial Botanic Garden and of the Museum of the Imperial Academy of Sciences of St. Petersburg, we wish to collate the various representative forms both with our Japanese materials and with their American relatives before we offer any definite remarks. Suffice it now to say, that the Amoor flora offers several additional species identical with peculiarly Eastern North American ones: e. g. *Acer spicatum*, *Pilea pumila*, *Asplenium thelypteroides*, and *Symplocarpus fœtidus!* Also several closely representative ones; such as *Caulophyllum robustum*, doubtless the same as the Japan plant, which in fruit answers perfectly to our *C. thalictroides*, and I still suspect not distinct from it; and *Maximoviczia Chinensis*, Rupr. (to which evidently belongs *Sphærostema Japonicum*, Gray), a close counterpart of our *Schizandra; Acer tegmentosum*, very nearly our *A. Pennsylvanicum; Hylomecon vernalis* which seems very close to our *Stylopnorum diphyllum;* and *Plagiorhegma dubium*, which has the look of a monstrous dicarpellary *Jeffersonia.* Indeed, good flowers are still wanting to make it at all certain it is not a *Jeffersonia!*

Very remarkable indeed is this division of monotypic or nearly monotypic genera or groups between Northeastern Asia and Northeastern America,—of which so extended a list can now be given,—and very suggestive is it (at least where the species are identical or nearly so) of a comparatively recent communication between the two countries. A. G.

2. *Harvey's Thesaurus Capensis*, No 2, has come to hand. The plates (26–50) are better printed and do more justice to Dr. Harvey's facile pencil than those of the first part. Among the illustrations is a new *Crotalaria*, "a sort of first fruits of the botany of Lake Ngami;" an interesting new Bixaceous genus (*Rawsonia*), with petaloid scales "evidently homologous with the crown of *Passiflora*, and with the inner stamens hypogynous, the outer perigynous"; also a fine new Scuphularineous genus, *Bowkeria.*—No. 3, just received, continues the work to plate 75. One of the plates illustrates a new genus of the Passion-flower family, with blossoms no larger than chickweed. A. G.

3. *Hooker's species Filicum, being Descriptions of all known Ferns*, illustrated with Plates. Part ix, or vol. iii, part 1. pp. 64, tab. 141–170.—This portion of this important and standard work contains the genera *Lomaria* and *Blechnum*, and excellent figures of 23 species. The most interesting Fern to us, here illustrated, is the *Blechnum doodiodes* of Hooker, known only from two specimens gathered by Douglas somewhere in Northwestern America, "probably up Frazer's River": the question is whether the plant may not prove to be a remarkable variation of *Lomaria Spicant*, which inhabits that region, and is the only Lomarioid Fern of so northern a latitude. A. G.

Part x, or vol. iii, part 2 of this important work has just come to hand. It comprises the first part of the very large and very difficult genus *Asplenium*, the reduction of which to order will be a great boon to botanists.

4. *Journal of the Proceedings of the Linnæan Society*, No. 14: Botany.—This part concludes Prof. Andersson's account of the *Salices* of the East Indies, 34 species, of which many are new. Mr. Spruce has a short article on the Piassaba Palm (*Leopoldinia Piassaba*) of the Rio Negro of the Am-

azon, the long fibrous beard of whose petioles (vestiges of the membrane which envelopes the frond in vernation) is so important for cordage, &c. On young palms this pendant beard is four feet or more in length, and reaches to the ground. Besides this useful fibre, the pulp of the ripe fruit of this palm is said to yield the most delicious of all palm-drinks, having great resemblance to cream both in color and taste. The remaining article is one by Mr. Mitten, upon various new *Musci* of New Zealand, Tasmania, and various parts of the Southern hemisphere.

No. 15, contains Observations on the Growth and time of Appearance of some of the Marine *Algæ*, by Dr. Cocks; and another of the Præcursores ad Floram Indicam, by Drs. Hooker and Thomson. The present paper is devoted to the *Balsamineæ*, amounting to about one hundred species, notwithstanding a large reduction. The great mass of the order belongs to India; the extra-Indian Balsams known being about eight in more oriental parts of Asia and Maylasia; about as many more in Africa and Madagascar, three in Europe and Siberia, and two in North America. The Indian species are "so universally and excessively prone to vary" that they have given their monographer immense trouble and small satisfaction. We may remark that our two American species and *I. Nolitangere* of the Old World seem to run together; at least, intermediate forms occasionally appear which strongly suggest a common origin. A. G.

No. 16, contains a paper by Mr. Babington on the forms or species compared with *Fumaria capreolata*, L.; another on a new Butter-tree of Southeastern Africa by T. Caruel of Florence,—the tree which Bertoloni supposed to be the shea-tree mentioned by Mungo Park, and which he described as the type not only of a new genus but of a new order. Mr. Caruel shows that the present tree is a *Combretum*, and doubtless not the shea-tree, the only grounds for considering them the same being that both belong to Africa, and both produce a kind of vegetable butter. Mr. Oliver, a young English botanist of promise, describes some new South American species of *Utricularia* (two of which are figured), with notes on *Polypompholyx* and *Akentra*, showing that Benjamin established the latter in a mistaken observation, and that the plant is an *Utricularia*. Mr. Spruce gives a very interesting account of a visit to the *Cinchona* forests of the Quitenian Andes. The *Cascarilla roja* was said to have a milky juice, a remarkable circumstance for a Rubiaceous plant. It appears that the juice as it flows from a wound is colorless, but that it turns white the instant it is exposed to the air, and in a few minutes changes to red. Mr. Moore announces the discovery in England of *Lastrea remota* (*Apidium remotum*, Braun), a form which has been confounded with *A. rigidum* and which appears to be as intermediate between *A. spinulosum* and *A. Filix-mas*, as *A. rigidum* is between the former and *A. cristatum*. Mr. Oliver continues an account of the British Herbarium now forming for the Linnæan Society; and Mr. Hogg, a note on the *Rosa rubella* of Winch., giving evidence that it is a mere variety of *R. spinosissima*. A. G.

5. *Martius, Flora Brasiliensis.*—This great work is conducted with such spirit,—now that the distinguished editor is able to devote his whole attention to it,—that three fasciculi reach us at the same moment, viz.:

Fasc. 18, part 3, is a supplement to the *Myrtaceæ* of Brazil, by Berg, of Berlin. This young botanist is thought to have elaborated the Amer-

ican forms of this great order in an able manner. Of the many new species and varieties described in this supplement, the greater part are from the Brazilian collection of Riedel, belonging to the Imperial Botanic Garden of St. Petersburg, and furnished to Dr. Berg by Dr. Regel, the accomplished curator of that establishment. To this is appended a tabular view of the geographical distribution of all the known American *Myrtaceæ*, now amounting to 1726 species, of which 1008 are nearly described by Berg (in the Fl. Brazil., and in the Linnæa), and 696 are indigenous to the Brazilian empire. The uses which the plants of the order are known to subserve in Brazil are likewise enumerated. Of four species the root, or its bark, is used medicinally: of three species the fibrous bark is turned to account, among which, from that of a *Couratari* the natives of the Uapes make clothing; the timber of *Couratari legalis*,—one of the largest trees of tropical Brazil—is so highly prized for a variety of purposes that its wasteful destruction is forbidden by law, whence the name *paos de ley*, i. e. *ligna legalia:* the leaves of several species are used medicinally for their aromatic properties combined with astringency: the flower-buds of a *Calyptranthes* are used as a substitute for the true Clove, which also is cultivated successfully in Brazil: the berries of no less than 55 species are enumerated as edible, or some of them medicinal; the most important being the *Guayavas;* five species of *Lecythis* and the famous *Bertholletia excelsa* furnish amygdaloid seeds of great richness and pleasantness (Brazil-nuts, &c.). Recent extrabrazilan genera are briefly noted. *Luma* (Bot. U. S. Expl. Exped.) is altogether overlooked, and in strictness may claim restoration, when the genera come to be revised and considerably reduced, as they probably will be. The three parts of fasc. 18, composing the *Myrtographia Brasiliensis* compose an entire volume, of 655 pages, with many plates.

Fasc. 23, is a continuation of the first fascicle (*Musci* and *Lycopodiaceæ*), and comprises the *Ophioglosseæ*, *Marattiaceæ*, *Osmundaceæ*, *Schizœaceæ*, *Gleicheniaceæ*, and *Hymenophylleæ*, by Dr. Sturm, of Nuremberg, with ten plates. One of these is a fine illustration of the rare and curious *Ophioglossum palmatum*,—a species which, by the way, Mr. Wright has lately collected abundantly in the eastern part of Cuba. Under the name of *Osmunda palustris*, Schrad., our North American *O. spectabilis*, a form of the European *O. regalis*, is figured: it appears to be rather common in the southern part of Brazil. It is interesting to remark that both this and two other more specially North American species, viz. *O. cinnamomea* and *Botrychium Virginicum*, occur on the one hand in Brazil, on the other in the Himalayas. The illustrations of the *Hymenophylleæ* are nature-printed, by the Vienna process. This does better for the fronds—so delicate in this tribe—than for the fructification.

Fasc. 24, of 215 pages, with 56 plates, contains the first part of Mr. Bentham's elaboration of the Brazilian *Leguminosæ*, including all the *Papilionaceæ* except the *Dalbergieæ* and the *Sophoreæ*. A double plate, filled with details, illustrates the structure of *Arachis*, the generic character of which, as well as of its allies, is remodelled, conformably to the observations of Dr. Niesler, as recorded in this Journal, several years ago. We perceive that the North American species referred to *Chæto-*

calyx (the fruit of the original species being unknown), belong to the allied and older genus *Nissolia*. *Chætocalyx Wislizeni*, Gray, is the same as *Nissolia platycarpa*, Benth. *C. Schottii* must take the name of *Nissolia Schotti*. A. G.

6. *J. D. Hooker's Flora Tasmania*,—the third and concluding part of the *Botany of the Antarctic Voyage* under Capt. Ross,—is now finished in parts 10 and 11, issued by the enterprising publisher, Lovell Reeve, in February last. The work makes two large quarto volumes, with 200 colored plates. Part 10 concludes the *Algæ* (by Dr. Harvey) and contains the *Lichenes* (the foliaceous ones by Mr. Babington, the crustaceous by Mr. Mitten), also a series of additions and corrections to the earlier part of the work. The letter-press of part 11 is entirely occupied, excepting the key to the genera, by the *Introductory Essay* upon the Botany of Tasmania and of Australia in general, of 128 pages, which admirable production may claim to be regarded as the most profound and far-reaching discussion of the abstruse theoretical questions bearing upon the origin and distribution of species which has ever been attempted. Although the literary composition bears some marks of haste, the subject-matter has been elaborated with great care, and in a manner at once bold, independent, and conscientious, opening new views and propounding new problems of the widest interest and, we may add, of the utmost difficulty. The more generally interesting portions of this essay are reproduced in the present volume of this Journal. A. G.

7. *Poison of Plants by Arsenic.*—It may be recollected that Professor Davy of Dublin last year reported to the Gardener's Chronicle (whence extracts were transferred to our pages*) the results of his experiments which went to show that some plants might with impunity be watered even with a saturated aqueous solution of arsenious acid; that the plants took up this arsenic and accumulated it in their tissues, to such an extent that traces of this metal were discoverable in the bodies of animals fed upon vegetables so treated. These astonishing results naturally excited enquiry. They have now been contradicted in a late number of the Pharmaceutical Journal (as we learn from the Gardener's Chronicle for March 10) by Mr. Ogston, an analytical and agricultural chemist, formerly a pupil of Prof. Graham. Mr. Ogston finds that, on watering the ground around the roots of some vigorous Cabbage plants, some months old, with a saturated solution of arsenious acid, in every trial, after two doses at intervals of three days, the plants died within the week. The same occurred with Scotch Kale, the only other plant subjected to the experiment. On testing the dead plants arsenic was detected only in the portion of the stem close to the roots, and which showed in its darkened color the marks of disease. In no case was any of the poison found in the leaves, or in the stem at more than five inches above the ground. Prof. Davy also startled the English agriculturists and medical jurists by calling attention to the fact that arsenic exists in the commercial superphosphate of lime, at least in certain kinds, coming from the iron pyrites used in the manufacture of the sulphuric acid employed in the production of the superphosphate, which arsenic, if plants may accumulate it in their tissues, would be conveyed

* Vol. xxviii, p. 443, 1859.

to the flesh of animals fed with turnips manured with such superphosphate, and so conveyed to the human system,—if not in quantity sufficient to poison, yet enough to account for the presence of arsenic in cases of death from supposed poisoning. Mr. Ogston now considers the question as to how much arsenic an agricultural crop (say of turnips) can obtain from an ordinary dressing of the superphosphate so prepared. "Take a very bad sample of pyrites to contain ·30 per cent of arsenic, and consider, as is the case, that in the manufacture of oil of vitriol, one-half of this is stopped by condensation in the flues; ·15 per cent will remain in relation to the pyrites, or about ·10 in relation to the manufactured oil of vitriol. Now suppose the superphosphate made from this acid to contain 20 per cent of it as a constituent, and that 3 cwt. are used as a dressing per acre, there will be added to this acre ·07 of a pound of arsenic, and this is to be distributed among from 20 to 25 tons of roots, giving a percentage infinitely small, and in my opinion relieving us from the necessity of the smallest anxiety on the subject. If, however, even this quantity is objectionable, the use of the Belgian pyrites, in which I have never found a trace of arsenic, would obviate all difficulty." A. G.

8. *Botanical Necrology for the year* 1859.—The following are the principal names upon this obituary record:—Some of them we have named before more briefly.

C. A. Agardh, Professor of Botany in the University of Lund, Sweden, from 1812 to 1834, when he became bishop of Carlstad; a voluminous writer upon botanical and other subjects, especially upon *Algæ*, and a distinguished and remarkable man. His earliest publication, a thesis upon the *Carices* of Scania, was published in the year 1806. He died on the 28th of January, 1859 at the age of 75 years. He was succeeded in his professorship by his son C. A. Agardh, the distinguished algologist.

Arthur Henfrey, Professor of Botany in King's College, London. The death, after a short illness, on the 7th of September last, of this amiable man and excellent vegetable anatomist, at the early age of 39 years, has has already been recorded in this Journal, (vol. 28, p. 443). In his field of research and in his knowledge of the literature of the subject, especially that of the Germans, he had no rival in Great Britain, and his death is deeply felt.

Dr. Thomas Horsfield, born in Pennsylvania, after completing his medical studies in Philadelphia he passed sixteen years in Java and the adjacent islands, in the service of the government, devoting much of his time to botanical and zoological researches; and the long remainder in a responsible position at the India House, in London. A selection only of his botanical collections was published by Mr. Brown and Mr. Bennett, under the title of *Plantæ Javanicæ Rariores*, etc.—a most important work. Dr. Horsfield died, on the 14th of July last, in the 86th year of his age.

A. L. S. Lejeune, a venerable Belgian botanist, died at Verviers, at the very close of the year 1858, in the 80th year of his age.

Thomas Nuttall, born at Settle, in the West Riding of Yorkshire, in the year 1784, may yet be reckoned as one of our own American botanists, since he came to the United States when only 22 years of age, and

made here his whole scientific career. He returned indeed to his native land in the year 1842, and took possession of a handsome estate bequeathed by his uncle, where he indulged his fondness for horticulture; but his only botanical publication in these later years was made at Philadelphia, and elaborated during a visit to this country in 1852. His writings are intimately connected with the history of North American Botany, and his personal biography is very interesting. A full account of these is given by Mr. Durand, in an excellent address delivered to the Philadelphia Academy of Natural Sciences, and in an article by Mr. Meehan in his Gardener's Monthly for January, 1860. A critical estimate of Mr. Nuttall's contributions to science must be deferred to another occasion. He died at his residence, Nutgrove, near Preston, Lancashire, on the 10th of September last, aged 75 years. A. G.

Zoological Notices.—

1. *A trip to Beaufort, N. Carolina;* by Wm. Stimpson, M.D.—The vicinity of Cape Hatteras, the most projecting point of our coast south of New York, has a peculiar interest for the student of zoology. This cape, which divides the Areniferous region* into two nearly equal parts, the Virginian and Carolinian provinces, is remarkable for the exhibition of a fauna more tropical in character than that of either of these provinces, as will be shown below. This is an evident result of its proximity to the Gulf Stream, the warm waters of which are even said to be deflected directly upon the Cape after violent southeast gales.

Beaufort, N. C., which lies several miles WSW from the Cape, in latitude 35° N. is the only convenient point of departure for explorations in the waters of the vicinity. Some account of the zoological richness of this locality was kindly communicated to me by Capt. J. D. Kurtz, U. S. A., and influenced by a desire of completing a catalogue of the shells of our Atlantic shores, and particularly of procuring and examining a species of *Lingula* said to be found on the southern coast, I undertook its exploration in the month of March last, in company with my friend Mr. Theodore Gill. The harbor of Beaufort is situated at one of the southern outlets of Pamlico Sound, where it joins Bogue Sound. It is shallow and much obstructed by extensive shoals. Centre-board boats only can be used, except in the deeper channels, which are mostly narrow. The bottom is generally sandy, but that of the deep channels is shelly, and that of the shallower channels often muddy. Outside the harbor and off the coast, the depth never exceeds eight fathoms within a few miles of the land, with a variable bottom, sometimes "sticky" or clayey. These bottoms were all pretty thoroughly raked with the dredges. The beaches were also examined for those bivalve shells which perforate the sand to a

* The eastern coast of the United States may be conveniently divided into three regions, viz.—the Rupiferous, Areniferous, and Coralliferous, named from the character of the shores. The Rupiferous or rocky region extends from our northern limit to Cape Cod, or Long Island. The Areniferous or sandy region,—in which there are no rocks whatever and scarce even a pebble except where human agency has been at work—reaches from Long Island to North Florida. The Coralliferous region is characterised by the presence of reef-coral, and includes the peninsular of Florida. Each of these regions has its peculiar fauna, the distinctness of which, enhanced by the two great causes of difference of latitude and diversity of ground, is too well known to require further comment here.

depth beyond the reach of the dredge and are only exposed by the eroding action of the breakers during heavy storms. In following these beaches we observed a decided increase of the tropical character in the shells as we proceeded eastward toward the Cape.

In order to show the character of the fauna and the results of our explorations I have given below a catalogue of the Mollusca and higher Crustacea which occurred during our short stay, which seems to be the most concise and satisfactory method. A few prefatory remarks of a general character will not be out of place. Geologists will be interested to notice the occrrence of several species hitherto known only as Tertiary fossils, such as species of *Axinaea*, *Lucina*, *Astarte*, *Amphidesma*, *Tellidora*, *Myalina*, *Panopaea*, *Entalis*, and *Columbella*. These were found either alive or in such condition as showed them to be recent shells, which would doubtless have been found alive upon further search. The occurrence of *Myalina subovata* is interesting, although our specimens of this species, as of *Amphimesma constricta*, are not certainly recent, being only single valves. Of the beautiful *Tellidora lunulata* we obtained several living examples, some attaining a length of nearly two inches. Among the shells of a tropical character several species will be noticed which have not hitherto been found north of the West Indies, and do not exist upon the South Carolina coast. The large *Cassis* to which we have applied the name *C. cameo* is identical with the common cameo-shell of the Bahamas, which usually figures in collections under the name of *C. madagascarensis*. Of this we obtained several fine specimens on the beaches, none living however.

In the following catalogues all species of which the identifications is at all doubtful are indicated by the mark of interrogation.

Mollusca.

Cynthia vittata.
Molgula sp.
Ascidia sp.
Lingula pyramidata.
Anomia ephippium.
Ostrea virginiana.
" equestris.
Plicatula depressa.
" sp.
Lima scabra?
" sp.
Pecten dislocatus.
" concentricus.
" nodosus.
Axinæa charlestonensis.
" sp.
Arca Holmesii.
" americana.
" cælata.
" lienosa.
" noæ?
" ponderosa.
" incongrua.
Nucula proxima.
Yoldia limatula.
Leda acuta.
Pinna squamosissima.
" carolinensis.
Avicula atlantica.
Modiolaria lateralis.
Modiola plicatula.
" americana.
" tulipa?
Mytilus edulis.
" cubitus.
Chama arcinella.
" macrophylla.
Cardium magnum.
" isocardia.
" muricatum.
Liocardium serratum.
" Mortoni.
Lucina crenulata.
" cribraria?
" edentula.
" strigilla.
Felania sp.
Diplodonta? punctata.
Lepton lepidum.
Astarte lunulata.
" undulata?
Cardita tridentata.
Mercenaria violacea.
" Mortoni.
" notata.
Trigona sp.
Venus rugosa?
Chione cancellata.
" pygmæa?
Callista gigantea.
" maculata.
Dosinia discus.
Lucinopsis sp.
" n. sp.
Petricola pholadiformis.
Raëta canaliculata.
" lineata.
Mactra oblonga.
" similis.
" lateralis.
Donax variabilis.
Cumingia tellinoides.
Semele orbiculata.
" reticulata?
Amphidesma constricta.
Abra æqualis.
Tellina alternata.
" fausta?

Tellina polita.
" tenera.
" versicolor?
" tenta.
" iris.
" constricta.
" sp.
" n. sp.
Strigilla carnaria.
" flexuosa.
Tellidora lunulata.
Solen ensis?
" viridis.
Siliquaria gibba.
" bidens.
Solenomya velum.
Mya arenaria.
Corbula contracta.
Myalina subovata, *Con.*
Panopæa americana.
Saxicava distorta.
Gastrochæna sp.
Lyonsia hyalina.
" sp.
Pandora trilineata.
Pholas costata.
" truncata.
Pholadidea cuneiformis.
Teredo sp.
Polycera sp.
Actinodoris? sp.
Utriculus canaliculatus.
Bulla solitaria.
Tornutella puncto-striata.

Chiton apiculatus.
Entalis pliocena.
Dentalium sp.
Crepidula unguiformis.
" fornicata.
" convexa.
Clypidella pustula.
Fissurella alternata.
Zizyphinus sp.
Turbo crenulatus.
Littorina irrorata.
Scalaria Humphreysii.
" turbinata.
" lineata.
" novangliæ?
" multistriata.
Solarium granulatum.
Vermetus radicula.
Cerithium sp.
Bittium nigrum?
" Greenii.
" sp.
Triforis nigrocinctus.
Odostomia seminuda.
" impressa.
Turbonilla interrupta.
" sp.
" sp.
Obeliscus crenulatus.
Eulima oleacea.
Catinus perspectivus.
Natica pusilla.
Neverita duplicata.
Volva uniplicata.

Pleurotoma cerina.
Mangelia rubella.
" filiformis, *Holmes.*
Oliva litterata.
Olivella mutica.
Columbella avara.
" ornata.
" lunata.
" n. sp.
Dolium galea.
Semicassis granulosa.
Cassis cameo, *Stm.*
Purpura floridana.
Murex spinicostatus.
Nassa obsoleta.
" trivittata.
" vibex.
" ambigua?
Cerithiopsis terebralis.
" ? n. sp.
Acus dislocatus.
" concavus.
Busycon pyrum.
" canaliculatum.
" carica.
" perversum.
Cancellaria reticulata.
Fasciolaria gigantea.
" tulipa.
" distans.
Ranella caudata.
Strombus alatus.

Crustacea Decapoda.

Libinia canaliculata.
Pelia mutica.
Leptopodia calcarata.
Cryptopodia granulata.
Cancer irroratus.
Menippe mercenaria.
Panopeus Herbstii.
Pilumnus aculeatus.
Platyonichus ocellatus.
Lupa hastata.
" Gibbesii.
" spinimana.
Gelasimus pugilator.

Ocypoda arenaria.
Pinnotheres ostreum.
" maculatus.
Pinnixa cylindrica.
" sayana.
" chætopterana.
Persephona punctata.
Lithadia cariosa.
Hepatus decorus.
Calappa marmorata.
Porcellana ocellata.
" sociata.
Euceramus prælongus.

Hippa talpoida.
Lepidops scutellata.
Eupagurus pollicaris.
" longicarpus.
" annulipes.
Callianassa major.
Crangon septemspinosus.
Alpheus intermedius.
Virbius pleuracanthus.
Palæmonopsis carolinus.
Penæus brasiliensis.
" constrictus.

In the collection made at Beaufort and now deposited in the Smithsonian Institution a considerable number of new genera and species occur. We add descriptions of two of the most interesting.

LINGULA PYRAMIDATA. Shell greenish-white, elongated-ovate, convex, regularly tapering from the middle to the summit with an outline very slightly convex; also a little tapering toward the extremity, which is less than two-thirds as wide as the middle, and subtruncate with broadly rounded corners. Surface smooth and glossy; lines of increment inconspicuous, but sufficiently distinct near the margins; two or three of them however at irregular intervals sometimes projecting more strongly, indi-

cating epochs in the growth of the shell. Marginal setæ of the mantle well developed, those on either side at the extremity longer than the rest, equalling in length one-third the width of the shell. There are two black spots on the margin of the mantle at the extremity. Peduncle in life three times as long as the shell, thick, (one-third width of the shell,) at its point of attachment, but rapidly tapering and becoming very slender and hyaline, with an opaque axis or central cord; extremity glutinous and covered with adhering sand. Length of the animal 3·5; length of shell, 0·92; width of shell at the middle 0·35; at extremity, 0·21; half way between middle and summit, 0·26 inch.

This animal was found imbedded in the weedy sand at low water mark, on the occasion of one of the extraordinarily low tides which occur at the equinox. It lives in a perpendicular position with its peduncle deeply penetrating the sand, and its shell scarcely projecting above the surface at its extremity. When drawn out and placed in a vessel of sea-water it showed its uneasiness by snake-like gyrations of the peduncle, which, far from being a simple stem for attachment, is a powerful muscular organ, filling the function of the foot of the Lamellibranchiata. Mr. Peale informs me that this part in the *Lingula anatina* forms a favorite article of food among the Fiji Islanders. Our Lingula appears to be not uncommon near Beaufort, as several specimens were found during a single retreat of the tide. It is interesting as being the first species* of this most ancient genus described from the Atlantic ocean. The other recent species, ten in number, are all inhabitants of the Pacific.

EUCERAMUS, nov. gen. fam. *Porcellanidae.* Body subcylindrical. Carapax elongate-subrectangular, twice as long as broad; sides parallel. Front prominent, tridentate. Eyes minute, longitudinal, projecting a little beyond the orbits which are very incomplete, consisting only of the concave superior margin. Antennulæ placed immediately beneath the eyes; peduncle anteriorly bidentate. Antennæ large; mobile part nearly as long as the carapax and arising from the inner or superior side of the small coxal joint, thus being in contact with the eye at base. The outer maxillipeds are of the form usual in Porcellana, but the sternal piece to which they are attached is very large, nearly as long as broad, triangular in front, and truncate behind. Chelipeds small, subcylindrical, much shorter than the carapax; hands weak. Ambulatory feet subcylindrical; dactyli curved, setose, nearly as long as penult joint. Abdomen narrow, particularly in the males; appendages as in Porcellana.

This aberrant type should be referred to the Porcellanidea, notwithstanding its greatly elongated form, which would lead one to refer it at first sight to the Hippidæ or Raninidea.

EUCERAMUS PRÆLONGUS. Carapax regularly curved like a segment of a cylinder, above glabrous, and minutely striated transversely; striæ curved forward at the sides. Inter-orbital front one-third the width of the carapax, tridendate, teeth slender, pointed, median longest. Hands externally scabrous, or small-tuberculose and setose; fingers as long as the palm and not gaping. Ambulatory feet of the second and third pairs as long as the chelipeds; those of the first pair smaller. Length about three-fourths of an inch. Dredged on shelly ground in 4 to 8 fathoms.

* We understand that there is a specimen of Lingula from the coast of South Carolina in possession of Prof. Agassiz. Not having access to this specimen, we are unable to say whether it be identical with ours or not.

IV. METEOROLOGY AND ASTRONOMY.

1. *Abstract of Meteorological Observations, made during the year* 1859 *—with the average of seven years—at Sacramento, Cal.*, lat. 38° 34′ 41″, long. 121° 29′ 44″; by THOMAS M. LOGAN, M.D.

1859.	January.	Feb'y.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.	7 years average.
Barometer.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,.....	30·491	30·332	30·385	30·371	30·171	30·211	30·066	30·080	30·126	30·270	30·326	30·368	30·266	30·243
Minima,.....	29·438	29·626	29·771	29·698	29·744	29·605	29·747	29·731	29·725	29·694	29·791	29·788	29·697	29·706
Mean,.....	30·211	30·026	30·151	30·004	29·976	29·836	29·917	29·892	29·927	29·943	30·040	30·167	30·008	30·004
Thermometer.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,.....	57·00	62·00	64·00	76·00	80·00	96·00	87·00	85·00	82·00	83·00	68·00	53·00	74·42	75·80
Minima,.....	34·00	39·00	40·00	40·00	53·00	61·00	60·00	58·00	56·00	49·00	42·00	34·00	47·17	46·70
Mean,.....	44·87	50·49	51·47	57·11	63·03	74·85	69·07	67·16	65·89	63·28	54·05	43·52	58·73	59·36
Thermometrograph.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,.....	58·00	63·00	66·00	78·00	84·00	102·00	92·00	89·00	85·00	87·00	70·00	54·00	77·33	77·99
Minima,.....	30·00	34·00	36·00	36·00	45·00	51·00	51·00	52·00	50·00	43·00	37·00	28·00	41·08	41·46
Range,.....	28·00	29·00	30·00	42·00	39·00	51·00	41·00	37·00	35·00	44·00	33·00	26·00	36·25	36·53
Force of Vapor.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,.....	·362	·436	·410	·524	·522	·942	·677	·564	·581	·524	·529	·334	·534	·529
Minima,.....	·120	·173	·078	·133	·136	·296	·338	·350	·226	·186	·189	·087	·193	·197
Mean,.....	·231	·289	·257	·321	·363	·503	·469	·451	·410	·367	·347	·230	·353	·357
Relative Humidity.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.
Maxima,.....	93·00	94·00	92·00	87·00	82·00	82·00	77·00	82·00	84·00	85·00	94·00	92·00	87·00	87·81
Minima,.....	32·00	60·00	19·00	30·00	19·00	18·00	36·00	29·00	25·00	32·00	51·00	30·00	31·75	31·26
Mean,.....	78·05	78·73	68·32	68·90	64·81	59·51	66·95	68·68	65·61	64·41	82·32	80·30	70·55	69·95
Number of clear days,..	7	3½	11	10½	14½	20½	22½	25½	16½	16½	4	10½	162½	176 7–15
No. cloudy and foggy d'ys	17	8½	6	14½	12½	9½	7½	5½	11½	14½	10	15½	132½	126 1–15
No. rainy days,.....	7	16	14	5	4	0	1	....	2	....	16	5	70	63 1–15
Quantity of clouds,.....	6·3	5·5	3·7	2·9	2·2	0·5	0·7	0·4	1·4	1·3	5·7	4·1	2·9	3·3
Quantity of rain and fog.	0·964	3·906	1·637	0·981	1·037	0·000	0·030	0·000	0·025	0·000	6·485	1·834	16·899	16·927
1st d'ys & 2d force N. wind	11½	2·3	4½	1·8	8½	3·2	5½	2·5	6	2·9	3	1·0	1	0·6	1½	0·6	7½	2·2	10½	1·4	7½	1·4	14½	1·4	81½	1·7
" " " N.E. "	6	2·0	1	1·7	1	1·2	2½	2·0	1½	1·1	1½	0·4	0	0·0	½	0·3	0	0·0	2	1·2	1½	1·2	5	1·7	21½	1·1
" " " E. "	1½	1·3	½	1·0	1	0·8	1	2·0	½	1·7	½	0·3	½	0·7	0	0·0	1½	0·5	2½	1·4	2	1·7	2	1·8	13½	1·1
" " " S.E. "	6½	2·8	8	3·1	5	3·2	3½	1·9	4	1·8	2½	1·1	4½	2·2	5½	2·3	4	0·8	3½	1·6	8½	2·7	7	2·1	63	2·1
" " " S. "	2	3·2	8½	3·1	4	2·6	10½	2·9	9½	2·6	11	2·2	18	2·5	15	2·5	7½	2·5	5½	2·0	4	3·0	1	2·0	95½	2·6
" " " S.W. "	1½	2·5	3½	1·8	4½	3·2	3	3·0	3½	2·3	6½	1·7	4	2·3	4½	2·2	5½	2·1	2½	1·0	2	1·7	0	0·0	41	1·9
" " " W. "	0	0·0	2	1·8	4	2·1	3	2·7	2½	1·5	21½	1·3	3	2·6	3	1·6	3	1·5	1	0·3	1½	1·0	½	0·7	26	1·4
" " " N.W. "	2½	3·1	0	0·0	2½	3·6	1½	3·0	3½	2·6	2	0·7	½	0·7	1	0·7	1½	1·8	3½	3·3	3	2·1	1	2·3	22½	1·8

General Remark.—The climatic feature of predominant interest in California lies in the rains of winter, which, although they fell in the early part of the present season in ample abundance for agricultural purposes, nevertheless, in their subsequent diminution, confirm the opinion expressed by us in former remarks, that the cultivator of the earth cannot depend with any certainty upon them alone, but must be prepared to supply their deficiency, whenever it occurs, by irrigation—for which expedient no other country, perhaps, is better adapted, both as regards soil and climate, as well as facilities of commanding water.

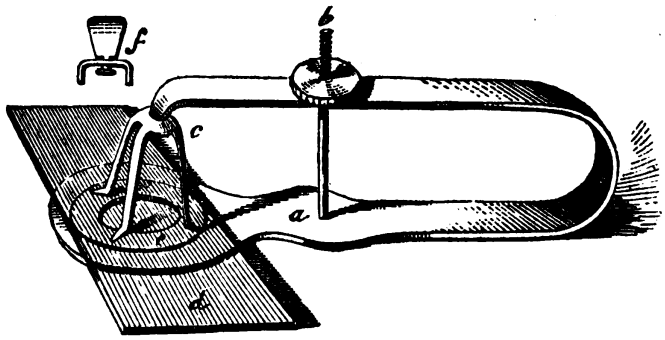
2. *Daylight Meteor of Nov. 15th*, 1859.—Of this remarkable body Professor Loomis has already published an interesting account at p. 137 and 298 of this volume. The meteor being one of the most brilliant on record deserves the fullest possible investigation, and we are glad to find in the Journal of the Franklin Institute of Philadelphia, for March and April, a valuable paper thereon by Mr. Benj. V. Marsh of that city, giving an extensive series of observations which he has collected, together with his deductions therefrom.

The meteor was seen in full sunshine, as a large ball of fire, from Salem, Mass., to Petersburg, Va. Its path was probably inclined to the vertical about 35°, and the direction of its motion was nearly west. Its velocity was very great, perhaps full 30 miles per second, and the meteor appears to have become luminous when more than 100 miles above the earth. During its brief passage of two or three seconds, it exploded several times, with reports which were loud and violent. These reports or detonations made two series, the whole occupying only half a second of time, the individual sounds being distinguishable because of the different distances they had to travel to reach the ear. The column of smoke resulting from the explosions was nearly a thousand feet in diameter, and its base was vertical about four miles north of Dennisville, N. J. The immense volume of smoke or substance of the meteor, dissipated by its excessive heat, shows that the body was of very considerable magnitude. The meteor's path would strike the earth near Hughesville, on the northwestern boundary of Cape May County, N. J., in which vicinity, or a few miles further west, it is probable that fragments may yet be found. Will nobody look for them?

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Probable Origin of Flint Nodules in Chalk.*—Dr. G. C. Wallich, Surgeon in the Indian army, has published (in the Quart. Jour. Microscopical Sci., No. xxx, p. 36) an interesting paper on the siliceous organisms found in the digestive cavities of Salpæ—embracing under this head the whole molluscoid tribes that frequent the open sea in shoals and live upon the microscopic organisms it contains. These creatures are in their turn the food of whales. In the digestive cavities of the Salpæ the siliceous shields of Diatomaceæ, &c., are freed of all or nearly all their soft portions, and these minute organisms aggregate into masses which in the whales are further aggregated and in the form of coprolites fall in vast numbers upon the pulpy cretaceous strata of Foraminiferæ, &c., now known to form the bottom of the ocean in many places, imbedding themselves there as nodules similar to, certainly, if not identical with, the flint nodules in the Chalk.

2. *New form of Compressor for use with the Microscope*, (in a letter to Prof. G. C. SCHÆFFER).—*My Dear Sir*,—At your request I enclose a sketch of the little instrument that I use as a substitute for the ordinary compressor, in mounting objects for the microscope.



The frame *a*, is made from one piece of hard brass, possessing sufficient elasticity for the purpose,—held in place and adjusted by the milled nut on the screw *b*. This screw is firmly riveted in the frame at *a*, and plays freely in a slot at *b*. The swinging tripod, (also cut from one piece,) is loosely riveted to the bent end of the frame as shown in section at *f*, having sufficient play in the collar to adjust itself to any inequality in the slide or the cover.

The centre of the frame, at *c*, under the tripod, is pierced with a hole of sufficient size to see the object to be mounted, so that the pressure from the screw can be adjusted without injury to the object. The sketch shows a slide, *d*, and cover, *e*, in place.

For mounting objects dry, or for covering cells I find it useful as it enables me to hold the cover securely while I have access to its entire edge, and can turn it in every direction to apply the cement. For mounting objects in balsam which require very thin covers, say ·01 to ·005 of an inch, such as the silicious epidermis of plants and other test objects, I add to the pierced hole in the frame a circular shield of glass, a little less in diameter than the cover, and of the thickness of an ordinary slide, imbedding but a small portion of its thickness in the brass. Then, after placing the cover on the balsam, and spreading it by heat, I put the slide cover down in the frame, and apply a pressure to drive off the superfluous balsam. The raised surface of the glass shield, keeps the exuding balsam from the frame, and at the same time prevents any bulging of the thin glass at the centre. Dried in this way, under pressure, it is easily taken from the frame and cleaned. I have found it a very simple and satisfactory way of perfecting the mounting of difficult test objects.

Other modifications of this little instrument will readily sugest themselves to you. It is, to me, a great convenience to have a dozen of them at hand, of various sizes, as I can thus get my slides thoroughly seasoned under a perfectly uniform pressure—and I can have twenty of them made for the cost of one English compressor.

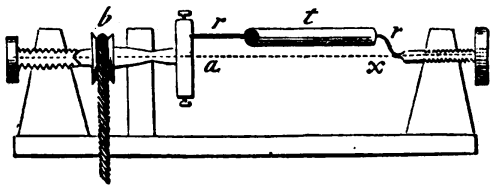
My friend, Mr. McAllister, Optician, 728 Chestnut St., Philadelphia, made them for me from a drawing, very neatly and accurately, at seven dollars and fifty cents per dozen ($\$7\frac{50}{100}$).

Very truly, your friend, S. MORTON CLARK.

Washington, Jan. 4th, 1860.

3. *On Contraction of the Muscles, induced by Contact with Bodies in Vibration;* by O. N. Rood, Professor of Chemistry in Troy University.—Some time since, when grinding a slide for microscopic purposes, as the strip from time to time accidentally came into vibration, I experienced, in the hand holding it, a numbness, and, at times, an absolute inability to relax the grasp. It seemed as though an involuntary contraction of the muscles had been effected by the vibratory action.

For the examination of this matter, the apparatus seen in the woodcut



was devised: b is made to revolve at a rate of from 50 to 60 revolutions per second; $r\,r$ is a rod of iron placed excentrically and so that the distance $r\,a$ is equal to $\frac{1}{8}$th of an inch. To protect the hand from blisters, the brass tube t encloses the rod, fitting it very loosely.

When the hand is laid on this sheath, the rate of rotation being between 40 and 60 revolutions per second, a feeling of numbness is first perceived; the muscles involuntarily contract with considerable force, and the hand grasps the sheath tightly. As long as this rate of revolution is kept up, it is almost impossible, by an effort of the will, to relax the grasp, just as is the case with the electro-magnetic machine employed for medical purposes. The sensations, indeed, resemble those occasioned by the use of this apparatus, and usually extended as high as the elbows. At the termination of these experiments no particular inconvenience was experienced, although the sensations produced by the higher rates of vibration were painful.

Experiments were made on different parts of the arm and hand. The results obtained differed in degree rather than in kind.

The resemblance of the symptoms and sensations produced by electricity and mechanical vibration, is at least singular, and may eventually throw some light on the method in which electricity causes contractions in the muscles.

Troy University, Feb. 3d, 1860.

4. *Large Object-Glass.*—Messrs. Alvan Clark & Sons, of Boston, have completed on their own account, an object-glass with a focal distance of sixteen feet, and clear aperture of *twelve inches*. It has a nice defining power, and Mimas, the nearer satellite of Saturn, was seen with it Feb. 14th and March 2d and 4th.

5. *Boyden Premium.*—Uriah A. Boyden, Esq., of Boston, Mass., has deposited with the Franklin Institute, Philad., the sum of one thousand dollars, to be awarded as a Premium to "any resident of North America, who shall determine by experiment whether all rays of light, and other physical rays, are, or are not transmitted with the same velocity."

The conditions are given in an advertisement at the end of this Number.

6. *Geological Survey of California.*—We learn that a bill for securing the geological survey of California is now under consideration and if not so already, is likely soon to become a Law. So important an act must meet with the approval of every one interested in the material prosperity of the golden state: while science has much to expect every way from the proper discharge of such a commission.

Book Notices.—

7. *Elements of Chemical Physics;* by Josiah P. Cooke, Jr., Erving Professor of Chemistry and Mineralogy in Harvard University. 8vo. pp. 739. Boston, Little, Brown and Company, 1860.—We cannot state the nature and scope of this important contribution to our scientific literature better than by quoting the following paragraphs from the Author's preface.

"The history of Chemistry as an exact science may be said to date from Lavoisier, who first used the balance in investigating chemical phenomena, and the progress of the science since his time has been owing, in a great measure, to the improvements which have been made in the processes of weighing and measuring small quantities of matter. These processes are now the chief instruments in the hands of the chemical investigator, and it is evidently essential that he should be familiar with the causes of error to which they are liable, and should be able to determine the degree of accuracy of which they are capable. All this, however, requires a theoretical knowledge of the principles which the processes involve, and a chemical investigator who, without it, relies on mere empirical rules, will be exposed to constant error. This volume is intended to furnish a full development of these principles, and it is hoped that it will serve to advance the study of chemistry in the colleges of this country. In order to adapt the work to the purposes of instruction, it has been prepared on a strictly inductive method throughout; and a student who has acquired an elementary knowledge of mathematics will be able to follow the course of reasoning without difficulty. So much of the subject-matter of mechanics has been given at the beginning of this volume as was necessary to secure this object; and for the same reason, each chapter is followed by a large number of problems, which are calculated, not only to test the knowledge of the student, but also to extend and apply the principles discussed in the work. Regarding a knowledge of methods and principles as the primary object in a course of scientific instruction, the author has developed several of the subjects to a greater extent than is usual in elementary works, solely for the purpose of illustrating the processes and the logic of physical research. Thus, the means of measuring temperature and the defects of the mercurial thermometer have been described at length, in order to show how rapidly the difficulties multiply when we attempt to push scientific observations beyond a limited degree of accuracy; so also the history of Mariotte's law has been given in detail, for the purpose of illustrating the nature of a physical law, and the limitations to which all laws are more or less liable; the condition of salts when in solution, and the nature of supersaturated solutions, have in like manner been fully discussed as examples of scientific theories; and, lastly, the method of representing physical phenomena by empirical formulas and curves, which are the preliminary substitutes for laws, has been illustrated in connection with Regnault's experiments on the tension of aqueous vapor."

After advising the student to study the details of science from original memoirs rather than from digests, and enumerating the chief sources from which he has drawn his facts and illustrations, the Author announces his design of following the present volume with two others.

"Although the present volume is a complete treatise in itself of the principles involved in the processes of weighing and measuring, it is also intended as the first volume of an extended work on the Philosophy of Chemistry. The arrangement of the chapters and sections has been adopted with this view, and the inductive method begun in this volume will be extended through the whole work. The second volume will treat of the theory of Light in its relation to Crystallography (including Mathematical Crystallography), and also of

Electricity in its relations to Chemistry. The third and last volume will be on Stoichiometry and the principles of Chemical Classification. This volume is now in preparation, and will be published next."

We have read, or studied, the larger portion of Prof. Cooke's present volume, with care, and, are happy to add, with much satisfaction. It is a more elaborate and thorough discussion of the subjects on which it treats than has before appeared in any text-book. All the important propositions are mathematically demonstrated in a simple but thorough manner.

The volume demands and must receive exact and searching study, and any chemical teacher who intends to employ it as his class book will find it capable of the same treatment which he has been accustomed to regard as peculiar to mathematical text-books. The French units of weight and measure are employed exclusively, and a collection of tables (21 in number) is added for scientific reference and for the convenience of the student and teacher in solving the problems (420 in number) which are appended at the close of each principal subdivision of the subject. The whole subject matter of the volume is treated under five chapters, viz., I. Introduction, II. General Properties of Matter, III. The Three States of Matter, IV. Heat, and, V. Weighing and Measuring.

The mechanical execution of the work is beautiful, and the press seems to have been very carefully supervised.

One circumstance in connection with this work cannot fail to attract the attention of all teachers of Chemistry in American Colleges, namely, that a revision of our whole scientific curriculum is demanded in most of our higher Institutions in order to admit of the expansion demanded by the introduction of such a treatise as Cooke's Chemical Physics into the course of study. Such a change Prof. Cooke has been able to effect since his appointment at Cambridge, and now his chemical teachings fill a course of recitations and lectures commencing in the Sophomore year and covering two or three years. This is a great change in the policy of a college where this subject was formerly a by-word, and it offers every encouragement for efforts to secure a similar change in other leading colleges. For this reform, as for the high scientific character of his present work, Prof. Cooke will receive the hearty thanks and esteem of all teachers in this department of science.

8. *Smithsonian Miscellaneous Collections.*—Catalogue of the Publications of Societies and of the periodical works in the Library of the Smithsonian Institution, July 1, 1858. *Foreign works.* Washington, 1859. pp. 259, 8vo, with a Supplement.—The arrangement of this catalogue is geographical, commencing with Scandinavia and ending in Europe with Great Britain. It is followed by an alphabetical Index to Learned Societies, and another of miscellaneous publications, chiefly Journals. The Smithsonian Library possesses, as appears from this list, the Transactions in full or in part of 501 Institutions and Learned Societies, and series, more or less complete, of 254 Scientific Journals exclusively foreign. The domestic publications form the subject of another catalogue. It is a most valuable aid to the student in ferreting out the often enigmatical references constantly found in books of science, and for determining the probability of being able to verify such references by a visit to Washington, or by correspondence. Only those who have undertaken researches can appreciate the value of such an aid.

9. *The New American Cyclopædia.* Vol. IX. New York, D. Appleton and Co., 1860.—This volume contains some fifty articles on zoölogical subjects. Some of them are of considerable length and show marks of much care and labor. The sections on Herpetology and Ichthyology are especially to be commended for the plan on which they are written. Instead of entering into trivial details the author has confined himself to giving the ways in which the best authors have arranged the divisions of Reptiles and of Fishes. The subject is thus properly kept within scientific limits. The more popular treatment is reserved for particular animals or for the minor zoölogical divisions. Thus, there is a very interesting and acceptable article on the Horse, and another on Insects. Some of the definitions are not quite precise, and terms are, in one or two instances, somewhat loosely applied: *e. g.*, speaking of the nervous system of an insect as a "brain and spinal cord." But these are of little consequence compared with the general good judgment and accuracy displayed. Nor is it too much to say that these contributions are far better than those commonly met with in Cyclopædias. It is understood that the zoölogical articles are from the pen of Dr. Kneeland, Secretary of the Boston Society of Natural History.

Other articles on scientific topics in this volume worthy of particular mention are Iron and the Iron manufacture by Mr. Hodge, and that on Isomerism by Dr. F. H. Storer, who is also the author of an excellent article on Chemistry in a former volume.

Among the living scientists of whom biographies are given we notice Herschel, T. S. Hunt, and C. T. Jackson.

10. *Cavendish Socy's. Ed. of Gmelin's Hand Book of Chemistry*, vol. XIII.—This is vol. VII of Organic Chemistry embracing organic compounds containing sixteen and eighteen atoms of carbon. The Cavendish Society announce that they will give only one volume for 1857, '58, and '59. This action of the Society is not well calculated to content their subscribers or to invite new recruits. But we can well imagine that the Council have found Gmelin a heavy load and that they are anxious to close it before undertaking any new publication; meanwhile let us hope that "Rose's Analytical Chemistry," long since undertaken, will not be unnecessarily delayed.

11. *Lieber: Geology of South Carolina.* Report IV. 1859. 8vo, pp. 194. Columbia, S. C. Apr. 2, 1859.—We have already announced (this vol., p. 287) the unfortunate discontinuance of the South Carolina Survey from an unwise withholding of the requisite appropriation of money. Dr. Leiber in this Report sums up his results and presents some general discussions on Metamorphism, a subject certainly of much importance in both a scientific and economic view in South Carolina. The Geognostic maps of Anderson and Abbeville districts and the "Industrial map" of the State, are very neatly printed in colors by Colton of New York.

Some of the most important subjects of this Report have already been laid before our readers, *e. g.*, the evidence of a change of level on the coast of South Carolina and the mineralogical details.

12. *Fundamental Ideas of Mechanics and Experimental Data*, by A. Morin. Revised, translated and reduced to English Measure, by Joseph Bennett, Civil Engineer. New York, Appleton and Co., 1860.—This

work is designed to illustrate the practical application of mechanics to the construction of machinery, draught vehicles, ship building, &c., and contains experimental data of great value in calculating the resistance to be overcome in the form of friction, vibration and wear and tear of machinery and other structures. The work is of great value to practical engineers and machinists and we rejoice to see it presented to the public in an English dress.

13. *Gangstudien, oder Beiträge zur Kenntniss der Erzgänge*, Dritter Band, drittes und viertes Heft.—The continuation of this valuable work, edited by Prof. Cotta and Herrmann Müller, contains a very important paper by Müller, on the relation between *mineral springs* and *mineral veins* in Northern Bohemia and Saxony; also, an extended article on *itacolumite*, its associates, and the metal bearing characters of the same, by Oscar M. Leiber, late State Geologist of South Carolina.

Ferrel, Wm., A.M.: The motions of fluids and solids relative to the Earth's surface; comprising applications to the winds and currents of the Ocean. From the Math. Monthly, Vols. I. and II. New York, Ivison and Phinney. 1860. pp. 72. [A very important discussion an abstract of which, prepared by the Author, we shall present at an early day].

Wilkes, Capt. Chas, U.S.N.: On the circulation of the Oceans. Phila., 1859. pp. 24, 8vo.

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Stevenson, D. and T.: Answer to Sir David Brewster's Reply to Messrs. Stevenson, on Sir D. B.'s memorial to the Treasury. Wm. Blackwood and Sons. 1859. 8vo. pp. 16.

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Durand, Elias: Memoir of the late Thomas Nuttall, (from the proceedings of the Am. Phil. Soc.). 8vo, pp. 19. 1860, Phila.

Royal Society, London, Proceedings, Nos. 35, 36 and 37, from May 26, 1859, to January 12, 1860. 8vo, pp. 1–234.

Hall, James: Contributions to the Palæontology of New York, being some of the results of investigations made during the years 1855–1858, being part of the 12th Annual Report of the Regents of the University of the State of New York. March 15th, 1859. Albany, 8vo, pp. 110.

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Isaac Lea: Observations on the genus Unio, &c., Vol. 7, Part II, with 25 plates, 4to, read before the Acad. Nat. Science. Philadelphia, 1859.

Haidinger (Wm.): Ansprache gehalten am Schlusse des Ersten decenniums der K. K. Geolg. Reichanstalt in Wein, 22 Nov., 1859. 8vo, pp. 38. 3 plates.

(The same). *Die Rutilkrystalle von* Graves Mount in Georgia. 5 January, 1860.

J. B. Schneider und Ernst Hartig: Untersuchungen über dir Heizkraft der Steinkohlen sachsens: Leipzig, W. Engelmann. 1860. 4to, pp. 509. Taf. IV. This is the third and concluding part of the great government research on the Coals of Saxony. Dr. Geinitz prepared the volume on their Geology and Prof. W. Stein

is the author of the Chemical volume. The three parts form by far the most complete research on this important subject hitherto published.

Dr. J. Lamont: Magnetische unterschungen in Nord-Deutschland, Belgien, Holland, Dänemark, München. 1859, 4to, pp. xlv, Tab. ix.

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Ludwig Seidel: Die Theorie der Lichterscheisung des Saturn München, 1859, 4to, pp. 102, 1 chart.

Von Martius (Dr. Carl): Eine Rede zur feifer des Akademischen Saccularfestes am. 29 März, 1859. München. 4to pp. 74.

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ERRATA.

Page 6, l. 14 from top, for "1847," read "1845."—P. 7, l. 7 from top, for "Regent," read "Honorary Member."

P. 76, l. 27 from top, for "produced," read "protected."

P. 53, line 13 from bottom, for "$t = 1, 2, 3$," read "$t = 0, 1, 2, 3$."

P. 136, foot-note 3, insert "om" before Stoffernes.

P. 146, line 18 from bottom, in table, for "92·3," read "72·3."

p. 148, in formula (47), for $\frac{de}{dt}$ read $\frac{dc}{dt}$.

p. 149, l. 19 from bottom, for γ' read γt.

P. 312, foot-note 1, for "this memoir," read "the memoir of which this is an abstract."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

BENJAMIN SILLIMAN.

OUR honored associate, Professor BENJAMIN SILLIMAN, the founder of this Journal, whose name has appeared upon the title page of every number, from the first until the present, is with us no more. He died at his residence in New Haven, early Thursday morning, Nov. 24, 1864 (the day set apart for a national thanksgiving), having reached the age of 85 years.

It becomes our duty to place on record in these pages, as an inscription to the monument which he has himself erected, an outline of his career and a tribute to his memory. Few men enter life with such promise as he; fewer still sustain themselves so evenly, and die so widely lamented.

Instruction in natural science has been his great work; and in it he was emphatically a man of the times. Beginning when almost nothing was known in this country of the departments to which he was especially devoted, he lived to see them carried forward to a high degree of progress, and their importance everywhere acknowledged. His life, which was one of few marked incidents, was passed in his native State, in connection with Yale College, the institution that early selected him as one of its faculty. Two or three times he was invited to become the president of colleges elsewhere, but New Haven continued his chosen home. Twice he visited Europe, first in 1805–6, in order to qualify himself for his work in life by attendance upon lectures in London and Edinburgh, and by observation of foreign institutions of learning; and again, near

the close of his life, in 1851, when he was accompanied by his son, and made a more extended tour of observation and inquiry. Frequent journeys in his own country made him acquainted personally with the institutions and the men of every State, while his habits of prompt and friendly correspondence perpetuated the intimacies which he formed at home and abroad.

Without attempting a formal biography (which the late day of his decease renders impossible at this time), we propose to speak briefly of Professor Silliman's career as an officer of Yale College, and as a man of science, and then of his personal character and influence in the community.

The Silliman family has resided in Fairfield, Conn., since the early colonial days. Tradition says that Claudio Sillimandi, their earliest known ancestor, was driven, in 1517, from Lucca, Italy, to Switzerland, by religious persecution. The descendants resided in Berne, and afterward in Geneva, whence they emigrated through Holland to this country about the middle of the seventeenth century. A worthy pastor of the name, living with his family near Neuchatel, was visited by Prof. Silliman in 1851.

Ebenezer Silliman, the grandfather of Benjamin, graduated at Yale College in 1727, and Gold Selleck, the father, in 1752. The latter was a Brigadier General of militia in the Revolution, and was entrusted for a time with the defence of the Long Island coast. In 1775 he was married to Mary, the daughter of Rev. Joseph Fish of Stonington, and the widow of Rev. John Noyes. The two children of this marriage, Gold Selleck and Benjamin, became members of the same class in college, and have maintained through life an intimacy peculiarly fresh and cordial. The younger brother, Benjamin, was born in North Stratford, Conn. (now the town of Trumbull), August 8, 1779. The elder, who was born in 1777, is still living in Brooklyn, N. Y.[1]

Throughout his active life, Professor Silliman has been identified with Yale College. He entered the institution in 1792, graduated in 1796, became a Tutor in 1799, was appointed Professor of Chemistry and Natural History in 1804; and in 1853, having been relieved at his own request from further service as an instructor, he was designated, by the Corporation, Professor *emeritus*. Thus, during a period of nearly three-quarters of a century, his name has appeared as a student and

[1] Prof. Silliman was twice married: first, in 1809, to Harriet, daughter of the second Gov. Trumbull of Connecticut, the mother of his nine children; and again, in 1851, to Mrs. Sarah Webb, daughter of John McClellan. Five children survive him, one son and four daughters. All are married, the eldest daughter to J. B. Church, the second to Prof. O. P. Hubbard, the third to Prof. J. D. Dana, and the fourth to Rev. E. W. Gilman. His descendants include twenty-three grandchildren, besides five deceased, and two great-grandchildren.

teacher successively on the catalogues of the college. He was a pupil both of Dr. Stiles and Dr. Dwight, and the colleague of the latter during eighteen years. With President Day and Professor Kingsley he was associated for half a century and more in the government of the institution.

In the capacity of a college officer, he was preëminent as a teacher. The professor's chair, in the laboratory or the lecture-room, was the place above all others in which his enthusiasm, his sympathy with youthful aspirations, his varied acquisitions, his acquaintance with the world of Nature and of Art, and his graceful utterance, exerted their highest and most enduring influence. The minds which he aroused to the study of Nature have become investigators and teachers in every portion of the country, and all his pupils, whether devoted to science or to letters, will bear testimony to the interest which he awakened in these pursuits. They will never forget the admirable tact with which the manipulations of the laboratory were performed, or the brilliant experiments in chemistry which the lecturer seemed to enjoy as if, like the class, he had never witnessed them before. The course in chemistry, in early years, extended through one hundred and twenty lectures. In later days it was not so long, but was followed by a course in mineralogy and another in geology. Here, too, Prof. Silliman had the same magnetic influence on his students, sending them off on long walks about New Haven and at home to search for specimens, or to study the phenomena of geology. The third of these annual courses, that on geology, he gave with peculiar zest and eloquence. He delighted to depict the catastrophes of geological history and to clothe the world with the plants and animals of former days.

Professor Silliman was less concerned in the government of the students than some of his associates; but questions were continually arising in which his counsel was of weight. He was prompt in rebuking every form of youthful delinquency, yet was never harsh or inconsiderate. No student ever left his presence feeling wronged or indignant. He would much rather sacrifice a rule than injure an offender. If he seemed sometimes to be lenient, it was the leniency of a father, for his mind regarded the improvement of his scholars rather than the enforcement of routine and discipline. His paternal lectures to the Freshman class on morals and manners were admirable in their influence, and many a graduate of the college will acknowledge that his habits for life were affected by the judicious hints which he received from his kind and sympathising teacher.

Mr. Silliman's labors began with instruction; but they did not end there. His active and versatile disposition led him to become interested in and to help forward whatever would con-

tribute to the welfare of Yale College. When he went abroad, in 1805, to fit himself for the duties of his professorship, the purchase of books for the library was one of the duties with which he was especially charged. He was one of the library committee until his retirement. In his own departments, not only the Chemical Laboratory, but also the Cabinet of Minerals owed its existence to his energy. This collection is indeed so important, that something more than the mere mention of it seems due. About the time when Mr. Silliman was appointed a professor, the entire mineralogical and geological collection of Yale College was transported to Philadelphia in one small box, that the specimens might be named by Dr. Adam Seybert, then fresh from Werner's School at Freiberg, the only man in this country who could be regarded as a mineralogist scientifically trained. From this small beginning grew the present cabinet. In 1810, owing to personal regard for Prof. Silliman, Col. George Gibbs deposited with Yale College his valuable collection of minerals, and, after it had remained open to the public fifteen years, various friends of the college, chiefly through the instrumentality of Prof. Silliman, subscribed for its purchase the sum of $20,000. Other important accessions were also secured through his influence, not only from college graduates and other American gentlemen, but from various foreign collectors.

The Clark telescope is another of the donations to Yale College due to Prof. Silliman. This excellent glass, the best in the country at the time of its purchase, was the means of exciting among the students of the college unusual attention to astronomical pursuits for many years after its reception. The liberal donor, a farmer near New Haven, by this and other more important gifts, placed himself foremost among all the benefactors of the college up to that time, and Mr. Silliman was the medium through whom his benefactions were bestowed. The Trumbull Gallery of Paintings, a collection of priceless value, not only as works of art but also as illustrations of American history and biography, was secured to the college through the same enlightened instrumentality.[2] The Medical Institution of Yale College and the Sheffield School of Science, important branches of the University, were both greatly aided in their beginnings by the influential exertions put forth by Professor Silliman. He was one of the chief founders of the Alumni association of the college, and at their anniversaries and on other occasions, he was, as another has said, "the standing 'orator' of the college,

[2] It is an interesting fact that as early as 1842, Prof. Silliman, in his Alumni address, pointed out the need of another edifice for the Fine Arts, or an extension of the Trumbull Gallery, at no distant day. This want is about to be supplied by the liberality of an alumnus of the college. Prof. Silliman was prevented by illness, a week before his decease, from taking a public part in the exercises of laying the corner stone of the proposed structure.

the principal medium between those who dwelt in the academic shade and the great public." Not unfrequently he was the college solicitor, asking funds for the expansion of the institution, and never asking in vain.

Although his services as a college officer were great, Prof. Silliman's strongest claim to the gratitude of men of science rests upon the establishment, and the maintenance, often under very discouraging circumstances, of the "American Journal of Science." The history of this undertaking has already been given, in his own words, in the introduction to the 50th or Index volume of the First series of the Journal; and it is for others, rather than for us, to give an estimate of his editorial services. It is but just, however, to call attention to a few circumstances, which all will regard as creditable to its founder.

He had the sagacity to foresee, as long ago as 1818, the scope which such a magazine should take. The prospectus which he then wrote is applicable almost exactly to our pages to-day. Experience has established the wisdom of the course which he marked out.

He maintained the Journal, from the beginning, at his own pecuniary risk. Its publication has often been a serious financial burden, and in its most prosperous days has not yielded a fair return for editorial labor. But it has been continued, at this personal inconvenience, for the sake of American science, that the labors of our countrymen might be made known abroad, and the labors of Europeans understood in this country.

The Journal has never been used for the benefit of any party or individual, but solely for the advancement and diffusion of scientific truth. Its pages have been always open to free scientific discussion, with truth as the single end in view.

The original investigations of Prof. Silliman are not numerous. In the early part of his career he began with energy some important experiments and researches. He undertook a geological survey of Connecticut; he published a paper in conjunction with Prof. Kingsley on the famous Weston meteorite; he applied the newly invented blowpipe of his friend, Dr. Hare, to the fusion of a variety of bodies, which were before regarded as infusible; he demonstrated in the galvanic battery the transfer of particles of carbon from one charcoal point to the other; he made scientific examinations of various localities interesting in their geological or mineralogical aspects. But he was too much needed elsewhere to be allowed to remain a close student in the laboratory, or to engage with constancy as an explorer in the field of geological research. He has probably been a more useful man in the wider spheres of influence to which he was called than he could have been in a life devoted to scientific investigation.

During a considerable part of his life, he was one of the few men in the country who could hold a popular audience with a lecture on science. The public early knew of his capabilities, and for many years he yielded to invitations from various parts of the country to deliver lectures on Geology and Chemistry. In 1833 he gave his first popular course on Geology at New Haven, which was repeated in 1834 at Hartford and Lowell, and in 1835 at Boston and Salem. At Boston, the audience desiring to attend was so much larger than the largest hall would hold, that each lecture was given twice for the accommodation of the public. From 1840 to 1843 inclusive, he gave four successive courses of the "Lowell Lectures" in Boston. Besides various other engagements in the Northern and Eastern states, he went in 1847 by invitation to New Orleans, and on his way appeared before crowded audiences in other cities of the South; and five years after the resignation of his professorship in college, when he had passed his 75th year, he made the long journey to St. Louis, in obedience to a call for a course of lectures from the citizens of that place.

In lecturing, his language was simple—his flow of words easy, generous and appropriate—his style animated, abounding in life-like and well-adorned description, often eloquent, and sometimes varied with anecdote running occasionally into wide digressions. His manner was natural, and every feature spoke as well as his mouth; his noble countenance and commanding figure (he was nearly six feet in height, with a well-built frame) often called forth, as he entered the lecture hall, the involuntary applause of his audience.

In his popular courses he often lectured on the subject of "Geology and Genesis," and as he was widely known not only as a man of science, but as a sincere believer in the sacred Scriptures, he greatly aided in removing from the religious world the apprehension that science and religion were hostile in their teachings.

Mr. Silliman found great pleasure in helping forward other men of science. He rejoiced heartily in their progress; his house and his laboratory were always open to receive them, and if a friendly word or letter from him could advance their interests, he was ever ready to bestow it. He also felt a deep concern for the advancement of scientific investigations in every part of the country, and whenever, in halls of legislation, or before the public, the name of Benjamin Silliman would advance a useful project, it was not withheld. In more than one instance, the foreigner, or the exile, remembers his kindness with almost filial devotion.

Prof. Silliman's scientific publications, apart from his contributions to this journal, were chiefly text-books. He edited

Henry's Chemistry and Bakewell's Geology, for the use of his pupils, and also published a work on Chemistry, in two volumes.

His long labors for science brought him honors from all parts of the world. His name is on the roll of several of the principal scientific Academies or Societies of Europe, and of those of his own country. He was one of the original members of the National Academy of Sciences, and a Regent of the Smithsonian Institution.

Aside from Professor Silliman's influence as an officer of Yale College, and as a well known man of science, his personal hold upon the community at large was remarkably strong. This was due somewhat to the favor with which his popular lectures were received, and to the wide circuit over which he had journeyed. It was also owing in part to the pleasure and instruction which were afforded by his books of travel. Twice, as we have stated, Professor Silliman visited Europe, the interval between his journeys being nearly fifty years. Both these visits led to the publication of his observations in volumes which were widely read. The narrative of his earlier journey especially was received by the public with great delight. Few Americans then went abroad; and hardly any had published narratives of what they had seen. Mr. Silliman's volumes were fascinating to young and old,—and many were the testimonials which he received of the interest thus awakened in European institutions and manners. His Journal of a Tour to Canada was another contribution to the literature of the day.

But the general influence of Mr. Silliman must be attributed to his personal character rather than to any of what may be termed the accidental circumstances of his life. He was a man of vigorous understanding and sound judgment, led on, but never carried away, by an enthusiastic disposition, glowing and constant. With this was associated sterling integrity, which never harbored a selfish or dishonorable purpose, but rejoiced in doing and encouraging whatever was right. Every one could trust him. These fundamental traits were adorned by the outward qualities of affability and courtesy, or rather were expressed in manners at once so dignified and so kind that all with whom he came in contact were charmed at once, and on closer intercourse were bound to him as friends for life. Such friendships he never neglected or forgot. Even the sons and the grandsons of his early associates inherited a share in the regard which he had bestowed upon their parents. Blending with and ennobling all these virtues, was the child-like simplicity of his Christian faith.

A character like this shines the brighter the nearer it is seen. In his own family circle, Mr. Silliman has moved for years as a patriarch, surrounded by his descendants to the third and

fourth generation. The very house which he occupied has become historic, reflecting in its arrangements, its family portraits, its interesting mementos of absent friends, and its long shelves of books, the controlling mind which has dwelt there.

In the neighborhood and town where he resided, Mr. Silliman was peculiarly beloved and respected. "New Haven will not be New Haven without him," said more than one of his associates, as he heard of his death. His hand was always open to the needy. He was given to hospitality. He frequently took part in public meetings, and was actively concerned in all questions of local improvement. He rarely, if ever, failed to discharge his duties as a citizen at the polls, and was always ready to express his opinions on questions of public policy.

A whole-souled patriot, he viewed with the deepest interest the complications brought into the affairs of the country by the system of slavery. His general benevolence ever led him to sympathize with the oppressed, and the wrongs of the African touched him deeply. We cannot better indicate his feelings on this subject than by quoting a few sentences from his private journal under the date of April, 1850. After mentioning the death of the champion of what have been called "Southern rights," John C. Calhoun, his former pupil and friend, he gives a brief sketch of his character, concluding as follows:

"His public career has been highly distinguished. It is, however, very much to be regretted that he, many years ago, narrowed down his great mind to sectional views, and that he became morbidly sensitive and jealous of encroachment as regards the South, especially in reference to the protective tariff and to slavery. The former prompted his efforts for nullification, and the latter excited him to a vindication of slavery in the abstract. He, in a great measure, changed the state of opinion and the manner of speaking and writing upon this subject in the South, until we have come to present to the world the mortifying and disgraceful spectacle of a great republic—and the only real republic in the world—standing forth in vindication of slavery, without prospect of, or a wish for, its extinction. If the views of Mr. Calhoun, and of those who think with him, are to prevail, slavery is to be sustained on this great continent forever. I will not occupy my pages with any extended remarks upon this subject which is now agitating the national councils, and to a degree the nation itself. * * * It [the great question] is in better hands than man's; and I trust ultimately the colored men of all races on this continent will be received into the great human family as rational beings and as heirs of immortality."

As soon as the atrocities in Kansas revealed the determination of the advocates of slavery to perpetuate and extend that institution, even if they dissevered or destroyed the nation, Mr. Silliman came out with all his youthful ardor, and with the influence of his years and reputation, as the opponent of the slave-power. He thus became the object of personal defamation, even

in the Senate chamber at Washington; but he still remained firm, for he recognized in this war a slaveholder's rebellion. All the lofty sentiments of patriotism which were awakened in childhood, as he witnessed the commencement of national life, were intensified by this struggle to maintain the Union. He was sure that the nation would be purified by the conflict, and liberty established through all the land.

Mr. Silliman has always been remarkable for uniform good health, and in his later years manifested but slightly the encroachments of age. To the last, his form was as erect, his brow as serene, and his features as full of life and cheerfulness as in his earlier days; and his gait was only a little slower and more cautious.

He continued as usual until the middle of November just past, when he was for a few days quite unwell, probably as an immediate consequence of exposure to cold in attending an evening meeting in behalf of the Sanitary Commission. He had gradually, to appearance, regained nearly his former strength during the following week, and, on Wednesday, was intending to join the family Thanksgiving festival the next day at the house of his son-in-law, Prof. Dana. On the morning of that day, Nov. 24th, he awoke early, after a night of quiet rest, feeling stronger, as he said, than he had done for some days. He spoke with his wife of the many reasons there were for thankfulness, both public and private, dwelling at length upon the causes for national gratitude, especially in the recent re-election to the Presidency of a man who had proved himself so true, so honest, so upright in conducting the affairs of the government as Mr. Lincoln. As was his custom, while still in his bed, he offered up a short prayer, and repeated a familiar hymn of praise. In resuming his conversation, before rising, he spoke of the possibility of his attending the public services of the day, of the happiness of his home, of the love of his children, and, in strong terms of endearment, of his wife. Just as these his last words of love were uttered, there was a sudden change of countenance, a slightly heavier breath and he was gone. At the advanced age of 85, life to him was still beautiful; and not less so was its close. His sun set in the blessedness of the Christian's faith, to rise on a brighter morrow.

ART. II.—*Notice of the Explorations of the Geological Survey of California, in the Sierra Nevada, during the summer of* 1864.

IN the September number of this Journal (vol. xxxviii, page 298) a notice was inserted giving a sketch of the explorations of the Geological Survey of California in the High Sierra about the heads of King's and Kern rivers (latitude 36° 30′ to 37°). It was mentioned that Mr. King had, on the 6th of July, ascended a peak over 14,000 feet high, and that he was about to make an attempt to reach a point still higher, which was one of two in sight from, and higher than, the peak he had just been on. Since the last number of the Journal was issued, some further particulars have been received with regard to the peak ascended on the 6th of July, to which the name of Mt. Tyndall was given, and also a brief account of the attempt made to get on to the highest points.

The ascent of Mt. Tyndall was a very difficult and dangerous one; but the summit was reached without accident either to Mr. King or his companion, although the trip required that several nights should be spent at an elevation of over 12,000 feet, of course without fire, and with but scanty covering. From the summit of Mt. Tyndall, which is considerably over 14,000 feet high, there appeared two other peaks of equal elevation and two still higher, all within a distance of seven miles of this. Mr. King says, "of the two highest, one rose close by, being hardly a mile away; it is an inaccessible bunch of needles. The other was equally inaccessible from any point on the north or west side. The first-mentioned was about 150 feet above us, the other was six or seven miles distant and I should think fully 350 feet higher than the peak we were on. Within our field of view were five mountains over 14,000 feet and about fifty over 13,000. The five highest peaks are all in the eastern ridge. Owen's valley, a brown sage plain, lies 10,000 feet below on the one side, and Kern cañon, once the rocky bed of a grand old glacier, 4000 feet down on the other. About fifteen miles north of here, King's river cuts through the western ridge and turns at a right angle toward the plain. North of this point, again, the two great ridges unite in a grand pile of granite mountains, whose outlines are all of the most rugged and fantastic character. Twenty-five miles south, the high group ends, there (certainly for a breadth of sixty miles) forming one broad, rolling, forest-covered plateau, 8000 to 9000 feet in elevation."

"From Mt. Brewer to Kaweah Peak, the two culminating points of the western ridge, for a distance of fifteen miles, there is nothing that can be called a separate mountain: it is, rather, a great mural ridge, capped by small sharp cones and low ragged domes, all covered with little minarets. At one place the ridge

forms a level table; upon this lies an unbroken cover of snow. To the eastward, all this range, from King's river gateway to Kaweah Peak, presents a series of blank, almost perpendicular precipices, broken every mile or so by a bold granite buttress. Between these are vast snow fields, and also numberless deep lakes, of which the most elevated are frozen."

"The few *Pinus contorta*, and the groves of our new pine[1] have a peculiar black color, or, rather, dark bluish-green, which rather augments than relieves the desolate, naked aspect of things." "The only bits of bright color to break the solemn monotony of granite and snow are the blue lakes, which lie everywhere in the ancient glacier beds."

"Far away in the north there is a broad red band in the granite; other than that, all is gray. Beyond Owen's valley is a low desert range. The Coso and Inyo mountains are in plain sight."

"To the eastward, parallel ridges, one beyond another, lie stretched before you, rigid and stony. They have the same aspect as the mountains near Washoe, the same brown color, with red and yellow shadings."

"Nearly due north, fifty or sixty miles distant, rises a lofty group of mountains, which culminates in a white cone. This must be very high, for unbroken snow covers fully two thousand feet on the south side of the peak, while even the highest mountains in our group have no snow except on the north flank. These are probably the 'White Mountains.' I venture 14,600 feet as a guess at the probable elevation of the highest point in the group."

The White mountains, of which Mr. King speaks, in the extract cited above, lie just on the borders of California and Nevada, in about lon. 118°, lat. 37° 30′; they were distinctly visible to us from Mt. Dana and the other high peaks near Mono lake. It is doubtful whether the highest points are within the State of California; but they are probably very near the line on one side or the other. As it is by no means impossible, although I do not consider it probable, that some points of this range of mountains are higher than any yet measured or ascended by our parties, we still have to remain for some time in uncertainty as to whether California can claim the highest elevations in the country as within her borders; we can, at least, say that the highest measured ones are.

The ascent of Mt. Tyndall was a successful one, the party returning to camp at the end of the fifth day, with bones and barometers unbroken, although several very narrow escapes are briefly mentioned in Mr. King's notes.

It appears from Mr. Brewer's notes, that Mr. King's barometrical measurements of Mt. Tyndall would make that mountain above the height of Mt. Shasta, or over 15,090 feet, Mt. Shasta being 14,440. The height of Visalia, which is the plane of

[1] A new and not yet named species discovered by the Survey during this trip.

reference for all our observations this year in the High Sierra, has not yet been as accurately determined as it will be after a longer series of observations has been worked up, this place having been, during the past summer, and still continuing to be, one of Major Williamson's stations.

Not satisfied with the former attempt to reach the highest peak of the Sierra, Mr. King made another, starting from Visalia, July 14th, with the intention of following the Owen's lake and Visalia trail, and endeavoring to reach the culminating point from somewhere on this route. This trail leads up the Kaweah river, following up the South fork, from its junction with the middle fork of the same river. Full particulars of this reconnoissance have not yet come to hand; but from letters lately received, it appears that the base of the high point toward which his efforts were directed was, with difficulty, reached; but that the actual summit of the peak was found to be inaccessible—at least from any direction except that of Owen's valley, and probably not to be scaled from that side. The highest point attained by Mr. King on this trip was 14,369 feet above Visalia, which place is approximately 360 feet above the sea level, making the total height 14,729 feet, which is considerably above Mt. Shasta. But Mr. King was not, by estimate, within 300 or 400 feet of the summit, so that it appears that the elevation above the sea-level of this, so far as known, the culminating point of the Sierra Nevada cannot fall short of 15,000 feet.[2]

In the mean time, the main party, under Professor Brewer, made an attempt to work up the topography of this portion of the Sierra north to the Merced river, by keeping along at a high altitude, on the west side of the main ridge; but, after many perilous and laborious attempts, they found the ridges between the north and south forks of King's river to be impassable, so that they were obliged to cross the summit of the Sierra and descend into Owen's valley, in order to get north in that valley, and by crossing back on some other pass to reach the region of the head-waters of the San Joaquin. The pass by which the Sierra was crossed, going into Owen's valley, was about 11,600 feet high, all the region about the summit being exceedingly rough, sharp peaks of granite rising 2,500 feet or more above the pass on both sides. The party travelled for three days up Owen's valley, then turned and crossed the Sierra, by an old Indian foot-trail, to the head-waters of the San Joaquin, the summit of the pass being just about 12,000 feet high, and the mountains on each side 1,000 to 1,200 feet higher

From here the party made their way to Clark's ranch, between the Mariposa estate and the Yo-semite, through many privations and some perils, but without serious accident to life or limb.

[2] To this peak the name of Mount Whitney was given by Messrs. Brewer, King and Hoffmann.

The result of the summer's reconnoissance has been that a general idea is secured of the topography of a region about as large as Massachusetts, lying wholly within the State of California, and of which nothing whatever was known previously to this. That mountain peaks should be found in this part of the State, higher than any known to exist in the United States, is a discovery equally interesting and unexpected. The details of this exploration will furnish many facts of great geological and geographical interest. J. D. W.

Northampton, Mass., Oct. 15, 1864.

ART. III.—*On the Mineral Waters of Bath and other hot springs, and their Geological effects;* by Sir CHARLES LYELL, Bart.[1]

* * * * What renders Bath a peculiar point of attraction to the student of natural phenomena is its thermal and mineral waters, to the sanatory powers of which the city has owed its origin and celebrity. The great volume and high temperature of these waters render them not only unique in our island, but perhaps without a parallel in the rest of Europe, when we duly take into account their distance from the nearest region of violent earthquakes or of active or extinct volcanos. The spot where they issue, as we learn from the researches of the historian and antiquary, was lonely and desert when the Romans first landed in this island, but in a few years it was converted into one of the chief cities of the newly conquered province. On the site of the hot springs was a large morass, from which clouds of white vapor rose into the air; and there first was the spacious bath-room built, in a highly ornamental style of architecture, and decorated with columns, pilasters, and tessellated pavements. By its side was erected a splendid temple dedicated to Minerva, of which some statues and altars, with their inscriptions, and ornate pillars are still to be seen in the Museum of this place. To these edifices the quarters of the garrison, and in the course of time the dwellings of new settlers, were added; and they were all encircled by a massive wall, the solid foundations of which still remain.

A dense mass of soil and rubbish, from 10 to 20 feet thick, now separates the level on which the present city stands from the level of the ancient Aquæ Solis of the Romans. Digging through this mass of heterogeneous materials, coins and coffins of the Saxon period have been found; and lower down, beginning at the depth of from 12 to 15 feet from the surface, coins have been disinterred of Imperial Rome, bearing dates from the reign of Claudius to that of Maximus in the fifth century. Be-

[1] From the inaugural address at the opening of the meeting of the British Association at Bath, Sept. 14, 1864.

neath the whole, are occasionally seen tesselated pavements still retaining their bright colors, one of which, on the site of the Mineral-water Hospital, is still carefully preserved, affording us an opportunity of gauging the difference of level of ancient and modern Bath.

One of our former Presidents, Dr. Daubeny, has remarked that nearly all the most celebrated hot springs of Europe, such as those of Aix-la-Chapelle, Baden-Baden, Naples, Auvergne, and the Pyrenees, have not declined in temperature since the days of the Romans; for many of them still retain as great a heat as is tolerable to the human body, and yet when employed by the ancients they do not seem to have required to be first cooled down by artificial means. This uniformity of temperature, maintained in some places for more than 2000 years, together with the constancy in the volume of the water, which never varies with the seasons, as in ordinary springs, the identity also of the mineral ingredients which, century after century, are held by each spring in solution, are striking facts, and they tempt us irresistibly to speculate on the deep subterranean sources both of the heat and mineral matter. How long has this uniformity prevailed? Are the springs really ancient in reference to the earth's history, or, like the courses of the present rivers and the actual shape of our hills and valleys, are they only of high antiquity when contrasted with the brief space of human annals? May they not be like Vesuvius and Etna, which, although they have been adding to their flanks in the course of the last 2000 years many a stream of lava and shower of ashes, were still mountains very much the same as they now are in height and dimensions from the earliest times to which we can trace back their existence? Yet although their foundations are tens of thousands of years old, they were laid at an era when the Mediterranean was already inhabited by the same species of marine shells as those with which it is now peopled; so that these volcanos must be regarded as things of yesterday in the geological calendar.

Notwithstanding the general persistency in character of mineral waters and hot springs ever since they were first known to us, we find on inquiry that some few of them, even in historical times, have been subject to great changes. These have happened during earthquakes which have been violent enough to disturb the subterranean drainage and alter the shape of the fissures up which the waters ascend. Thus, during the great earthquake at Lisbon in 1755, the temperature of the spring called La Source de la Reine at Bagnères de Luchon, in the Pyrenees, was suddenly raised as much as 75° F., or changed from a cold spring to one of 122° F., a heat which it has since retained. It is also recorded that the hot springs at Bagnères de Bigorre, in the same mountain-chain, became suddenly cold

during a great earthquake which, in 1660, threw down several houses in that town.

It has been ascertained that the hot springs of the Pyrenees, the Alps, and many other regions, are situated in lines along which the rocks have been rent, and usually where they have been displaced or "faulted." Similar dislocations in the solid crust of the earth are generally supposed to have determined the spots where active and extinct volcanos have burst forth; for several of these often affect a linear arrangement, their position seeming to have been determined by great lines of fissure. Another connecting link between the volcano and the hot spring is recognizable in the great abundance of hot springs in regions where volcanic eruptions still occur from time to time. It is also in the same districts that the waters occasionally attain the boiling-temperature, while some of the associated stufas emit steam considerably above the boiling-point. But in proportion as we recede from the great centres of igneous activity, we find the thermal waters decreasing in frequency and in their average heat, while at the same time they are most conspicuous in those territories where, as in Central France or the Eifel in Germany, there are cones and craters still so perfect in their form, and streams of lava bearing such a relation to the depth and shape of the existing valleys, as to indicate that the internal fires have become dormant in comparatively recent times. If there be exceptions to this rule, it is where hot springs are met with in parts of the Alps and Pyrenees which have been violently convulsed by modern earthquakes.

To pursue still further our comparison between the hot spring and the volcano, we may regard the water of the spring as representing those vast clouds of aqueous vapor which are copiously evolved for days, sometimes for weeks, in succession from craters during an eruption. But we shall perhaps be asked whether, when we contrast the work done by the two agents in question, there is not a marked failure of analogy in one respect —namely a want, in the case of the hot spring, of power to raise from great depths in the earth voluminous masses of solid matter corresponding to the heaps of scoriæ and streams of lava which the volcano pours out on the surface. To one who urges such an objection it may be said that the quantity of solid as well as gaseous matter, transferred by springs from the interior of the earth to its surface, is far more considerable than is commonly imagined. The thermal waters of Bath are far from being conspicuous among European hot springs, for the quantity of mineral matter contained in them in proportion to the water which acts as a solvent; yet Professor Ramsay has calculated that if the sulphates of lime and soda, and the chlorids of sodium and magnesium, and the other mineral ingredients which they contain, were solidified, they would form in one year a

square column 9 feet in diameter, and no less than 140 feet in height. All this matter is now quietly conveyed by a stream of limpid water, in an invisible form, to the Avon, and by the Avon to the sea; but if, instead of being thus removed, it were deposited around the orifice of eruption, like the siliceous layers which encrust the circular basin of an Icelandic geyser, we should soon see a considerable cone built up, with a crater in the middle; and if the action of the spring were intermittent, so that ten or twenty years should elapse between the periods when solid matter was emitted, or (say) an interval of three centuries, as in the case of Vesuvius between 1306 and 1631, the discharge would be on so grand a scale as to afford no mean object of comparison with the intermittent outpourings of a volcano.

Dr. Daubeny, after devoting a month to the analysis of the Bath waters, in 1833, ascertained that the daily evolution of nitrogen gas amounted to no less than 250 cubic feet in volume. This gas, he remarks, is not only characteristic of hot springs, but is largely disengaged from volcanic craters during eruptions. In both cases, he suggests that the nitrogen may be derived from atmospheric air, which is always dissolved in rain-water, and which, when this water penetrates the earth's crust, must be carried down to great depths, so as to reach the heated interior. When there, it may be subjected to deoxydating processes, so that the nitrogen, being left in a free state, may be driven upward by the expansive force of heat and steam, or by hydrostatic pressure. This theory has been very generally adopted, as best accounting for the constant disengagement of large bodies of nitrogen, even where the rocks through which the spring rises are crystalline and unfossiliferous. It will, however, of course be admitted, as Professor Bischof has pointed out, that in some places organic matter has supplied a large part of the nitrogen evolved.

Carbonic acid gas is another of the volatilized substances discharged by the Bath waters. Dr. Gustav Bischof, in the new edition of his valuable work on chemical and physical geology, when speaking of the exhalations of this gas, remarks that they are of universal occurrence, and that they originate at great depths, becoming more abundant the deeper we penetrate. He also observes that, when the silicates which enter so largely into the composition of the oldest rocks are percolated by this gas, they must be continually decomposed, and the carbonates formed by the new combinations thence arising must often augment the volume of the altered rocks. This increase of bulk, he says, must sometimes give rise to a mechanical force of expansion capable of uplifting the incumbent crust of the earth; and the same force may act laterally, so as to compress, dislocate, and tilt the strata on each side of a mass in which the new chemical changes are developed. The calculations made by this eminent

German chemist of the exact amount of distention which the origin of new mineral products may cause, by adding to the volume of the rocks, deserve the attention of geologists, as affording them aid in explaining those reiterated oscillations of level—those risings and sinkings of land—which have occurred on so grand a scale at successive periods of the past. There are probably many distinct causes of such upward, downward, and lateral movements, and any new suggestion on this head is most welcome; but I believe the expansion and contraction of solid rocks, when they are alternately heated and cooled, and the fusion and subsequent consolidation of mineral masses, will continue to rank, as heretofore, as the most influential causes of such movements.

The temperature of the Bath waters varies in the different springs from 117° to 120° F. This, as before stated, is exceptionally high, when we duly allow for the great distance of Bath from the nearest region of active, or recently extinct, volcanos and of violent earthquakes. The hot springs of Aix-la-Chapelle have a much higher temperature, viz. 135° F., but they are situated within forty miles of those cones and lava-streams of the Eifel which, though they may have spent their force ages before the earliest records of history, belong, nevertheless, to the most modern geological period. Bath is about 400 miles distant from the same part of Germany, and 440 from Auvergne—another volcanic region, the latest eruptions of which were geologically coëval with those of the Eifel. When these two regions in France and Germany were the theaters of frequent convulsions, we may well suppose that England was often more rudely shaken than now; and such shocks as that of October last, the sound and rocking motion of which caused so great a sensation as it traversed the southern part of the island, and seems to have been particularly violent in Herefordshire, may be only a languid reminder to us of a force of which the energy has been gradually dying out.

If you consult the geological map of the environs of this city, colored by the Government surveyors, you will perceive that numerous lines of fault or displacement of the rocks are there laid down, and one of these has shifted the strata vertically as much as 200 feet. Mr. Charles Moore pointed out to me last spring, when I had the advantage of examining the geology of this district under his guidance, that there are other lines of displacement not yet laid down on the Ordnance Map, the existence of which must be inferred from the different levels at which the same formations crop out on the flanks of the hills to the north and south of the city. I have therefore little doubt that the Bath springs, like most other thermal waters, mark the site of some great convulsion and fracture which took place in the crust

of the earth at some former period—perhaps not a very remote one, geologically speaking. The uppermost part of the rent through which the hot water rises is situated in horizontal strata of Lias, and Trias, 300 feet thick; and this may be more modern than the lower part, which passes through the inclined and broken strata of the subjacent Coal-measures, which are unconformable to the Trias. The nature and succession of these rocks penetrated by the Bath waters was first made out by the late William Smith, in 1817, when a shaft was sunk in the vicinity in search for coal. The shock which opened a communication through the upper rocks may have been of a much later date than that which fractured the older and underlying strata; for there is a tendency in the earth's crust to yield most readily along lines of ancient fracture, which constitute the points of least resistance to a force acting from below.

If we adopt the theory already alluded to, that the nitrogen is derived from the deoxydation of atmospheric air carried down by rain-water, we may imagine the supply of this water to be furnished by some mountainous region, perhaps a distant one, and that it descends through rents or porous rocks till it encounters some mass of heated matter by which it is converted into steam and then driven upward through a fissure. In its downward passage, the water may derive its sulphate of lime, chlorid of calcium, and other substances from the decomposition of the gypseous, saline, calcareous and other constituents of the rocks which it permeates. The greater part of the ingredients are common to sea-water, and might suggest the theory of a marine origin; but the analysis of the Bath springs by Merck and Galloway shows that the relative proportion of the solid matter is far from agreeing with that of the sea, the chlorid of magnesium being absolutely in excess, that is, 14 grains of it per gallon for 12 of common salt; whereas in sea-water there are 27 grains of salt, or chlorid of sodium, to 4 of the chlorid of magnesium. That some mineral springs, however, may derive an inexhaustible supply through rents and porous rocks, from the leaky bed of the ocean, is by no means an unreasonable theory, especially if we believe that the contiguity of nearly all the active volcanos to the sea is connected with the access of salt water to the subterranean foci of volcanic heat.

Professor Roscoe, of Manchester, has been lately engaged in making a careful analysis of the Bath waters, and has discovered in them three metals which they were not previously known to contain—namely, copper, strontium, and lithium; but he has searched in vain for cæsium and rubidium, those new metals, the existence of which has been revealed to us in the course of the last few years by what is called spectrum analysis. By this new method, the presence of infinitesimal quantities, such as would

have wholly escaped detection by ordinary tests, are made known to the eye by the agency of light. Thus, for example, a solid substance such as the residue obtained by evaporation from a mineral water is introduced on a platinum wire into a colorless gas-flame. The substance thus volatilized imparts its color to the flame, and the light, being then made to pass through a prism, is viewed through a small telescope or spectroscope, as it is called, by the aid of which one or more bright lines or bands are seen in the spectrum, which, according to their position and color, indicate the presence of different elementary bodies.

Professor Bunsen, of Heidelberg, led the way, in 1860, in the application of this new test to the hot waters of Baden-Baden and of Dürkheim in the Palatinate. He observed in the spectrum some colored lines of which he could not interpret the meaning, and was determined not to rest till he found out what they meant. This was no easy task, for it was necessary to evaporate fifty tons of water to obtain 200 grains of what proved to be two new metals. Taken together, their proportion to the water was only as one to three million. He named the first cæsium, from the bluish-gray lines which it presented in the spectrum; and the second rubidium, from its two red lines. Since these successful experiments were made, thallium, so called from its green line, was discovered in 1861 by Mr. Crooks; and a fourth metal, named indium, from its indigo colored band, was detected by Professor Richter, of Freiberg, in Saxony, in a zinc ore of the Hartz. It is impossible not to suspect that the wonderful efficacy of some mineral springs, both cold and thermal, in curing diseases, which no artificially prepared waters have as yet been able to rival, may be connected with the presence of one or more of these elementary bodies previously unknown; and some of the newly found ingredients, when procured in larger quantities, may furnish medical science with means of combating diseases which have hitherto baffled all human skill.

While I was pursuing my inquiries respecting the Bath waters, I learned casually that a hot spring had been discovered at a great depth in the copper mine near Redruth, in Cornwall, having about as high a temperature as that of the Bath waters, and of which, strange to say, no account has yet been published. It seems that, in the year 1839, a level was driven from an old shaft so as to intersect a rich copper-mine at the depth of 1350 feet from the surface. This lode or metalliferous fissure occurred in what was formerly called the United Mines, and which have since been named the Clifford Amalgamated Mines. Through the contents of the lode a powerful spring of hot water was observed to rise, which has continued to flow with undiminished strength ever since. At my request, Mr. Horton Davey, of Redruth, had the kindness to send up to London many

gallons of this water, which have been analyzed by Professor William Allen Miller, F.R.S., who finds that the quantity of solid matter is so great as to exceed by more than four times the proportion of that yielded by the Bath waters.[2] Its composition is also in many respects very different; for it contains but little sulphate of lime, and is almost free from the salts of magnesium. It is rich in the chlorids of calcium and sodium, and it contains one of the new metals—cæsium, never before detected in any mineral spring in England; but its peculiar characteristic is the extraordinary abundance of lithium, of which a mere trace had been found by Professor Roscoe in the Bath waters; whereas in this Cornish hot spring this metal constitutes no less than a twenty-sixth part of the whole of the solid contents, which, as before stated, are so voluminous. When Professor Miller exposed some of these contents to the test of spectrum analysis, he gave me an opportunity of seeing the beautiful bright crimson lines which the lithium produces in the spectrum.

Lithium was first made known in 1817 by Arfvedson, who extracted it from petalite; and it was believed to be extremely rare, until Bunsen and Kirchhoff, in 1860, by means of spectrum analysis, showed that it was a most widely diffused substance, existing in minute quantities in almost all mineral waters, and in the sea, as well as in milk, human blood, and the ashes of some plants. It has already been used in medicine, and we may therefore hope that, now that it is obtainable in large quantities, and at a much cheaper rate than before the Wheal-Clifford hot spring was analyzed, it may become of high value. According to a rough estimate which has been sent to me by Mr. Davey, the Wheal-Clifford spring yields no less than 250 gallons per minute, which is almost equal to the discharge of the King's Bath or chief spring of this city. As to the gases emitted, they are the same as those of the Bath water—namely carbonic acid, oxygen, and nitrogen.

Mr. Warington Smyth, who had already visited the Wheal-Clifford lode in 1855, re-examined it in July last, chiefly with the view of replying to several queries which I had put to him; and, in spite of the stifling heat, ascertained the geological structure of the lode, and the exact temperature of the water. This last he found to be 122° F. at the depth of 1350 feet; but he scarcely doubts that the thermometer would stand two or three degrees higher at a distance of 200 feet to the eastward, where the water is known to gush up more freely. The Wheal-Clifford lode is a fissure varying in width from six to twelve feet, one wall consisting of elvan or porphyritic granite, and the other of killas or clay-slate. Along the line of the rent, which runs

[2] See for the analysis, the last volume of this Journal, p. 447.

east and west, there has been a slight throw or shift of the rocks. The vein-stuff is chiefly formed of cellular pyrites of copper and iron, the porous nature of which allows the hot water to percolate freely through it. It seems, however, that in the continuation upward of the same fissure, little or no metalliferous ore was deposited, but in its place, quartz and other impermeable substances, which obstructed the course of the hot spring, so as to prevent its flowing out on the surface of the country. It has been always a favorite theory of the miners that the high temperature of this Cornish spring is due to the oxydation of the sulphurets of copper and iron, which are decomposed when air is admitted. That such oxydation must have some slight effect is undeniable; but that it materially influences the temperature of so large a body of water is out of the question. Its effect must be almost insensible; for Professor Miller has scarcely been able to detect any sulphuric acid in the water, and a minute trace only of iron and copper in solution.

When we compare the temperature of the Bath springs, which issue at a level of less than 100 feet above the sea, with the Wheal-Clifford spring found at a depth of 1350 feet from the surface, we must of course make allowance for the increase of heat always experienced when we descend into the interior of the earth. The difference would amount to about 20° F., if we adopt the estimate deduced by Mr. Hopkins from an accurate series of observations made in the Monkwearmouth shaft, near Durham, and in the Dukinfield shaft near Manchester, each of them 2000 feet in depth. In these shafts, the temperature was found to rise at the rate of only 1° F. for every increase of depth of from 65 to 70 feet. But if the Wheal-Clifford spring, instead of being arrested in its upward course, had continued to rise freely through porous and loose materials so as to reach the surface, it would probably not have lost anything approaching to 20° F., since the renewed heat derived from below would have warmed the walls and contents of the lode, so as to raise their temperature above that which would naturally belong to the rocks at corresponding levels on each side of the lode. The almost entire absence of magnesium raises an obvious objection to the hypothesis of this spring deriving its waters from the sea; or if such a source be suggested for the salt and other marine products, we shall be under the necessity of supposing the magnesium to be left behind in combination with some of the elements of the decomposed and altered rocks through which the thermal waters may have passed.

Hot springs are, for the most part, charged with alkaline and other highly soluble substances, and, as a rule, are barren of the precious metals, gold, silver, and platinum, as well as of tin, copper, lead, and many others, a slight trace of copper in the Bath

waters being exceptional. Nevertheless, there is a strong presumption that there exists some relationship between the action of thermal waters and the filling of rents with metallic ores. The component elements of these ores may, in the first instance, rise from great depths in a state of sublimation or of solution in intensely heated water, and may then be precipitated on the walls of a fissure as soon as the ascending vapors or fluids begin to part with some of their heat. Almost everything, save the alkaline metals, silica, and certain gases, may thus be left behind long before the spring reaches the earth's surface. If this theory be adopted, it will follow that the metalliferous portion of a fissure, originally thousands of feet or fathoms deep, will never be exposed in regions accessible to the miner until it has been upheaved by a long series of convulsions, and until the higher parts of the same rent, together with its contents and the rocks which it had traversed, have been removed by aqueous denudation. Ages before such changes are accomplished, thermal and mineral springs will have ceased to act; so that the want of identity between the mineral ingredients of hot springs and the contents of metalliferous veins, instead of militating against their intimate relationship, is in favor of their being the complementary results of one and the same natural operation.

But there are other characters in the structure of the earth's crust more mysterious in their nature than the phenomena of metalliferous veins, on which the study of hot springs has thrown light: I allude to the metamorphism of sedimentary rocks. Strata of various ages, many of them once full of organic remains, have been rendered partially or wholly crystalline. It is admitted on all hands that heat has been instrumental in bringing about this re-arrangement of particles, which, when the metamorphism has been carried out to its fullest extent, obliterates all trace of the imbedded fossils. But as mountain-masses many miles in length and breadth, and several thousands of feet in height, have undergone such alteration, it has always been difficult to explain in what manner an amount of heat capable of so entirely changing the molecular condition of sedimentary masses could have come into play without utterly annihilating every sign of stratification, as well as of organic structure.

Various experiments have led to the conclusion that the minerals which enter most largely into the composition of the metamorphic rocks have not been formed by crystallizing from a state of fusion, or in the dry way, but that they have been derived from liquid solutions, or in the wet way—a process requiring a far less intense degree of heat. Thermal springs, charged with carbonic acid and with fluohydric acid (which last is often present in small quantities), are powerful causes of decomposition and chemical reaction in rocks through which they percolate.

If, therefore, large bodies of hot water permeate mountain-masses at great depths, they may in the course of ages superinduce in them a crystalline structure; and in some cases strata in a lower position and of older date may be comparatively unaltered, retaining their fossil remains undefaced, while newer rocks are rendered metamorphic. This may happen where the waters, after passing upward for thousands of feet, meet with some obstruction, as in the case of the Wheal-Clifford spring, causing the same to be laterally diverted so as to percolate the surrounding rocks. The efficacy of such hydro-thermal action has been admirably lilustrated of late years by the experiments and observations of Sénarmont, Daubrée, Delesse, Scheerer, Sorby, Sterry Hunt, and others.

The changes which Daubrée has shown to have been produced by the alkaline waters of Plombières, in the Vosges, are more especially instructive. These thermal waters have a temperature of 160° F., and were conveyed by the Romans to baths through long conduits or aqueducts. The foundations of some of their works consisted of a bed of concrete, made of lime, fragments of brick and sandstone. Through this and other masonry the hot waters have been percolating for centuries, and have given rise to various zeolites—apophyllite and chabazite besides others; also to calcareous spar, aragonite, and fluor spar, together with siliceous minerals, such as opal,—all found in the interspaces of the bricks and mortar, or constituting part of their rearranged materials. The quantity of heat brought into action in this instance in the course of 2000 years has, no doubt, been enormous, although the intensity of it developed at any one moment has been always inconsiderable.

The study, of late years, of the constituent parts of granite has in like manner led to the conclusion that their consolidation has taken place at temperatures far below those formerly supposed to be indispensable. Gustav Rose has pointed out that the quartz of granite has the specific gravity of 2·6, which characterizes silica when it is precipitated from a liquid solvent, and not that inferior density, namely 2·3, which belongs to it when it cools and solidifies in the dry way from a state of fusion.

But some geologists, when made aware of the intervention on a large scale, of water, in the formation of the component minerals of the granitic and volcanic rocks, appear of late years to have been too much disposed to dispense with intense heat when accounting for the formation of the crystalline and unstratified rocks. As water in a state of solid combination enters largely into the aluminous and some other minerals, and therefore plays no small part in the composition of the earth's crust, it follows that, when rocks are melted, water must be present, independently of the supplies of rain-water and sea-water which find

their way into the regions of subterranean heat. But the existence of water under great pressure affords no argument against our attributing an excessively high temperature to the mass with which it is mixed up. Still less does the point to which the melted matter must be cooled down before it consolidates or crystallizes into lava or granite afford any test of the degree of heat which the same matter must have acquired when it was melted and made to form lakes and seas in the interior of the earth's crust.

We learn from Bunsen's experiments on the Great Geyser in Iceland, that at the depth of only seventy-four feet, at the bottom of the tube, a column of water may be in a state of rest, and yet possess a heat of 120° Centigrade, or 248° F. What, then, may not the temperature of such water be at the depth of a few thousand feet? It might soon attain a white heat under pressure; and as to lava, they who have beheld it issue, as I did in 1858, from the southwestern flanks of Vesuvius, with a surface white and glowing like that of the sun, and who have felt the scorching heat which it radiates, will form a high conception of the intense temperature of the same lava at the bottom of a vertical column several miles high, and communicating with a great reservoir of fused matter, which, if it were to begin at once to cool down, and were never to receive future accessions of heat, might require a whole geological period before it solidified. Of such slow refrigeration, hot springs may be among the most effective instruments, abstracting slowly from the subterranean molten mass that heat which clouds of vapor are seen to carry off in a latent form from a volcanic crater during an eruption, or from a lava-stream during its solidification. It is more than forty years since Mr. Scrope, in his work on volcanos, insisted on the important part which water plays in an eruption, when intimately mixed up with the component materials of lava, aiding as he supposed, in giving mobility to the more solid materials of the fluid mass. But, when advocating this igneo-aqueous theory, he never dreamed of impugning the Huttonian doctrine as to the intensity of heat which the production of the unstratified rocks, those of the plutonic class especially, implies.

The exact nature of the chemical changes which hydrothermal action may effect in the earth's interior will long remain obscure to us, because the regions where they take place are inaccessible to man; but the manner in which volcanos have shifted their position throughout a vast series of geological epochs—becoming extinct in one region and breaking out in another—may, perhaps, explain the increase of heat as we descend toward the interior, without the necessity of our appealing to an original central heat, or the igneous fluidity of the earth's nucleus.

ART. IV.—*On the Nebular Hypothesis;* by DAVID TROWBRIDGE, A.M.

[Concluded from vol. xxxviii, p. 360.]

1. *The Breaking-up of the Rings.*

34. The process of cooling would still continue after the rings had separated. The loss of caloric from radiation would cause the rings to contract their dimensions; and this, the unequal density of different parts, and the extraordinary perturbations to which they would necessarily be subject in their motions, would cause a separation of the rings in certain weak places. When once broken into parts while the density of the ring was very small, even if the parts were not many, the case would be extraordinary in which the parts would be prevented from re-uniting into a single planet, owing to the very great perturbations to which their motions would be subject, as separate bodies. It is impossible at present to tell just how a ring would be resolved into a planet. A system of waves would probably be developed, owing to perturbations; and by an accumulation in one part, owing to the nature of the disturbing force, such as calculation shows might exist under certain conditions in the case of the rings of Saturn, and thus a considerable portion of the matter of the ring would be driven into one side, and this accumulation would be the center of attraction around which the planet would be formed.

35. We may now ask in what direction the planet thus formed would rotate? The direction of rotation would depend on circumstances. Let us take the breadth of the ring from which Uranus was formed, the same as the diameter of his sphere of attraction in Kirkwood's Analogy. The diameter of the sphere of attraction of Uranus, as given by Professor Kirkwood,[28] is 7·438. The inner radius is 2·558, and the outer one, 4·879. If we suppose the inner and the outer parts of the ring each to have the velocity due it according to Kepler's third law, then, the velocity of the inner part being called 1, that of the outer will be 0·8287; and the angular velocities of the same parts will be to each other as 1 to 0·4715. If every part of the ring have the same angular velocity, and that of the inner part be 1, the angular velocity of the outer part will be 1·457. If the inner and the outer parts have the same velocity, the angular velocity of the inner part being called 1, that of the outer part will be 0·6866. But none of these cases can obtain in any of the rings, but one or another will be approximated to, accord-

[28] This Journal, [2], xiv, p. 213.

ing to circumstances. The outer rings would approximate to the first case, and the inner ones, to the second case; while intermediate rings would approximate more or less, according to their position in the System, to the last case. In confirmation of this, we may refer to the case of Saturn's rings. It is demonstrated by mathematicians that the stability of the motions of those rings cannot exist unless the parts at different distances from the center of Saturn, have different angular velocities.[29]

But observation[30] shows that the stability of the motion of the Rings exists. We hence conclude that those parts of the Rings situated at different distances from the center of Saturn do have different angular velocities. But it is concluded from observation that the rings of Saturn are liquid;[31] we hence conclude that there is still greater necessity for the different parts of a gaseous ring situated at different distances from the center of attraction, to have different angular velocities; just as we have concluded in speaking of the separation of the rings from the equatorial parts of the great solar spheroid.

36. In the case of the first two or three rings abandoned by the primitive spheroid, their density would be relatively so small that the velocity of the outer parts would be regulated approximately by Kepler's third law. Such being the case, it is probable that the velocity of the inner parts will exceed that of the outer parts. When the rings break up to form planets, the parts will in a great measure retain the velocity which they had in the complete rings, and the consequence will be that the rotation of such planets will, at first, be performed in a direction opposite to that of their motions around the central body.[32] In consequence of the great friction of the different strata of the fluid planetary rings abandoned by the solar spheroid, the outer strata would, in most cases, probably, have a greater absolute velocity than the inner strata; and the planets formed from them would naturally at first take the direct motion of rotation, because the external parts having a greater velocity than the inner parts, the excess of velocity would give the planet a direct rotatory velocity. Even if some of the outer planets should prove to have a retro-

[29] Maxwell, *On the Stability of the Motion of Saturn's Rings*, pp. 3 and 45; Peirce in Gould's *Astronomical Journal*, ii, 17–18.

[30] Observation seems to show that the Rings of Saturn are gradually approaching the body of the planet; but it by no means follows that they will ever come in contact with the body of the planet. They have existed too long for us to suppose there is any probability that the Rings are to be precipitated upon the body of the planet before our eyes, as it were.

[31] See *Annual of Scientific Discovery* for 1852, p. 377.

[32] Professor Kirkwood has pointed this out in his article in this Journal, [2], xxxviii, 2-4; and he has also attempted to show that in certain cases the rotation will become direct from the action of other members of the System. See also, volume xxxvii, page 51, where Prof. Hinrichs has given a formula indicating the same thing that we have arrived at in the text.

grade rotatory motion, our reasoning would not lead us to conclude that such would be the case with many of them.

37. But are we certain that all the rings would break up; or if broken up, that each would form a single planet? In this part of our enquiry we cannot guess at it; nor will it do to be guided by the phenomena of the Solar System; for it is the phenomena of such a system of which we are to render a rational account. We must here be guided by strictly mathematical and philosophical deductions. Prof. Peirce, of Harvard University, in his investigation of the problem of the stability of the motions of Saturn's Rings, arrived at the remarkable conclusion that the dynamical equilibrium of the rings is preserved by the sustaining effect of the satellites in the very act of perturbation. He then makes the remark that the only place in the Solar System, among the primary planets, where we could, from the above conclusions, expect a permanent ring, is just within the powerful masses of Jupiter and Saturn.[33] But even here, he says, the ring could not exist, but must ultimately be destroyed. He says, "But had there been a ring at this part of the system, it must have been subject to such extraordinary perturbations that it would, in the course of time, have been vibrated up against the next inferior planet, *Mars;* and in this way have been broken into the asteroids. The orbits of planets, formed under such circumstances, must have been characterized by great eccentricity."

The above view of the formation of the Asteroids, needs some modification. According to Peirce's conclusions, drawn from his investigation of the problem of Saturn's Rings, a fluid ring might, perhaps, exist for some considerable length of time, within the orbit of Jupiter. Granting this to be true, it is difficult to escape the conclusion that the process of cooling, to which the ring would be subject, even if it were gaseous, would gradually reduce it to a condition approximating to that of the rings of Saturn at the present time; that is, a liquid state. The various conclusions, then, at which mathematicians have arrived, respecting the perturbations of those rings, will apply with greater or less force, according to circumstances, to the supposed ring within the orbit of Jupiter. Prof. Maxwell,[34] in his investigation of the problem of the motion of Saturn's Rings, discussed the case of a solid ring, a ring of disconnected particles, and a fluid (or more definitely a liquid,) ring. The first case he found to be one of unstable equilibrium. In the last two cases he found that if the perturbations to which the ring would be sub-

[33] Gould's Ast. Journal, vol. ii, p. 18. Also Annual of Scientific Discovery for 1852, p. 379.

[34] On the stability of the motion of Saturn's rings. An essay which obtained the Adams Prize for the year 1856, in the University of Cambridge, Eng. By J. Clerk Maxwell, M. A.

ject could be propagated around the ring in the form of waves, subject to particular conditions, the ring would be permanent; but if these particular conditions were not fulfilled, the ring would be destroyed. In the case of a fluid ring, (incompressible,) he found that in general a wide ring could not revolve every part of which had the same angular velocity."[35] In his recapitulation[36] he says, "We next took up the case of a flattened ring, composed of incompressible fluid, and moving with uniform angular velocity. The internal forces here arise partly from attraction and partly from fluid pressure. We began by taking the case of an infinite stratum of fluid affected by regular waves. I found the accurate values of the forces in this case. For long waves, the resultant force is in the same direction as the displacement, reaching a maximum for waves whose length is about ten times the thickness of the stratum. For waves about five times as long as the stratum is thick, there is no resultant force; and for shorter waves, the force is in the opposite direction to the displacement.

39. "Applying these results to the case of the ring, we find that it will be destroyed by the long waves unless the fluid is less than $\frac{1}{4\frac{1}{2}}$d of the density of the planet, and that in all cases the short waves will break up the ring into small satellites.

40. "Passing to the case of *narrow* rings, we should find a somewhat larger maximum density, but we should still find that very short waves produce forces in the direction opposite to the displacement, and that therefore, as already explained, these short undulations would increase in magnitude without being propagated along the ring, till they had broken up the fluid filament into drops. These drops may or may not fulfill the condition formerly given [in the essay] for the stability of a ring of equal satellites. If they fulfill the conditions, they will move as a permanent ring. If they do not, short waves will arise and be propagated among the satellites, with ever increasing magnitude, till a sufficient number of drops have been brought into collision, so as to unite and form a smaller number of larger drops, which may be capable of revolving as a permanent ring."

41. Basing our reasoning on the preceding results, we are led to the conclusion that under certain conditions—such as probably exist within the orbits of Jupiter and Saturn in the Solar System—the abandoned fluid ring may preserve its form for immense ages, and thus have time to cool down somewhat and approximate to the condition of an incompressible fluid, and then, with the changes already taken place, undergo the necessary transformation, to convert it into small, separate bodies. These bodies would continue to come into collision until the conditions of dynamical equilibrium were fully established, and thus en-

[35] Maxwell, p. 45. [36] Ib., p. 64.

able each body to move in its own independent orbit. Bodies so formed must vary in size from the largest of them to bodies too minute to be visible through our most powerful telescopes.[37] The original ring would necessarily have considerable width, and since it would be prevented from uniting into a single planet, any such zone of small planets would also be of considerable width, and the planets would range in mean distance from one limit of the broken ring to the opposite limit. The various disturbing causes would very probably cause the small planetary bodies to move in very eccentric (planetary) orbits, and the action of the short waves which would break up the ring, might, perhaps, cause the resulting planets to move in orbits considerably inclined to each other, and to the plane of the ecliptic.

42. The heterogeneousness of the materials composing the outer rings, and the great rarity of their physical condition, would, perhaps, cause some of the less dense portions, at the time of the breaking-up of the rings, owing to the action of the great disturbing forces to which their motions would be subject, to be projected to a considerable distance from either the outer or the inner parts of the rings, and such detached portions might never return to the parent masses, but would move around the central solar body in an elliptical orbit, having, perhaps, in some cases, considerable inclination to the plane of the equator of the rings from which they were projected. Such bodies would revolve around the sun as *Comets*. The theory of Central Forces[38] shows us that, when the distances from the central body and the initial velocity are given, the eccentricity is dependent on the angle of projection; that is, the angle formed by the radius-vector and the line of projection. Supposing comets to have been formed as we have just described, we see that the great eccentricity of their orbits results from the direction with respect to the radius-vector, in which the mass is thrown from the ring. Such comets would very probably move around the sun in the same direction as the planets. Those rings abandoned nearer the centre of the solar spheroid, would, very likely, be more symmetrical in the disposition of their materials, and consequently less likely to have any large collection of rare materials that could be projected from the rings. We hence conclude that those comets which have a comparatively short period of revolution, will not be very large and conspicuous objects when they approach their perihelia. We may further conclude that the last three or four rings would not throw off any comets. We hence conclude that periodic comets having a direct motion should be found in groups, perhaps, with periods of revolution a little greater or a little less than the planets to which they

[37] May we not in this way account for the existence of meteoric rings?

[38] See Math. Monthly, ii, 160.

nearest correspond in their motions. Retrograde comets, and a portion of those which have a direct motion, cannot very well be accounted for upon this hypothesis of the breaking-up of the rings, nor should we expect to find retrograde comets with short periods. If, as Laplace supposed, and which, according to our hypothesis, is not improbable, comparatively small quantities of nebulous matter exist in different regions of the universe, the Solar System may, in its motion through space, come into their immediate neighborhood at different times, and such nebulous matter would be drawn, by the attractive influence of the sun and planets, into the system, and it would appear in the form of comets—either retrograde or direct, according to circumstances—and meteors. All comets so formed would necessarily have a comparatively long period of revolution, unless (which might be the case in some instances) the perturbative influence of the planets should change their orbits. We may hence conclude that all, or nearly all, comets having a short period, will have a direct motion; and of the comets of long periods, the direct and retrograde should, considering all time, be nearly equally divided, so far as we can determine.

2. *The Planets and their Satellites.*

43. After the rings from which the planets were formed were broken up, and all the parts of any one ring reduced to a planet, such planet, as we have shown, would have a rotation on an axis. This axis would, necessarily, be a *natural* or *principal axis.* It would be a delicate adjustment to give a solid body, differing from a sphere, a rotation around a principal axis; but a fluid body would readily adapt itself to such an axis. We do not doubt but that Infinite Wisdom and Power could cause any solid body to rotate around a natural axis; but we have not the least evidence that Infinite Wisdom ever works in that special manner. God always adapts means to ends, so that all things are produced under the action of fixed natural laws. We, therefore, conclude that the planets, formed as required by the Nebular Hypothesis, should all be found to rotate on *natural axes.*

44. After the parts of the broken-up rings had united, and the resulting bodies commenced their rotatory motion, the cooling of the masses, in consequence of the radiation of heat, would cause their rotatory velocities to be increased, and this process would continue till, in most instances, a secondary ring would be abandoned by such planetary bodies.[39] The outer planets being larger and much less dense than the inner ones, would abandon *several* secondary rings. In reference to the number of these secondary rings cast off by each of the planets, all that we can

[39] We have attempted here to be guided by strictly reasonable conclusions, without any reference to the phenomena of the Solar System.

at present conclude on, with any degree of certainty, is, that the number should, upon the whole, increase from the sun outward. We cannot, at present, say that the outermost planet ought to have abandoned the greatest number of rings. These secondary rings would, in general, break up and form secondary planets, or satellites. Under certain conditions—such as Prof. Peirce has found to exist in the System of Saturn—a ring, or rings might remain entire, or at least, not break up into a single satellite. According to this view, rings, if they exist, must be found interior to several satellites. In every case—unless changed by disturbing forces—the planet should rotate on its axis in less time than is required for any ring or satellite to revolve around the primary. The same must hold in the case of the sun and the planets.

45. When the last ring has been abandoned by any one of the planets, the remaining part[40] must cool down and thus form a primary planet. The outer portions being exposed to the low temperature of space—at least 50° below zero Fah.—they will cool much more rapidly than the inner parts, and after the whole is reduced to a liquid, a crust would, comparatively soon, form around the liquid planet, and further radiation would take place very slowly compared with its former rate. A certain amount of heat would be received from the other bodies, particularly the sun, (whatever might be its condition), and finally a balance would be reached beyond which the planet would not cool,—or at most very slowly,—and thus the inner parts would remain liquid. The outer planets being from the beginning less dense than the others, a crust would, perhaps, be formed so comparatively early as to leave the mean density of the planet comparatively small. We should, therefore, look for an increase in mean density from the outermost planet to the innermost. We see no reason, however, to believe that any simple law regulates this variation of mean density from one planet to another.

46. It would at first seem as if the satellites of the primaries should follow the same law of rotation that the primaries themselves do; but we must recollect that the numbers representing the distances of the former, expressed in radii of the latter, may differ very considerably from the numbers representing the distances of the latter expressed in radii of the sun. Again, the rings from which the satellites were formed, were abandoned when the primaries were much reduced in temperature, and condensed, when compared with the condition of the primary rings

[40] In the Nebular Hypothesis, after a fluid body has abandoned all the rings possible, there *must* remain a central body of comparatively large dimensions, and of a mass far greater than the sum of the masses of all the rings separated. How would the author of the meteoric theory as given in the 204th number of the North American Review, account for the fact that our Solar System is constructed upon this principle?

when they were separated from the primitive solar spheroid. All the satellites will be much smaller than the primaries; and, being comparatively small bodies, they will cool down and become solid much sooner than the primaries will. In consequence of the forces acting on the planets, both primaries and secondaries, they will become spheroidal in shape as soon as the rings from which they are made are broken up and the parts united. The attraction of the primaries on the fluid satellites, will raise the fluid particles into great tidal-waves; and as the waves on opposite sides are slightly unequal, the result will be a retardation of the rotatory velocity of the secondaries. William Ferrel, Esq., of the Nautical Almanac Office, Cambridge, Mass., has given[41] a mathematical discussion of the retarding effect of the tides of the earth on its rotatory velocity. According to his conclusions, the retarding effect of the earth upon the moon's rotatory velocity, both acting under similar circumstances, would be between 500 and 600 times as great as that of the moon on the earth's. According to his results, it would amount to an entire circumference of the moon, or one period of rotation of the moon in a hundred years. Even if these numbers be wide of the truth, the investigation shows us that, in general, the retarding effect of the attraction of the primaries on their secondaries, when in a fluid state, is sufficient, in the course of immense ages, to reduce the periods of revolution and rotation of the latter to isochronism. We have, therefore, great reason to suppose that as a general rule the satellites turn on their axes but once during a revolution around their primaries.

47. If there exists a cosmical ether, as is at present pretty generally admitted, in order that it may remain spread throughout universal space, it is only necessary for it to possess an elasticity so great that the action of luminous bodies is sufficient to produce a mechanical action in it that will enable it to maintain its temperature and fluid condition under all circumstances. This cosmical ether being material in its nature, it would necessarily partake of the motion of those bodies with which it remains in contact for immense ages of time. In the Solar System, the motion of the ether around the sun would be in the general direction of all the planets. It would also be regulated in its motion

[41] Gould's Ast. Journal, iii, 138–141. Professor Ferrel informs me that some numbers which he employed he now knows to be about ten times too large, and hence his results should be diminished in nearly the same ratio. He found that in one century the moon retarded the rotatory motion of the earth's equator 87·44 miles. Dividing this by 10, to correspond more nearly with more recent data, we have about 3½ miles for the retarding effect of the moon in a century. The effect of the earth on the moon he found to be 562½ times as great as that of the moon on the earth; and this will give—since the diameter of the moon is only about one-fourth of the diameter of the earth—between 500 and 600 miles for the retarding effect of the earth upon the moon's equater, if it be only partly fluid. If it be wholly fluid, it would be somewhere near 1000 miles per century.

partly according to the third law of Kepler; and thus the planets, whatever might be their distances from the sun, would scarcely be retarded by it in their annual revolutions.[42] Since the ether is supposed to maintain its temperature, and therefore elastic force, it would not gather into separate collections, leaving some parts without it; but it would only be slightly condensed around the different bodies of the universe.

48. If we calculate the principal radii of gyration of Jupiter, at the time when each secondary ring was abandoned, upon the same principle as that adopted in the case of the sun, viz: by neglecting the masses of the satellites, we shall find such radii, at the time when each satellite ring was separated, commencing with the nearest and taking them in the order of distance—the equatorial radius of Jupiter being the unit—to be as follows:

TABLE IV.

$k_1 = 1{\cdot}308;$	$a_1 = 6{\cdot}048;$
$k_2 = 1{\cdot}832;$	$a_2 = 9{\cdot}623;$
$k_3 = 2{\cdot}630;$	$a_3 = 15{\cdot}350;$
$k_4 = 4{\cdot}018;$	$a_4 = 26{\cdot}998;$

a_1, &c., being the distances of the satellites. If we compare the radii of gyration, k, with the mean distance of the satellites, a, we see that in every instance the latter is not far from six times the former. If we make a similar comparison for the primary planets, using Table II, we see that the radius of gyration is only about the one-hundredth of the mean distance of the planet from the center of the sun. We hence conclude from this that the density of the primitive solar spheroid increased much more rapidly toward the center, than the primitive Jovian spheroid. In other words, the solar spheroid was relatively much more dense about the central parts than Jupiter. This we had reason to expect, since, according to the hypothesis, the ring from which the Jovian system was formed was already somewhat condensed when it was abandoned, and the general density much more nearly uniform than that of the solar spheroid; and hence the secondary rings were much denser relatively to the Jovian spheroid than was the ring from which Jupiter was formed relatively to the solar spheroid. This, it would seem, is a strong proof of the truth of the nebular hypothesis. The mass of Jupiter is 6000 times as great as the sum of the masses of his satellites; so that neglecting the masses of the satellites in comparison with the mass of Jupiter we neglected only the small fraction $\frac{1}{6000}$th. I supposed Jupiter homogeneous in finding his principal radius of gyration.

The periods of rotation of Uranus and of Neptune being un-

[42] This conclusion differs from the basis assumed by Prof. Hinrichs in his article on the Age of the Planets, this Journ., [2], xxxvii, 36-56.

known, we cannot compute the radii of gyration of the planets at the time when the satellite rings were abandoned, as we have done in the case of Jupiter. And if Bessel's mass of the rings of Saturn ($\frac{1}{11\frac{1}{8}}$th of that of Saturn) be admitted, it is probable that this, and the sum of the masses of the satellites, will make a fraction too great to be neglected. The same is true of the earth and moon.

49. Let us now collect together in this article the various facts which ought to be found to exist in the Solar System, according to the preceding view of the nebular hypothesis.

1st. The planets should all move in the same direction around the sun. 2nd. Some comets whose distances correspond with those of the planets within moderate limits, should have the same direction as the planets. 3d. The orbits of the planets should not be much inclined to each other. 4th. All the planets, except, perhaps, a few of the outer ones, should turn on their axes in the same direction as that in which they move around the sun. 5th. The satellites should revolve around their primaries in the same direction in which the latter turn on their axes. 6th. The periods of revolution and rotation of the satellites, should, in many cases, at least, be equal. 7th. The sun must rotate on an axis in the same direction as that in which the planets revolve around him. 8th. The outer planets of the system should be larger than the inner ones. 9th. The outer planets should have the greater number of satellites. 10th. The outer planets should rotate on their axes in less time than the inner ones. 11th. The outer planets should in general have greater masses than the inner ones. 12th. The planets should, upon the whole, increase in density as they are found nearer the sun. 13th. The satellites should be less than their primary, and the planets less than the sun, both in bulk and mass. 14th. All the members of the Solar system should be composed of similar materials.

50. We have thus, in the preceding pages, attempted to give a connected view of the nebular hypothesis, uncompared, except in a few cases, with the real phenomena of nature. In what follows, we shall show that the phenomena of the sidereal heavens, and of the Solar System, agree very closely with the preceding deductions.

3. *A Comparison of the preceding Theory with the Phenomena of Nature.*

51. The origin of material existence is at present a mystery to the human mind. To say that it was created by the Deity is an assertion that conveys to the understanding something of which we have no definite conception. On the other hand, to say that it has always existed is equally unsatisfactory, since the human mind always looks toward a beginning of everything.

But with such speculations, we have here nothing to do. The nebular hypothesis supposes matter and all physical forces to exist; and we have only to carry back our speculations to the most ancient and chaotic state in which it is possible for us to suppose matter to exist. I have supposed matter in its earlier chaotic condition to have been necessarily heterogeneous in structure. Others have different views.[43] Even if it[44] were at first, or at any time, perfectly homogeneous, but not of symmetrical form, it would soon become heterogeneous, because various centers of attraction would be established, around which matter would accumulate; and thus the change in the different parts would generate a rotatory motion. Thus, under almost any conceivable—certainly probable—circumstances, the nebulous mass must be considered as having sufficient potential energy to give it a motion of rotation. But we must not be confined to these speculative views. "Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate. For instance, the minute particles of dew which whiten the grass blade in early morn, are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form."[45] "It is remarkable of physical laws that we see them operating on every kind of scale as to magnitude, with the same regularity and perseverance. . . . . Two eddies in a stream fall into a mutual revolution at the distance of a couple of inches, through the same cause that makes a pair of suns link in mutual

[43] "I regard the gaseous as not the *ultimate* form of matter, but that in its ultimate state it was *absolutely* imponderable and perfectly elastic, and therefore it completely filled, absolutely, infinite space, and that the myriads of Solar vortices were developed out of this perfectly homogeneous imponderable fluid in a manner as definite and orderly as that which *regulates the production* of a plant or an animal on the earth."—*Dr. Joel E. Hendricks.*

[44] Some writers seem to doubt the existence of nebulous matter, because the telescope continues to resolve the nebulæ. But comets are evidently in a nebulous state, and the existence of these shows us that matter may assume the nebulous condition. It is a fact that the evidence derived from observation, that nebulous matter, in large quantities, exists, is by no means conclusive. In fact, the truth of the nebular hypothesis cannot depend on our knowledge of the existence of nebulous matter in large quantities. It is sufficient to show that matter in such a state can and does exist in *any* quantity, and after that we must look to the general and particular phenomena of nature. It is scarcely probable that we shall ever learn, at least for ages to come, directly from observation, that large quantities of matter exist in a nebulous condition. But the general appearance of starry clusters, may indicate the origin whence they were derived. The appearance and arrangement of the planetary system are more conclusive in reference to the origin whence that system was derived.

[45] Mr. Nasmyth. See Annual of Scientific Discovery for 1857, p. 187.

revolution at the distance of millions of miles. There is, we might say, a sublime simplicity in this indifference of the grand regulations to the vastness or the minuteness of the field of their operations."[46] We thus may learn from the minuter operations of nature of those grand revolutions which we have reason to conclude have taken place in past ages of duration. Binary, ternary, and multiple stars, in some, perhaps many, cases, may be the resulting motion of two or more stars—I should more properly say primitive stellar spheroids, coming so near together that their atmospheres came in contact and thus gave them a motion around their common center of gravity.[47]

52. All apparent motion in the region of the so-called fixed stars, is very slow; so slow, indeed, that in most instances it requires several years to elapse before *any* motion becomes apparent. Certainly, in most cases the nebulæ are situated far beyond the region of the visible stars. How much slower, then, than the stars, must the nebulæ appear to change their relative parts or positions in the heavens! We must, therefore, expect little from the motion of nebulæ or even clusters, in confirmation of the nebular hypothesis. We must look to the general conformation of nebulæ and of clusters. Lord Rosse has within a comparatively few years, by means of his great reflectors, shown that many of the nebulæ are of the *spiral* form. They appear as if they possess a rotatory motion on an axis, and this motion has so far increased as to project some parts of the nebulæ tangentially, and thus to break up the spheroidal form, causing parts of the nebulæ to fly off in one or more streams. The general principles which would lead to such a result, we have attempted to give from Arts. 3 to 12 inclusive. Mr. Nasmyth has also given[48] an exposition of the principles, though he has not based his explanations on mathematical formulæ and deductions as I have attempted to do. Many nebulæ present that appearance which we suppose our Solar System to have presented before the first planetary ring was abandoned, viz: a projected circular or elliptic appearance, not very bright in the outer portions, but increasing in luminosity with considerable rapidity near the center.

53. Some observations and deductions of a very interesting character, have recently been made. Many years ago, Sir William Herschel concluded, from a comparison of a figure of the nebula in Orion, made by Huygens many years before, with a figure of his own drawing, that some parts of the great nebula —(cluster, perhaps we should now say, since it has been par-

[46] Vestiges of Creation. Harper's Ed., p. 17.

[47] I am unable to ascertain in what direction, if any definite one, double stars revolve around their common center of gravity. Who will give the required information? It is worthy of attention.

[48] Ann. of Sci. Disc. for 1852, pp. 187-8.

tially resolved since Herschel's day)—have changed. It is very probable that no such change has taken place; but that the supposed variation came from a defect in Huygen's representation, and from a difference in the telescopes employed. But it seems now quite certain that some nebulæ have changed. The nebula around *Eta Argus*, seems to be one of that character.[49] Also several others have been discovered in which some change seems to have taken place. Accurate micrometrical measurements and photographic representations only can here serve for the basis of positive knowledge in this interesting department of astronomy. It would seem that such patches of nebulæ, if, indeed, they *are* nebulæ, proper, cannot be very extensive, since they must be comparatively near our system. We must now leave this interesting part of our subject, since this, alone, could never, so far as we can see, furnish us the necessary data to enable us to say whether the God of Nature followed one or another method in the process of world-making. We must descend to the general and some of the particular phenomena of the Solar System, to enable us to draw a definite conclusion on this subject.

54. We know from observation that all the primary planets revolve around the sun in the same direction, and in nearly the same plane. The sun rotates on an axis in the same direction in which the planets revolve around him, from west to east. The period of the sun's rotation is about $25\frac{1}{3}$ days, which is about $\frac{7}{24}$ths of the periodic time of Mercury. According to our theory, no planet will ever be discovered so near the sun that its period of revolution will be less than the period of the sun, viz: $25\frac{1}{3}$ days, if this number be accurately determined. This is contrary to the supposed discovery of Mr. Lescarbault.

The inclination of the orbit of Neptune, as we have shown, corresponds very approximately with that of the invariable plane of the Solar System, as our theory requires. We also find that the distance between the planetary orbits increases with the distance from the sun. The following table will show

TABLE V.

Planet.	Mean dist.	Dist. to next Ex. Orbit.	Diam. of sphere of attraction.
Mercury,.....	0·3870981	0·336235	0·200979
Venus,.....	0·7233306	0·2766684	0·383390
Earth,.....	1·0000000	0·5236923	0·521348
Mars,.....	1·5236923	1·544983	0·779537
Ast. Planet,.....	3·068675	2·134101	0·968693
Jupiter,.....	5·202776	4·336010	4·876551
Saturn,.....	9·588786	9·643604	8·618608
Uranus,.....	19·18239	10·857110	7·437871
Neptune, ...	30·03950		

[49] This Journal, [2], xxxvii, 294-5.

the difference between the mean distances of the planets, in terms of the mean distance between the earth and sun; and also the breadth of the primitive rings, according to Kirkwood's Analogy.[50] We have at present no method probably more accurate than Kirkwood's to determine the breadth of the primitive rings.

We thus see that, generally speaking, there is an increase of distance between the orbits of the planets, and also in the breadth of the rings abandoned by the sun; and yet it is not absolutely the case in every instance; nor is the increase according to any simple law, as we had occasion to point out from what was assumed to be the probable condition of the primitive solar spheroid.

55. If we compare the masses and the densities of the planets, we shall see here, also, a general increase in the masses, and a decrease in the densities, as we get farther from the sun, as can be seen in the following table:

TABLE VI.

Planet.	Magnitude.	Mass.	Density.
Mercury,.....	0·0595	0·0729	1·225
Venus,.....	0·9960	0·9101	0·908
Earth,.....	1·0000	1·0000	1·000
Mars,.....	0·1364	0·1324	0·972
Ast. Planet,.....		0·2869 [51]	
Jupiter,.....	1491·0000	338·7180	0·227
Saturn,.....	772·0000	101·3640	0·131
Uranus,.....	86·5000	14·2510	0·167
Neptune,.....	76·6000	18·9000	0·321

We see from this table what was concluded from theory to be very probable, viz: that in general the volumes and masses increase from the sun to a maximum—which is reached in Jupiter—and then decrease to the extremity of the system. The fact that there is no simple law perceptible which regulates the increase and decrease of the numbers in the two preceding tables, shows that the condition of the primitive solar spheroid was far from being symmetrical. The fact that the earth is more dense than Venus may be thus explained. The difference in the condition of the Earth-ring and the Venus-ring could not have been very considerable. But the former, after it united into a single spheroid, threw off a satellite-ring, taking off the rarer part and leaving the denser, so that the ultimate mean density of the earth became a little greater than that of Venus. There seems to be some peculiarity about Saturn in respect to density that cannot be easily accounted for.

[50] This Journal, [2], xiv, 213.

[51] According to Kirkwood's Analogy, this Journal, [2], xiv, 213.

56. In examining the secondary planets, we see a general increase in number to each primary planet, as we go from the sun to the extremity of the system. The Earth has 1, Jupiter 4, Saturn 8, Uranus 6 and perhaps 8, Neptune 2 that have probably been seen. We cannot tell with certainty the number of satellites belonging to each of the last two planets, owing to the difficulty of observing such bodies at so remote distances. The satellites of Jupiter are distributed in the Jovian system very similarly to the primary planets in the Solar System. We find the masses to increase as the distance from the center of Jupiter increases, till we arrive at the third satellite, where we reach the maximum. The fourth satellite is the second in mass. The third satellite is also the largest, and the fourth the next in size, but the second, although it is of greater mass, yet is of smaller size than the first. The satellites of Saturn follow a similar general law. *Titan*, the 6th in the order of distance from the primary, is the largest, and *Japetus*, the most distant of them all, is next in size. Of the others we cannot so well judge, but it seems not improbable that they decrease in size as they are nearer the primary. We know still less of the satellites of Uranus, but the second and fourth, so-called, are very probably the largest. But within the second, (in the order of distance according to Sir William Herschel's discoveries), two, and possibly three, exist. We thus see it to be a general principle of the planetary system that the largest bodies are within some, and without some of the orbits of the others. We also observe this fact in reference to some of the secondary planets, viz: if we divide the distance of the remotest satellite of any primary, from the center of the planet, by the diameter of the sphere of attraction (which we shall consider to be the same as the breadth of the ring, or the diameter of the primitive planet,) we shall find that, in general, the quotient decreases from the earth to Uranus. We may therefore conclude that the outer planets had to condense much more than the inner ones before a satellite-ring was abandoned. It is a fact, also, that, so far as observation has determined, the satellites rotate on their axes in the same direction and in the same time in which they revolve around their respective primaries. All these motions are in the direction of the rotation of the primaries.

57. The rings of Saturn offer a living example of the primitive secondary rings. They open to us, in a measure, the nature and constitution of the primitive rings, both the primary and secondary. These rings have been retained as such, according to Prof. Peirce, by the attraction of the satellites. Should these rings entirely break up, they would probably form asteroid satellites. Since the rings of Saturn are very thin in comparison with their width, we conclude that the primitive planet was very

much flattened about the poles. If the rings of Saturn are fluid, they exist as such in consequence of the friction of their particles.

58. It is an interesting fact, and one confirmatory of our theory, that the rings of Saturn have a longer period of rotation than the planet itself, as required by the nebular hypothesis. According to the observations of Sir William Herschel, the rings of Saturn revolve around the planet in the space of 10^h 32^m 15^s.[52] The period of rotation of Saturn is 10^h 29^m 17^s.[53] We thus see that the rings require 2^m 58^s longer time to revolve about Saturn, than the latter does to turn on his axis. Here again we appear to have obtained conclusive evidence in favor of the nebular hypothesis. Had the period of Saturn's rotation been greater than that of the rings, it would have been very difficult to reconcile it with the nebular hypothesis.

59. The rings of Saturn are either fluid or composed of unconnected particles. According to Maxwell, even a fluid ring would be broken up into small satellites; but it does not follow that these satellites may not still be fluid and unite again into a ring—or perhaps several nearly concentric rings—after having been separate a sufficient length of time to restore the equilibrium by counteracting the disturbances to which the rings are subject. We are thus reduced to the very probable conclusion that the rings of Saturn are fluid (liquid). We are thus carried back one great step toward the gaseous condition from which we have supposed all the planets and their satellites to have been developed.

60. Observation shows that the satellites of Uranus revolve in a retrograde order. But we do not know in what direction, nor in what time, the planet rotates. If it should finally prove to be the case that the satellites of Uranus are in the plane of the planet's equator, and revolve in a direction opposite to that of the rotation of the planet, it will be very difficult, if not impossible, to reconcile the two motions with the nebular hypothesis. But should the axis of the planet be found to have an inclination to the plane of the orbits of the satellites differing considerably from a right-angle, the difficulty will not be insuperable.[54] But since the nebular hypothesis accounts so satisfactorily for so many of the phenomena of the solar system, we may venture to predict that the rotation of the planet will be found to be retrograde, or closely approaching that direction, if we may so express ourselves. But our theory, as we have

[52] Grant's Hist. of Phys. Astr., p. 262.

[53] Herschel's Outlines, Art. 514.

[54] By giving an artificial globe a motion of rotation on its axis, and then reversing the poles, we can easily see how readily the direction of rotation is changed to the opposite direction. The same primitive impetus carries it in the opposite direction.

shown, requires that the outer planets rotate in a retrograde direction. Kirkwood's Analogy seems to require that Uranus should occupy about 37 hours in performing one rotation. Is it possible that a retrograde motion will influence the time of rotation of the planet? It would seem that some of the great telescopes, either in this country or in Europe, ought to be able to settle the question of the rotation of Uranus. I have not attempted to account for the great difference between the greatest and least diameters of Mars. A mere supposition could be of no advantage.

61. The following table gives the time of rotation of the several planets so far as known:

TABLE VII.

Planet.	Time of rotation.			Diam. of sphere of attraction.
	h.	m.	s.	
Mercury, ...	24	5	28	0·200979
Venus, ...	23	21	21	0·383390
Earth, ...	23	56	4	0·521348
Mars, ...	24	37	22	0·779537
Jupiter, ...	9	55	26	4·876551
Saturn, ...	10	29	17	8·618608

By the above table we see that, roughly speaking, the time of rotation diminishes as the sphere of attraction increases. The original impulse of rotation communicated to a planet, by the ring from which it was formed, at the time of its breaking-up, would be dependent on the difference between the velocity of those parts of the ring lying upon the opposite sides of the orbit of the future planet; or, in other words, upon the difference of the living force of each part. This difference would not depend on the absolute velocity of either part, but rather upon the relative distance of the two parts from the sun. We hence see that since the outer primitive rings were wider than the inner ones, we should expect a more rapid rotatory motion in the outer planets.[55] Since Saturn has the greatest sphere of attraction we should at first expect that planet to have the shortest period of rotation. But such is not necessarily the case. If this whole planetary ring had condensed into a single body, such might and probably would have been the case. But every time a planet abandoned a ring to form a satellite, it, in effect, lengthened the period of rotation of the future planetary spheroid. Jupiter abandoned only four rings of which we have any knowledge, and the united mass of them forms only the $\frac{1}{6000}$th of the mass of Jupiter; while Saturn abandoned no fewer than eleven rings, three of which still remain. According to Bessel, the mass of

[55] Since the outer rings were less dense than the inner ones, the planets would have farther to contract than the inner ones, and this also would increase the rotatory velocity.

the remaining rings alone equals the $\frac{1}{1\,1\,5}$th of the mass of Saturn. Even now the period of Saturn's rotation exceeds that of Jupiter by only $3^m\ 51^s$.

62. Humboldt has called attention[86] to the fact that the group of Asteroids divides the planets into two classes, those within and those without the zone of small planets, each class being marked by certain distinctive features. The outer group, "Neptune, Uranus, Saturn and Jupiter, are distinguished by considerable volume and slight density; the time of rotation about their axes is about ten hours, and from this there results in these bodies considerable flattening; moreover, of the 22 satellites of the solar system, 21 pertain to this group. The four *inferior* planets, Mars, the Earth, Venus and Mercury, have, on the contrary, much smaller volumes and much greater density; they revolve on themselves in nearly twenty-four hours, are but little flattened, and possess, among all the four, but one satellite, the Moon." Let us now see what account we can give of these peculiarities. We have already shown that according to Prof. Peirce's deductions a planetary-ring might exist for a great length of time just within the orbit of Jupiter; the same influence that would sustain a ring where the asteroids now exist, would tend to sustain one where Mars now revolves; and the same influence would extend to the orbits of the Earth and of Venus; and each of these latter bodies would lend their influence, and would thus influence the Mercurial-ring. These interior rings (as we will call them, being interior to the Asteroids,) being thus sustained for a great length of time, would loose much of their heat, and thus become condensed considerably perhaps, as have the rings of Saturn. When such rings broke up and assumed the spheroidal form, they would be much less likely to abandon satellite rings, than those planets whose rarity was greater. We have already shown that the mean density of those planets nearer the sun should be found to be greater than that of the outer planets; we now refer to the influence above mentioned to account for the sudden increase of mean density within the orbit of Jupiter; for evidently the mean density of a planet formed from a ring considerably condensed, would be greater than one formed from a ring less so; for a crust would form around one approximately as soon as around the other. The period of rotation of such planets would be greater than that of others, since they would not have so far to contract. The times of rotation would be approximately equal, since they would contract nearly equally.

63. The existence of 80 known Asteroids between the orbits of Mars and Jupiter, seems to confirm the view which we have given of their formation, in a preceding part of this paper. If we divide 360° by 80, we find an average of one Asteroid for

[86] Cosmos, iv, 422, Bohn's ed.

every $4\frac{1}{2}°$, making approximately a ring of these small bodies. But we cannot for a moment entertain the idea that nearly all these bodies have been discovered. They are very small and necessarily difficult to detect; and judging from what we already know of the size of these small bodies, we must conclude that many more exist, much the larger share of which are too small to be detected by our best telescopes. The most rational theory of meteoric stones, is that which regards them as minute asteroids. The whole group, many thousands very probably, perhaps millions—between the orbits of Mars and Jupiter, must have very eccentric orbits, and be subject to extraordinary perturbations in their motions, and it is highly probable that many of them become satellites of some of the planets, and finally come in contact with their surfaces as meteoric stones. It is certainly a little curious that the orbits of the known Asteroids intersect. The mean width of the whole zone so far as known, lies between the limits 2·145 (Feronia) and 3·452 (Maxamiliana) giving a breath of 1 307, which is rather greater than the diameter of the sphere of attraction of Kirkwood's Asteroid-planet.

At present we shall add nothing respecting comets, as Prof. Kirkwood has called attention, in several places, to the orbits of these bodies.

Hector, N. Y., Nov. 11, 1864.

ART. V.—*On Brushite, a new mineral occurring in Phosphatic Guano;* by GIDEON E. MOORE, Ph.B.

(Communicated to the California Academy of Sciences, Sept. 5th, 1864.)

IN the spring of the present year, I received, through the kindness of Wm. E. Brown, Esq., of Mare Island in this State, a specimen of a mineral discovered by him in a cargo of phosphatic guano at Camden, N. J. The locality from which it was derived is not known,[1] and, though letters of inquiry have been sent to the parties to whom the cargo was originally consigned, no reply has been received up to this date. The texture and appearance of the guano would, however, point to some one of the Carribean islands, and more particularly to the island of Sombrero as its probable source. It is very probable that the mineral may be recognized among the crystallized products occurring in other guano deposits.

In the specimen in my possession, the mineral occurs filling seams in the guano, varying from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch in width. The matrix itself is of the variety known as rock guano. It

[1] In a letter from Mr. Moore, dated San Francisco, Nov. 13th, 1864, he states that he has ascertained the locality of the new mineral to be Avis Island in the Carribean Sea.—EDS.

possesses an oölitic structure and a brownish white color, interspersed with small spots of pure white.

The mineral is in the form of small but very perfect and brilliant crystals, with a cleavage in the direction of their greatest length nearly equal to that of selenite, the laminæ being also slightly flexible, as in the case of the latter species. Hardness, 2·25. Specific gravity, 2·208 (mean of two determinations). Color yellowish white. Transparent. Lustre vitreous, splendent, inclining to pearly on the cleavage faces.

When heated in a closed tube before the blowpipe, it whitens and gives off water at an incipient red heat. In the platinum forceps, it fuses with intumescence at about 2 on von Kobell's scale, tinging the flame with the peculiar green characteristic of phosphoric acid. The button formed by fusion crystallizes on cooling, showing numerous brilliant facets. It readily dissolves, even in coarse crystals, in dilute nitric and chlorhydric acids.

A qualitative analysis revealed the presence of lime, phosphoric acid and water, with barely discernible traces of magnesia and alumina.

The quantity of mineral at my disposal was very small, scarcely exceeding one gram in weight. In each of the two following analyses, the water was determined in 0·2 gram, the remaining 0·3 gram being employed in the determination of the lime and phosphoric acid. The result was as follows:

	1.	2.
Lime, - - - -	32·65	32·73
Phosphoric acid, - -	41·50	41·32
Water, - - - -	26·33	26·40
	100·48	100·45

These figures agree exactly with the composition of the neutral tri-basic phosphate of lime, $2CaO, HO, PO_5$, with the addition of four equivalents of water of crystallization ($2CaO, HO, PO_5 + 4aq$), viz:

2CaO, - - - -	56·26	= 32·59
PO_5, - - - -	71·36	41·34
HO - - - - -	9·00	
4 aq, - - - -	36·00	26·07
	172·62	100·00

In the polarizing microscope, the mineral shows a vivid succession of colors. A sample has been sent to Prof. J. D. Dana, who has kindly undertaken the study of its crystallographic characters, and I hope in a short time to be able to communicate the results of his investigations to the Academy.

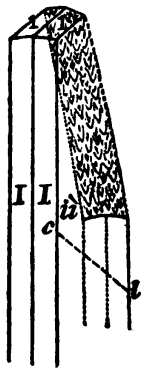
It is with great pleasure that I dedicate this species to Prof. G. J. Brush, of Yale College, to whose unwearied zeal and efficient labors American Mineralogy stands so deeply indebted.

San Francisco, Cal., Sept., 1864.

ART. VI.—*On the Crystallization of Brushite;* by JAMES D. DANA.

(Communicated to the California Academy of Sciences.)

THE crystals of the new mineral Brushite which I have had under examination were received from Mr. G. E. Moore, to whose chemical investigation science owes the first determination of the species.



The crystals are slender prisms, not over a third of an inch in length. A common form (containing all the occurring planes) is shown in the annexed figure. The prisms are monoclinic, and are often flattened parallel to the clinodiagonal, as here represented.

Cleavage is perfect parallel to the clinodiagonal section, or the plane $i\bar{i}$; also distinct, parallel to the lines cl, as apparent often in the cross fractures of crystals, and by occasional striæ. This plane of cleavage may be called the basal, or O.

The planes I and 1 are brilliant, especially the former. The oblique plane, situated on the back side in the figure, which may be called r, is quite rough, owing to an oscillatory combination of two hemi-octahedral planes. In many of the crystals, only the right one of the two planes I is present, and also only the left one of the two planes 1. The prisms frequently terminate above in an irregular edge made by the meeting of the one or two planes I and the rough plane r, and this edge is sometimes cut off, more or less deeply, by a single oblique plane, which is one of the planes 1.

According to measurements with the reflective goniometer—

$I : I = 142° 26'$	$1 : i\bar{i} = 101° 40'$
$I : i\bar{i} = 108\ 47$	$1 : 1 = 156\ 20$ (approximately.)

The inclination of 1 on 1 could not be accurately measured on account of the minuteness of the planes in the crystals in which both planes occur, and the want of perfection in the reflection. The angle obtained for $1 : i\bar{i}$ would give for 1 : 1 156° 40′.

By measurement with a goniometer attached to a compound microscope, the plane angle between the lines of cross cleavage, or cl, and the edge $I : I$ (which equals the inclination of O on the orthodiagonal section or the plane $i\breve{i}$) was found to be 117°–117½°; and that between edge $I : I$ and edge 1 : 1 (which equals $i\breve{i}$ on $1\breve{i}$, both unobserved planes), 95° to 95½°; whence, $O : 1\breve{i}$ would equal, approximately, 147° 30′. The inclination of the rough plane r on the edge 1 : 1 is about 110°, but varies much.

The results of calculation, taking as data the above-mentioned

angles $I:I$ and $1:i\dot{1}$, along with the inclination of O to $i\dot{i} =$ 117° 15′, and that of the edge 1 : 1 (or $1i$) to $i\dot{i} = 95°\ 15'$, are as follows:

$C\ (=O:i\dot{i}) = 117°\ 15'$ and $62°\ 45'$

a (vertical axis) : b (clinodiagonal) : $c = 0{\cdot}5396 : 1 : 2{\cdot}614$

$1:1 = 156°\ 46'$ $\quad -1:-1$ (unobserved planes) $= 164°\ 22'$

The species is related in form to Vivianite, in which

$$a : b : c = 1{\cdot}002 : 1 : 1{\cdot}3843;$$

for, if we double the a of Brushite, and halve the c, we have for the ratio of its axes—

$$2a : b : \tfrac{1}{2}c = 1{\cdot}0792 : 1 : 1{\cdot}307.$$

The two species are also alike in the perfect and pearly clinodiagonal cleavage.

ART. VII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

THE *nebular hypothesis*—the boldest thought that ever elevated the human mind, by bringing us, as it were, in sight of the mysterious fiat of the Almighty—was, in its great general features, unfolded almost at the same time by Germany's deepest thinker, the Königsberg philosopher, IMMANUEL KANT, and by PIERRE SIMON DE LAPLACE, the greatest mathematician of France. It is truly the closing stone in the philosophy of the celestial vault; for Copernicus and Kepler made us behold the foundation,—the first, by placing the sun as the lantern of the world in the center, and surrounding it with the planets—the second, by destroying the cycles and unravelling the harmony of the spheres in his immortal laws; and after the existing phenomena had thus been rightly viewed, Newton made us behold the invisible bond that connects the members of the system, while at length Kant and Laplace pointed out to us the hand that at "the beginning" projected these celestial balls into space and thereby insured the continued existence of the system.

But notwithstanding this noble parentage and its being the *logical sequence* of the discoveries in the theory of cosmos made by Copernicus, Kepler and Newton, the nebular theory enjoys as yet but slight consideration among astronomers. Arago[1] is the only one of these who has deigned to consider it earnestly, and he probably did so more in his capacity as a physicist than as an astronomer.

[1] Arago, Astronomie Populaire, ii, 7. Paris and Leipsic, 1855.

The reason of this neglect seems to be the incomplete state in which even Laplace himself left the theory. Direct observation, moreover, seemed to contradict some laws given as necessary consequences of this hypothesis.

We have already, in a former article,[2] tried to vindicate the theory in this last respect by showing that the hypothesis is really confirmed even in these apparently contradicting observations. We will now endeavor to give a somewhat more complete development to the fundamental principles of Kant and Laplace, and to exhibit the exact position of the nebular theory itself, hoping thereby to show that this theory, if we only study it earnestly and patiently both by experiment and analysis, fully deserves our confidence.

As this subject is as vast as it is difficult, we beg the critic always to keep in mind that we do not pretend to give a treatise, but merely offer an *introduction* to this almost new field of analysis.

We commence with a short survey of the fundamental principles and the aim of the theory of the solar system, in order clearly to understand why the nebular theory is necessary, what it will have to accomplish, and how far it already has done its duty.

§ 1. *The fundamental constants of the Solar system.*

As the discovery of a law of nature is but the reduction of the infinitude of observed quantities to a few constants by means of a function, the algebraic expression of the law—we see that *the progress of astronomy to a great extent must be identical with the reduction of the number of such constants.* This is fully borne out by the history of the science. For, while the Ptolemaic theory[3] of the planetary motion required the radii and inclinations of seventeen different circles to express the observed motions of Saturn, Kepler reduced this number of constants to three, the semi-major axis, eccentricity, and inclination of the orbit. This very principle is also placed by Laplace[4] at the head of his *Mécanique Céléste.*

We may therefore trust in this principle, and with Laplace try to reduce the number of indispensable constants. We must first, however, ascertain which are those constants that are now *considered* fundamental or indispensable.

The *constants of the solar system* now exclusively deduced from observation are:

1. The *mass*, m, of the planet.

[2] The density, rotation, and relative age of the planets: this Journal, Jan., 1864.

[3] Fracastor; see Bailly, Histoire de l'Astronomie moderne; vol. i. Paris, 1779. Eclaircissements, livre iv, § 23, and livre viii, § 27.

[4] Il importe extrêmement d'en bannir tout empirisme et de la réduire à n'emprunter de l'observation que les données indispensables.—*Méc. Cél.—Plan.*

2. The *figure.* On assuming a primitive fluidity and a small velocity of rotation, theory gives an ellipsoidal figure of the planets; hence not independent of observation.

3. The *ellipticity* of the planet. Theory may assign to it a higher and a lower limit by means of the mass (1) and the angular velocity (6)—but though the connection of this constant with others thereby is manifest, still its exact value can only be derived from observation.

4. The *volume*, or *diameter*, of a planet. Combining this constant with the first (mass) we obtain the *density.*

5. The *plane* (or *inclination* of the axis),

6. The *direction*, and

7. The *velocity of rotation.* Though this last element bears relation to others, still its exact value can only be obtained from observation. Even if Kirkwood's law of rotation[5] should prove to be perfectly exact, this element would continue to be a fundamental constant as long as that law remains an empirical one.

8. The *distance*, a, of a planet from the sun, or the semi-major axis of its orbit. By the theoretically proved third law of Kepler, we get from this constant the *periodic time*, T, requiring only the *constant*, μ, *of gravitation* to be known, and this latter is the same for all planets.

9. The *plane* or *inclination*, i, of the orbit.

10. The *direction* of the motion. The *velocity* is given by the distance.

11. The *eccentricity of the orbit.* This constant *is* fundamental, for the theory of gravitation only proves the orbit to be a *conic section* of some kind. The eccentricity can only be found from observation.

12. The *number of satellites* of a planet is also fundamental—and for each of them the same eleven constants have to be taken from observation; the first seven even are required by the sun.

From these twelve empirical data, theoretical astronomy can deduce the motion of the corresponding planet. The whole number of empirical constants is not at all inconsiderable; for

8 principal planets, constants	1–12,	- - -	96
80 small planets, "	8–11,	- - -	320
23 secondary planets, "	1–11,	- - -	253
The sun, "	1–7,	- - -	7
Total number of constants,			676

to which the corresponding constants for the comets would have to be added.

What, in the face of this great number of constants that astronomy has to borrow from observation, shall we say about the boasted perfection of this science? Is it not in science, as in

[5] This Journal, ix, 395, May, 1850; also xiv, 210, Sept., 1852.

morals, that self-adoration hinders progress? Can any astronomer who has not merely studied the details of the celestial mechanics, but also kept in mind the great principle laid down by its author in his "Plan"—can he still pretend that Newton's theory of the solar system merely needs further development, seeing that the few bodies of this system require him to borrow about seven hundred constants from observation?

We shall honor the memory of Newton much more by trying to go beyond the results of his labors than by stupidly worshipping[6] the same, and thus arresting the progress of that science to promote which he spent his life.

§ 2. *These fundamental constants sustain remarkable relations to each other.*

The Newtonian theory of gravitation simply accepts these constants as observation gives them. For if our earth had Jupiter's mass, the rings of Saturn, the moons of Uranus and its axis in the ecliptic, the latter perpendicular to the orbit of Jupiter, a retrograde motion in a hyperbolic orbit—it still would as fully and as beautifully confirm the theory of universal gravitation as it does now; for, let us openly and frankly acknowledge it, these constants are independent of the theory of gravitation because the latter is independent of the former.

But, though this theory does not give any reasons for any kind of dependence between the often mentioned constants, observation shows that they sustain very remarkable relations to each other;[7] or in other words, *there are relations and laws in our solar*

[6] The literature of astronomy teems with implicit instances hereof; but we find also direct expressions of this feeling, like the following:

Enfin, nous avons vu que ces resultats eux mêmes peuvent se composer en un seul et se representer par une loi unique, celle de la Pésanteur universelle; parvenus à principe nous nous voyons en quelque sorte élévé à *la source commune de tous les faits astronomiques; tous en dérivent* de la manière la plus simple et ils y sont en quelque sorte comme concentrés. Nous avons donc pour ainsi dire décomposé le systême du monde, *nous l'avons réduit à son élément unique*, et nous l'avons ensuite recomposé.—Biot, *Traité élém. d'astronomie physique.* Paris, 1805. Concluding remark of the work.

This "élément unique" is rather singularly unique, requiring no less than seven hundred elements to be borrowed from observation alone!

[7] Newton was aware of this—indeed, nobody can help seeing some of these relations. In the scholium to the third book of Principia he says:

"Planetæ sex principalis revolvuntur circum solem in circulis soli concentricis, eâdem motus directione, in eodem plano quamproximé. Lunæ decem revolvuntur circum terram, jovem et saturnam in circulis concentricis, eâdem motus directione, in planis orbium planetarum quamproximæ. *Et hi omnes motus regulares originem non habent ex causis mechanicis.*" Edit. *le Seur* et *Jacquier* Geneva 1749–42.

It is customary to censure Kepler's fancy in contrast to the solidity of all Newton's words; still a sentence like the above is much more objectionable in science than the boldest fancy, for the latter is not accepted without severe scrutiny, while the former is repeated as a sacred truth. If Newton had written "mechanical causes known to me" instead of by "mechanical causes," simply to imply that he knew them all, he would have prevented many a drawback that has encumbered

world of a still higher order than those deduced from gravitation. Thus, the inclinations of the orbits of the principal planets, instead of being uniformly distributed over the first quadrant, are all very small; and their direction, instead of being as often retrograde as direct, are for *all* planets and most satellites *direct.* Instead of having the eccentricities regularly varying from 0 to ∞, we find them for all planets nearly zero! The same may be said of any of the above fundamental constants, and not least of the distance, as it is found *approximating* to Titius-Bode's law, that is, to

$$a_n = 4 + 3 \cdot 2^{n-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But quantities that sustain mutual relations to each other are the particular values of a certain function for definite given values of the variable quantities; hence, if we intend to be true to the spirit expressed by the words of Laplace above quoted, it is a problem legitimately belonging to astronomy *to find these functions of which the fundamental constants are but particular values.*

Let us boldly face this great problem and not desist though astronomers tell us that it is not part of their science.

§ 3. *The fundamental constants satisfy the conditions of stability of the system.*

Since relations exist between the values of the fundamental constants, we may ask for the most general expression of these relations. The principal of these relations are, by Lagrange and Laplace, proved to be such as to fulfill the conditions of stability of the system. These conditions are—

1. *Incommensurability of the times of rotation,* ensured by the distances forming an exponential series (1).
2. The *central mass* vastly preponderating, and the greatest masses revolving where the mutual distances are the most considerable.
3. The *direction of all motions* is the same.
4. *The plane of all orbits is and remains nearly the same,* because

$$\Sigma m \sqrt{a} \cdot tg^2 i = c_1, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is and remains a small quantity (the letters having the same signification as in § 1).

science. Less arrogant, because more true, he is when writing to Burnett (about 1680–81), "but yet I must confess I know no sufficient cause of y^e earth's diurnal motion."

Even Biot, notwithstanding his blind admiration, above cited, cannot help seeing something *more than* gravitation can account for, when noticing the harmony in the rotary and translatory motions. He says: "cet accord, *qui tient sans doute aux premières causes qui ont déterminé les mouvements planétaires, est un des phénomènes les plus remarquables du système du monde.*—Astron. physique, vol. iv, chap. v, p. 467.

5. *The eccentricity, e, of the orbit is and remains nearly the same,* because

$$\Sigma m \sqrt{a} . e^2 = c_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is and remains a small quantity.

6. The *density, d,* is such that the diameter of the bodies is small in comparison to their distances.

We may add—

7. The *form* of the planets is such that the influence of the deviation from a sphere is the smallest possible.

§ 4. *Gravitation is insufficient.*

The laws of Kepler are grand—as well as Newton's theory in accounting for them; but the above laws of Lagrange and Laplace are certainly of a superior order, and the theory of gravitation in failing to give even a shadow of a reason for these laws proves itself to be not the whole truth: we must go beyond this force!

Astronomers seem to forget the *history* of their own science; for how could they otherwise deny the legitimacy of accounting for the fact that the above laws express the stability of the world? Had not astronomers at the time of Kepler the same reason to be satisfied with his laws as astronomers have now in abiding by the laws of stability? And is it not as urgent to discover the causal connection between these laws ensuring great *duration* to our system, as it was to find in gravitation the mechanical cause of those laws ruling the spheres at the time being?

Now the hypothesis of Kant and Laplace will be found to account for the laws of stability as rigidly as the hypothesis of Newton accounts for the laws of Kepler; why, then, deride the former and adore the latter hypothesis? Or do we even forget that "the principle of gravitation" is but a "*hypothesis?*" Did not Newton himself consider it as such? Is not this force fully as mysterious and fully as much beyond the reach of direct observation as the chaos of Kant and Laplace? The former we *assume* as continually acting, because we find that *the motions* are such as this force would produce (*provided* a tangential force, of which gravitation knows nothing, also acts in a definite manner, etc., etc.). Why not also assume the latter as having been real, if we find by the same mechanical deductions that the existing harmony, as expressed in the stability of the system and ensured in the mutual relations of the fundamental constants follows directly from the above-named chaos? If in the one case we reason from fact or law to cause—why not also in the other, provided our conclusion is as legitimate?

Here astronomers will not fail to object that this last condition

is *not* satisfied. We fully admit this; but beg them to remember that it has taken two centuries of labor to ensure this legitimacy to gravitation—that Newton did not leave gravitation as a mere suggestion (as such it had existence before him), but in his immortal *Principia* gave the necessary mechanical firmness to this hypothesis: how different has been the lot of Kant and Laplace's hypothesis! The first of these expounded it rather fancifully in connection with speculations on the inhabitants of distant globes;[8] the latter only gave a few bold and deep outlines of the nucleus of the theory! What would to-day be the estimation of gravitation if we, instead of Newton's *Principia*, only had a few of Hooke's sublime guesses, if these had only been considered by men like Fontenelle[9] instead of being investigated by Euler, the Bernoullis, Lagrange, Laplace, Gauss, Hansen, Plana, etc.?

Can anything be more unjust than exaltation of the hypothesis of Newton—this deservedly cherished subject of the master minds of two centuries—above the hypothesis of Kant–Laplace, which, being too early left even by its astronomical parent, has been ever since considered an outcast in the world, endangering the reputation of any one who would dare to touch it?

We will adopt this almost forlorn hypothesis as a *mere hypothesis*—we will patiently and carefully trace its bearings by means of as rigid an analysis as we can command in this most intricate field; we will minutely compare the results thus obtained with the actually observed state of things; and if we find *the correspondence between idea and phenomenon*, between analysis and observation, to be very close, we hope that those who have analysis more at their command than we, will pay as much attention to this high branch of astronomy as has been, and deservedly continues to be, bestowed on Newton's hypothesis of gravitation. If our feeble endeavors only succeed in making Kant–Laplace's *hypothesis* admitted as such among analysts, we shall have accomplished all we desire: for then this hypothesis will soon be considered as firmly established a principle as gravitation,[10] or as the fact of the rotation of the earth,[11] which latter—notwithstanding Foucault's pendulum and gyroscopes—still remains unproved by *ocular evidence*, all "demonstrations" being in fact

[8] Allgemeine Naturgeschichte und Theorie des Himmels, 1755.

[9] Théorie des Tourbillons Cartésiens avec des réflections sur l'attraction (1752). Also his Entretiens sur la pluralité des mondes.

[10] Gravitation was always treated as a mere hypothesis by the Cartesians; the work of Fontenelle above cited offers the instance most generally known.

[11] The scientific prejudice existing against the nebular theory is perhaps as injurious as the religious prejudice once "*resisting*" the motion of the earth. See the amusing statement in the preface to the 'Dei massimi sistemi,' (ed. Padova, 1744,) that the "'Moto della Terra' non può né dee amettersi se non come pura ipotesi matematica, che serve a spiegare più agevolmente certi fenomeni." Also Galileo himself, in his Dialogue (*Opere compl.* Firenze. Vol. i, 1848, pp. 387 and 447).

inductive, reasonings with our senses. Thus, in the latter, we *see* the plane of the oscillations rotate, but *conclude* it to be the earth. It requires at least as much mental effort to apprehend its true bearing as the simple reference to the diurnal motion of sun, moon, and stars.

§ 5. *How far the nebular theory accounts for the stability of the system.*

The theory of gravitation can never, therefore, account for the stability of the solar system. How far, then, does the nebular theory explain those great fundamental conditions of the system that ensure not only the harmony of the solar world but even make this harmony (almost) permanent?

In order to invite physicists and astronomers to the perusal of the following introduction, we will try to give a simple answer to this most important question.

I. *The plane of all planetary orbits must be nearly the same* (see § 1, 9, and § 3, 4, also § 10).

This theorem has been clearly seen by Kant and Laplace; it is the most immediate expression of the hypothesis. Mr. Trowbridge has recently pointed out[12] to us a very interesting consequence hereof, viz: the most distant planet *must move in the invariable plane;* and, indeed, the inclination of the orbit of Neptune is 1° 47′, that of the invariable plane 1° 41′.

II. *The direction of all planetary revolutions must be the same;* this obvious consequence of the hypothesis accounts for § 3, 3, and § 1, 10.

III. *The eccentricity of the planetary orbits must be very small*— accounting for observation, § 1, 11, and condition of stability, § 3, 5 and § 10.

This proposition has been deduced in general reasonings by Kant and Laplace; in the following we will try to give a demonstration of it.

IV. *The planetary distances are such, that the successive planets were evolved at equal intervals of time;* or if $t=1, 2, 3, \ldots$ respectively for Mercury, Venus, Earth ..., then the distance is

$$a_t = \alpha + \beta . \gamma^t, \quad \ldots \quad (4)$$

where α, β, γ are constants, and t *the age of the planets* above that of Mercury. From this follows the condition of stability that *the periodic times are incommensurable* (§ 3, 1). Besides, it is seen that this law accounts for the empirical law of Bode (§ 1, 8).

The analytical demonstration of this law is one of the principal objects attempted in the present introduction. (See § 13.)

V. *The mass of the more distant planets is the greater*, on account of the greater space from which the material of the planet was condensed (space increasing according to IV). This is confirmed

[12] On the Nebular Hypothesis, § 24; this Journal, November, 1864, page 355.

by the fact that the sum of the *four great exterior planets* is 480 times the mass of the earth, while the sum of the masses of the *four interior planets* is but twice the mass of the earth. Furthermore, *the mass must increase toward the sun*—as the density from which the rings were formed was greater nearer the center. This is confirmed, as the mass of Neptune is 25·6, the mass of Uranus but 14·5, so that the mean of the two most distant is 20; the mass of Saturn is *five* times as great (101·6), and the mass of Jupiter, again, three times greater (339·2). The minimum in the case of the mass of Uranus is evidently produced by the simultaneous influence of both the above principles.

The mass of Jupiter is a maximum; it is so great that *the next following ring was broken up into fragments by the perturbing influence of so stupendous a mass*—thus originating the host of *asteroids*, and perhaps also the *meteorites*.

The very small mass of the interior planets as compared with the exterior ones is not astonishing, if we remember that the inter-planetary space between Jupiter and Saturn is to that between Venus and the Earth, as 10^3–$5{\cdot}2^3$ to $1{\cdot}00^3$–$\cdot72^3$, or as 860 to ·63, or nearly as 1300 to 1. The mass of Jupiter is to the mass of our earth as 340 : 1, thus giving us still some margin for an increase in density toward the center of the nebula.

Being as yet unable to give the precise *theoretical law of the masses*, we are obliged to make the above few suggestions, in order to show that the nebular hypothesis at least gives a general law of distribution of the planetary masses in conformity with § 1, 1, and § 3, 2. (Prof. Kirkwood takes a similar view of the asteroids; see this Journal, 1852, xiv, 214.)

VI. *The figure of the planets* (being a *condensed* vapor) *must be an oblate spheroid* of

VII. *Small ellipticity*, because

VIII. *The velocity of rotation is but small* (compare § 1, 2, 3, 7, and § 3, 7).

This last proposition is based upon the fact that the moment of rotation is but the *difference* between the moment of revolution of the exterior and interior part of the planetary ring.

Still, *the exact amount of this velocity*, as well as the period of rotation of the sun, has not yet been deduced from the nebular hypothesis; we have often attempted it, but as yet have not been able to solve this difficult problem.

Prof. Kirkwood has found[13] the empirical law of the velocity of rotation, *a law analogous to the third law of Kepler*. We have repeatedly arrived at expressions similar to (but not identical with) Kirkwood's law.

IX. *The Plane;* and

[13] As above, this Journal, 1850, ix, 395.

X. *The direction of rotation* of the planets has been considered at variance with the nebular theory, ever since the discovery of the lunar system of Uranus. We believe that our analysis of this problem[14] shows that the rotation of Uranus and Neptune, both as to position of the axis and direction of motion affords a very interesting confirmation of the theory. See § 1, 5, 6.

XI. *The density* of the planets has also been considered as being adverse to the theory; but if, as necessary, the influence of the age is taken into account, it is found that the minimum density exhibited by Saturn is demanded by the theory.[15] Compare § 1, 4, and § 3, 6.

XII. *The number of satellites* was already shown, by Kant, to increase with the distance from the center of the nebula. Though not usually given as such, it nevertheless is a condition of stability of the system—at any rate it is conformable to observation (§ 1, 12).

The rings of Saturn are best considered as a host of satelloids, corresponding to the planetoids (and meteorites) of the solar world—thereby accounting for the excessive thinness and the subdivisions of the rings.[16]

In looking back upon the preceding account of the present aspect of the nebular theory, it will be seen

A. That the four great fundamental conditions of stability referring to *the system at large* are now satisfactorily deduced from the hypothesis of Kant–Laplace (I–IV above).

B. That the problem of the *mass* (V) and the *number of satellites* (XII), though not completely evolved, still is sufficiently comprehended to enable us to say that the analytical solution is *possible;* and

C. That the elements referring to the *single planets*, or rather their subordinate systems, are, with the exception of the *exact* law of rotation (VIII), fully deducible from the fundamental hypothesis of Kant and Laplace.

We see, then, that the *fundamental constants* of the solar system, which number about seven hundred (§ 1), exhibit very remarkable mutual dependencies (§ 2), which are such as ensure the permanence or stability of the system (§ 3), which Newton's law of gravitation cannot account for (§ 4). Though they offer a higher problem for theory than Kepler's laws, astronomers have hitherto been unwilling to recognize the analysis of the above conditions of stability as part of their science. Laplace, while instrumental in bringing to light the great laws of the stability of the system, independently reproduced the bold hy-

[14] On the density, rotation, and relative age of the planets; this Journal, 1864, xxxvii, 36, 48.

[15] As above, p. 49.

[16] On the density, etc.; this Journal, 1864, xxxvii, 54.

pothesis of Kant, and though this has been most grievously neglected by analysts and astronomers, still it now affords us a full solution of the four great harmonies ensuring the permanency of the solar world, and also solves most, and at least indicates the solution of, all other problems relating to the harmony of the fundamental constants of the solar system.

May we not hope that astronomers will begin to bestow on this theory some share of their labor?

§ 6. *The Hypothesis.*

We assume, with Kant and Laplace, as the primitive condition of the solar system, or as *nebula:*

The space of the solar system was filled with matter having a moment of rotation.

This *matter* is endowed with the same forces we know it to possess; a simple calculation will furthermore show that it was a highly rare vapor. Its chemical constitution we will leave out of consideration for the present; we therefore consider it as composed of the elements we know here on earth, many of which we *now* know to be found on the sun, and are probably also on the distant stars;[17] still there can be no doubt but that many more elements exist than we are acquainted with. Many of the spectral lines even of our own central star are irreducible to spectra of known elements. We therefore mean simply to say that at the above primitive period the *elements had been created.* I hope at some future time to publish an attempt at a *mechanical theory of the elementary bodies*, which has occupied my time for about ten years, and wherein I endeavor to show the physical properties of the known elements to be *definite functions* of their *atomic number and form.* Accordingly, there would yet remain a more primitive condition, the existence of the *one primitive matter* (Urstoff) which would be considered as the direct creation of the *one* God.

The *rotation* of the nebula is *not* to be thought regular, but simply amounting to a certain momentum. I have elsewhere[18] tried to show that such rotation may be considered as the *effect of a difference of any kind between the primitive forces of attraction and repulsion* wherewith we know matter to have been endowed.

If, therefore, these views should be well founded, we should have arrived at the grandest principle we can conceive of in the present state of our knowledge; we should be able to see how from *created matter alone the whole of the solar system has been developed;* we would be enabled to conceive the almighty *fiat* as

[17] Rutherford, Astronomical Observations with the Spectroscope; this Journal, 1863, xxxv, 71. Above all, Bunsen's and Kirchhoff's memoirs on their great discovery.

[18] This Journal, 1864, xxxvii, 52.

one single act. How much such a theory would tend to elevate our conceptions of the great Author, we cannot here develop.

In the present paper I shall not go farther back in time than to the existence of the nebula of Kant and Laplace as above defined.[19]

§ 7. *Plateau's experiment.*

Before entering upon the analysis of the nebula, we must refer to the experimental evidence of the nebular theory afforded by the beautiful experiments of Plateau, detailed in his *Mémoire sur les phénomènes que présente une masse liquide libre et soustraite à l'action de la pésanteur*, Pt. I (Neuv. mém. de l'Acad. de Bruxelles, vol. xvi, 1843). His results are:

1. A liquid, subject only to the action of its molecular forces assumes the form of a perfect sphere (§ 2).

2. This globe is flattened at its poles, if subject to rotation (§ 10). Although, as he thinks, the molecular forces are not *identical* with those acting in the nebula, still the results ought to be analogous, if they are not identical.

[19] It is perhaps not out of place here to give a synopsis of the different distinct *ages* that are characterized by a further individualization or a new direct creation according to the views above indicated.

Three (or four) direct acts of the Deity may be recognized, viz: the creation of *matter*, of *life*, of *mind* (and the *redemption*). The formation of the *elements* out of matter characterizes the *first age;* the formation of the *solar world*, with its planets, moons and central sun, the *second age;* while the *third age* beheld the development of our *earth* from a vaporous ball to its present shape; in the *fourth age*, *life* was created in the form of *plants* and *animals;* in the *fifth age*, mind, the investigating mind, was introduced by the creation of *man*, the *cephalized animal;* and in the *sixth*, redemption took place; while a *seventh age* will behold the destruction of the whole system, occasioned by the *extinction* of the solar body and the *resistance* of ether. (See this Journal, 1864, xxxvii, 56.)

To *every age* correspond two sciences: the first relates to the development—Whewell would call it the *Paletiology* of the age—while the second relates to the actually *existing product* of the development or creation of that age, i. e., *the* science. Thus we obtain the following general view of the natural sciences:

Age	1	2	3	4	5	(6)	(7)
	I. *Creation of* Matter. Development of			II. Creation of	III. Creation of	*Redemption.*	Destruction of the world.
	Elements.	*Solar system.*	*Earth.*	*Life.*	*Mind.*		
Paletiology	Atomology	Planetology.	Geology	Paleontology.	Archeology.	Old Testament	
Science.	Physics, Chemistry.	Astronomy.	Geography.	Botany, Zoology	History.	New Testament	

These names have of course to be taken in their widest sense; thus geography stands for physical geography, meteorology, etc., and history comprehends not only political but also the intellectual history of the human race, thus including again *all* the sciences in their historical development. We see how "planetology" is allied to geology and astronomy.

3. If the rotation becomes sufficiently rapid, the *whole globe* is transformed into a *ring* in the equatorial plane (§§ 11–14) which, by continuing the rapid rotation of the large central disc, even loses its regularity, and separates into small masses which immediately assume globular forms. "But," continues he (§ 19), "this is not all; one or more of these spheres are always seen to take, at the instant of formation, a motion of rotation on their own axes, and this motion is almost always in the same direction as that of the ring."[20] He even found that still smaller globes were formed.

4. If a small disc is put into very rapid rotation, a ring is formed, while a part of the original globe remains on the axis (§ 21), so that Plateau rightly concludes with saying (§ 27) that most of the phenomena of the relative configuration of the heavenly bodies have been reproduced by him on a small scale.[21]

[*To be continued.*]

ART. VIII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.—No. 2.

§ 1.

On the separation of chromium from aluminum, iron, manganese, cobalt, nickel, zinc and magnesium.—Sesquioxyd of chromium in an alkaline solution is readily oxydized to chromic acid by means of chlorine, bromine or deutoxyd of lead. When chlorine or bromine are employed as oxydizing agents, the alkaline solution may be neutralized by acetic acid, after the oxydation is complete, and the chromic acid may then be precipitated by acetate of barium, when the solution is free from sulphuric acid, and directly weighed in the form of chromate of barium. In place of free alkali it will be found in practice very much more convenient to employ acetate of sodium or potassium. When a solution of sesquioxyd of chromium is rendered nearly neutral by a solution of carbonate of sodium, and acetate of sodium is added in excess, a current of chlorine gas, or a solution of chlorine water, readily converts the whole of the chromium present into chromic acid, especially when the solution is hot, and when it is kept nearly neutral by occasional addition of carbonate of sodium. The excess of chlorine is easily expelled by boiling,

[20] Of course; for in this oil the density is the same. See our article, this Journal, 1864, xxxvii, 51, $\delta=0$.

[21] Malgré la difference des lois que suivent les forces attractives dans ce cas et celui des grandes masses planétaires, nous avons vu se produire, en petit, une représentation frappante de la pluspart des phénomènes de configuration relatifs aux corps célestes.

after which, in the presence of bases not precipitated by ammonia, the chromic acid may be precipitated by acetate of lead, or acetate of barium, and weighed in the form of chromate, provided, of course, that no sulphuric acid is present. When sulphate and chromate of barium are thrown down together, the chromic acid may be reduced to sesquioxyd by boiling with concentrated chlorhydric acid and alcohol, after which the barium may be precipitated by sulphuric acid, and the sesquioxyd of chromium thrown down in the filtrate by boiling with ammonia in the usual manner. As the reduction of chromate of barium by means of chlorhydric acid and alcohol does not take place very readily, it is better to boil the chromate with an excess of carbonate of potassium or sodium, to filter off the carbonate of barium, and determine the chromic acid by means of nitrate of suboxyd of mercury, or by reduction to oxyd of chromium and precipitation with ammonia in the usual manner.

When aluminum and iron are to be separated from chromium by this process, the two oxyds may be precipitated together by simply boiling, the solution after the complete oxydation of the chromium to chromic acid, in the presence of excess of acetate of sodium. It is more convenient and equally accurate to neutralize the solution with ammonia, separate the alumina and sesquioxyd of iron by filtration, and determine the chromium in the filtrate by reduction and precipitation with ammonia.

When the oxyds of calcium, magnesium, zinc, nickel, cobalt and manganese are present in a solution containing sesquioxyd of chromium, it is best to oxydize the chromium to chromic acid as above, and then to precipitate with acetate of lead or barium.

I have stated in a former paper that chromic iron ore may be completely resolved by fusion with fluohydrate of fluorid of potassium. In this and in all similar applications of the fluohydrate, it is best to evaporate the finely pulverized mineral to dryness with a concentrated solution of the salt. On subsequently heating to low redness, the resolution of the mineral is effected with the utmost ease, a portion of the chromium being usually oxydized to chromic acid by the oxygen of the air. After expelling the fluorine by heating the fused mass with sulphuric acid, the remaining mass may be treated with acetate of sodium and chlorine in the manner already pointed out, the iron and aluminum separated by boiling, and the chromic and sulphuric acids precipitated by acetate of barium, after which the chromium may be determined as above.

In precipitating chromic acid by means of nitrate of suboxyd of mercury, hot solutions must not be employed, as a small portion of chromic acid is always reduced to sesquioxyd of chromium. The precipitated chromate should be allowed to

stand some hours before filtering. In general, the precipitation by acetate of lead or acetate of barium is to be preferred even when the resulting chromate is to be weighed as such.

§ 2.

On the employment of acetate of sodium for the separation of iron and aluminum from other bases.—The facility with which iron and aluminum are precipitated from neutral solutions of the sesquioxyd, by boiling with acetate of potassium or sodium, has led to frequent analytical applications, though the method is not so generally employed as it deserves. Mr. C. F. Atkinson has devoted much time to a careful study of the subject, and has arrived at the following results, which appear to me worthy of attention. The sesquioxyds of iron and aluminum may be perfectly separated from the protoxyds of manganese, cobalt, nickel, zinc, magnesium and calcium, and from sesquioxyd of uranium, by boiling the neutral or nearly neutral solutions with acetate of sodium, provided that the following precautions are observed. The solutions from which the sesquioxyds are to be precipitated must be dilute: half a liter of the solution should not contain more than one gram of either sesquioxyd or of the two when both are present. The quantity of acetate of sodium should be sufficient to convert by double decomposition all the bases present into neutral acetates. The acetate should be added to the metallic solution when cold and the whole should then be heated together and boiled for a short time. It is not necessary to filter upon a water-bath funnel, but the beaker containing the solution should be kept nearly at the boiling point during filtration, and a ribbed filter should be employed. In all cases it is best to add a few drops of free acetic acid to the solution, to prevent the formation of basic acetates of the protoxyds. This is especially necessary in separating iron and aluminum from zinc and nickel.

Finally, it is best, whenever it is possible, to have all the bases present in the form of chlorids. The iron and alumina upon the filter in the form of basic acetates must, whenever an absolutely complete separation is necessary, be redissolved in chlorhydric acid and again precipitated by boiling with the acetate after rendering the solutions nearly neutral by means of carbonate of sodium. In this manner only is it possible to separate the last traces of the stronger bases. Finally, the basic salts of the iron and aluminum, after washing, must be redissolved in chlorhydric acid, and precipitated by boiling with ammonia in the usual manner, to free them completely from alkali. The precaution of a second treatment with acetate of sodium is more necessary with alumina than with sesquioxyd of iron alone. It is scarcely worth the trouble in the separation of iron from calcium and magnesium.

According to my own observations, the sesquioxyds of iron and aluminum cannot be separated from sesquioxyd of chromium by boiling with acetate of sodium, although the last mentioned oxyd is not precipitated when alone in solution. In this case it is necessary to oxydize the chromium to chromic acid by chlorine in the manner already pointed out.

§ 3.

On the separation of manganese from cobalt, nickel and zinc.— Schiel's method of separating manganese from the alkaline earths by adding acetate of sodium to the mixed solutions, heating the liquid gently and then passing chlorine through it so as to convert the manganese into a hydrate of the sesquioxyd, is better than that formerly given by myself in which peroxyd of lead is used as the oxydizing agent. With respect to Schiel's method, however, it must be remarked that it cannot be employed to separate manganese from nickel or cobalt, because both of these metals are converted into higher oxyds under the same circumstances. Nickel may, as Popp has recently shown, be completely precipitated as a deep blue hyperoxyd, while as I have myself observed, cobalt is also oxydized, though not precipitated, unless the solution is boiled with an alkaline carbonate. In separating manganese from zinc, calcium or magnesium, I have repeatedly found that a second treatment is necessary in order to obtain a perfect separation. This second treatment may be neglected in separating manganese from calcium and magnesium, but not in separating it from zinc, although the addition of a few drops of free acetic acid renders the process more exact.

Though the method of separating manganese from other bases by means of peroxyd of lead, which I formerly proposed, will hardly be used in future, now that we are in possession of more convenient processes, it will still be of some interest to chemists to know the precise nature of the insoluble black compound which is formed when peroxyd of lead, PbO_2, is digested or boiled with an excess of a solution of chlorid or nitrate of manganese and afterward thoroughly washed. An analysis of this body, made some years since in my laboratory by my lamented friend and former pupil, Mr. Theodore Parkman, gave the following results:

		Anhydrous.	Theory.
Manganese, - -	35·10	37·96	37·53
Lead, - - -	32·49	35·13	35·26
Oxygen, - - -	24·87	26·90	27·20
Water, - - -	7·52	....	....
	100·00	100·00	100·00

Neglecting the water, which may have been, in part at least, mechanically combined, and which amounts to between three

and four equivalents, we have the formula, MnO_2+4PbO_2, as the simplest expression of the results of the analysis.

A simple and perfectly satisfactory process for separating manganese from cobalt, nickel, and zinc, is the following: To the neutral or nearly neutral solution of the chlorids, acetate of sodium is to be added in excess together with a few drops of free acetic acid. The solution is then to be boiled, and a rapid current of sulphydric acid gas passed through it while boiling and continued for half an hour. Every trace of cobalt, nickel and zinc, is precipitated in the form of sulphid, while the whole of the manganese remains in solution. The precipitate is to be thrown upon a ribbed filter and quickly washed with cold water saturated with sulphydric acid gas. It is easily washed, and though the sulphids of cobalt and nickel precipitated in this manner are far more easily oxydized than when precipitated by boiling sulphid of sodium from boiling solutions, they will be found to present no difficulty as regards oxydation upon the filter. Manganese may then be determined in the filtrate by boiling with chlorhydric acid and precipitating in the usual manner with carbonate of sodium. The mixed sulphids upon the filter, supposing for the sake of generality that all three are present, are to be dissolved in chlorhydric acid and the metals converted into double cyanids by means of an excess of cyanid of potassium, after which the zinc may be precipitated by means of sulphid of sodium, as recommended by Wöhler.

When perfectly pure cyanid of potassium is not at hand, the following process will be found particularly convenient. Acetate of sodium is to be added to the solution of the mixed chlorids, after which the vapor of cyanhydric acid generated in a flask from sulphuric acid and ferrocyanid of potassium is to be passed directly into the solution. Cyanid of zinc is immediately precipitated more or less completely as a perfectly white powder. A solution of sulphid of sodium is then to be added as long as a precipitate is formed, after which the sulphid of zinc is to be separated by filtration. Cobalt and nickel remain in solution as double cyanids. The same process may be used to separate manganese from cobalt and nickel, sulphid of sodium throwing down under these circumstances a pure flesh red precipitate. It is easy to see that zinc and manganese together may be separated from cobalt and nickel by the same process and at one operation. No cyanid of manganese appears to be formed when cyanhydric acid is passed into a solution containing a salt of manganese, acetic acid, and acetate of sodium.

I have stated in a former paper[1] that the sulphids of nickel and cobalt are thrown down from boiling solutions by a boiling solution of sulphid of sodium in an insoluble form, so that in

[1] This Journal, March, 1864.

fact even strong chlorhydric acid scarcely exerts upon them an appreciable action. This process has been applied to the separation of cobalt and nickel from zinc and manganese by my excellent assistant, Mr. Maurice Perkins, and gives results which are very satisfactory, especially for qualitative purposes, the sulphids of manganese and zinc precipitated under the same circumstances being readily soluble, even in dilute acid. The process is now substituted in this laboratory for that given in most of the recent works on qualitative analysis, and has been repeatedly tested with satisfactory results.

§ 4.

On the separation of cobalt from nickel.—A method of separating these metals, given some years since by Liebig,[4] consists in boiling the mixed double cyanids of nickel and potassium and cobalt and potassium with oxyd of mercury. Oxyd of nickel is precipitated, while an equivalent quantity of mercury is dissolved as cyanid. The method certainly gives good results but is not free from objection. Long boiling is necessary before the precipitation is complete, and it is difficult to prevent "bumping" during ebullition. The excess of oxyd of mercury must be separated from the oxyd of nickel by a special operation, and the nickel afterwards again precipitated by caustic alkali.

These inconveniences may be completely avoided by employing, instead of the oxyd alone, a solution of the oxyd in the cyanid of mercury. When this solution is added to a hot solution of the double cyanid of nickel and potassium, the whole of the nickel is immediately thrown down as a pale green hydrate of the protoxyd. Under the same circumstances cobalt is not precipitated from the double cyanid of cobalt and potassium. Mr. W. N. Hill, who has repeatedly employed this method and carefully tested it, has found that the separation effected is complete. No cobalt can be detected in the precipitated oxyd of nickel by the blowpipe, nor can nickel be detected in the cobalt (finally separated as oxyd) by Plattner's process with the gold bead. The solution of oxyd of mercury is easily obtained by boiling the oxyd with a strong solution of the cyanid, and filtering. According to Kühn[5] the cyanid formed in this manner has the formula $HgCy+3HgO$. The hydrated oxyd of nickel precipitated may be filtered off, washed, dried, ignited and weighed. The cobalt is more readily and conveniently determined by difference, when, as is always possible, the two metals have been weighed together as sulphates. I am not prepared to say that this modification of Liebig's method of separating nickel and cobalt gives better results than Stromeyer's process by

[4] Ann. der Chemie und Pharmacie, lxv, 244.
[5] Berzelius, Lehrbuch der Chemie, iii, 872.

means of nitrite of potassium, but it is at least very much more convenient and requires much less time. The complete precipitation of cobalt in the form of $Co_2O_3,2NO_3+3KONO_3$ usually requires at least forty-eight hours, and rarely succeeds perfectly except in experienced hands.

§ 5.

On the separation of uranium from zinc, cobalt and nickel.—The method which I have given above for the separation of manganese from cobalt, zinc, and nickel, by precipitating the sulphids of the three last named metals, by means of sulphydric acid gas, from a boiling solution of the acetates, may be also used, according to the carefully conducted experiments of Mr. Perkins, for the separation of uranium from the same metals. The process is in all respects the same, and requires, therefore, no further description. It will be found much simpler and more convenient than that described by Rose, by means of carbonate of barium.

§ 6.

On the electrolytic precipitation of copper and nickel as a method of analysis.—The precipitation of copper by zinc, in a platinum vessel, with the precautions recommended by Fresenius, leaves nothing to be desired, so far as accuracy, ease and rapidity of execution are concerned. The method labors, however, under a single disadvantage: the introduction of zinc renders it difficult, or at least inconvenient, to determine with accuracy other elements which may be present with the copper. It has occurred to me that this difficulty might be overcome, the principle of the method being still retained, by precipitating the copper by electrolysis with a separate rheomotor. The following numerical results, which are due to Mr. E. V. M'Candless, will satisfactorily show the advantages of the method for the particular cases in which it is desirable to employ it. The copper was, in each case, in the form of sulphate; the deposition took place in a small platinum capsule, which was made to form the negative electrode of a Bunsen's battery of one or two cells, in rather feeble action. The positive electrode consisted of a stout platinum wire, plunged into the surface of the solution of copper at its center. The following table gives the results obtained in the analysis of pure sulphate of copper:

Number.	Salt taken.	Copper found.	Percentage.
I.	1·2375	0·3145	25·41
II.	0·4235	0·1075	25·38
III.	1·0640	0·2705	25·42
IV.	1·3580	0·3440	25·33
V.	0·5665	0·1450	25·59
VI.	0·4735	0·1205	25·48

In seven determinations of copper in the alloy of copper and nickel employed by the government for small coins, the following results were obtained:

Number.	Weight of alloy.	Copper.	Percentage.
I.	0·4160	0·3640	87·50
II.	0·6180	0·5410	87·54
III.	0·4600	0·4090	88·91
IV.	0·5120	0·4481	87·51
V.	0·4220	0·3693	87·51
VI.	0·2525	0 2225	88·11
VII.	0·3705	0·3255	87·85

The percentage of copper required by the formula $CuO, SO_3 + 5HO$ is 25·42, while the government standard alloy of nickel and copper contains 87·50 per cent of copper. The time required for precipitation varied from one to three hours, the separation of the last traces of copper being in each case determined by testing a drop of the liquid upon a porcelain plate with sulphuretted hydrogen water. The copper, after precipitation, was washed with distilled water, dried in vacuo over sulphuric acid, and weighed with the platinum vessel. The only precaution necessary is to regulate the strength of the current so that the copper may be precipitated as a compact and bright metallic coating, and to dry as quickly as possible. When the copper is thrown down in a spongy condition, it not only oxydizes rapidly, but it is impossible to wash out the last traces of foreign matter contained in the solution. This is well shown by number III and number VI of the second series, in both of which cases the copper was precipitated too rapidly. The solution from which the copper has been deposited contains the other elements present in the original substance. It may be easily poured off without loss, and the washings added.

It appears at least probable that nickel may be determined by electrolysis in the same manner as copper, the solution employed being the ammoniacal sulphate with excess of free ammonia. Mr. M'Candless obtained in two determinations in a commercial sample 91·36 and 91·60 per cent nickel. In both cases the nickel was thrown down completely as a bright coherent metallic coating upon the platinum.

Cambridge, Oct. 1st, 1864.

ART. IX.—*Note on the Planetary Distances;* by Prof. DANIEL KIRKWOOD, Indiana State University.

To obviate any misappreheusion that may have existed in regard to the order of planetary distances proposed in this Journal for July, 1864, p. 12, it may be proper to state that by the term "radii of gyration of the primitive rings," was meant the radii of gyration of homogeneous circular rings, or, in other words, the radii of the circumferences which bisect the circular intervals between the orbits of Neptune and Uranus, Saturn and Jupiter, &c. The formula was given as empirical, in the hope that it might *suggest* a true law of nature, if not found itself in strict harmony with observation.

In the article referred to, the writer suggested the hypothesis of the contemporaneous formation of the different members of our planetary system. Some additional considerations bearing on this subject may not be destitute of interest.

The radius of gyration of the sun, regarded as a homogeneous sphere, is 0·002936; the unit being the mean distance of the sun from the earth. If we designate by K the principle radius of gyration when the rotation period was the same as Neptune's orbital revolution, we shall have

$$25^{d}\cdot325 : 60126^{d}\cdot7 :: (0\cdot002936)^{2} : K^{2};\ \text{whence } K = 0\cdot143.$$

But the radius of gyration of a *homogeneous* sphere filling Neptune's orbit is 18·974. Hence we infer that before the formation of the planets the condensation of the solar spheroid had advanced much more rapidly about the center than toward the surface.[1] The consequent tendency to unequal velocities of rotation would produce a divellent force, thus separating the nucleus from the outer parts, and also breaking up these exterior portions into distinct zones or annuli. As subsequent condensation would, to some extent, increase the rotary velocity of the nucleus, the primitive circle of division between the latter and the surrounding nebulous mass must have been somewhat exterior to the present circle of equilibrium between the centripetal and centrifugal forces. The points in the revolving nebulous matter at which the planetary formatious would originate, were determined by certain conditions which in the present state of our knowledge we are unable to define. It is not, however, an improbable assumption that the intervals between the orbits of such nuclei would be arranged in regular order. This, in fact, is found to be the case, at least approximately, in regard to Ju-

[1] This fact, together with its cause, as the writer has recently learned, was stated by Prof. Stephen Alexander, at the Montreal Meeting of the Am. Assoc. for the Adv. of Sci.

piter's satellites. The differences between the radii of the orbits, commencing with the greatest, are,

11·64811, 5·72677, and 3·57494.

But while the first is almost exactly double the second, the second is less than double the third. In other words, as in the case of Mercury, the theoretical distance of the innermost satellite is somewhat greater than the actual distance. Now, if the *original* intervals constituted an exact geometrical progression, can a probable reason be given for the *present* deviation? We reply (1) that in the case of the primary system considerable masses of meteoric matter are believed to be still revolving within the earth's orbit; portions of which are exterior to Mercury, and some even beyond the orbit of Venus; (2) that if the Nebular Hypothesis be true, the primitive constitution of the secondary systems was probably similar; (3) that in any system an increase of mass in the central body would diminish the orbits of the satellites; and (4) that the contraction of meteoric rings until portions originally *exterior* to a given orbit had become *interior*, would obviously have the same effect. In the manner thus indicated, the distance of the innermost satellite may have been diminished, and thus the original interval between the first and second orbits increased. The same explanation applies to Mercury.[2]

With an interval = 11·64811 between the third and fourth satellites, and a ratio = $\frac{1}{2}$, we find 3·70213 as the limit at which the central body separated from the exterior mass. This is a little beyond the present circle of equilibrium between the centrifugal and the centripetal forces, the radius of this circle being 2·299.

THE SATURNIAN SYSTEM.

In the primary system[3] and also in that of Jupiter, we have found that the original circle of separation between the nucleus and the surrounding mass was a little exterior to the present circle of equilibrium between the two central forces. This is what ought to be expected, as the contraction of the nucleus would accelerate the rotation. For Saturn, however, we found the primitive circle of equilibrium *less* than the present;[4] but in this calculation no allowance was made for the contraction of

[2] This explanation, it must be confessed, is apparently inconsistent with the conclusions of Leverrier in regard to Mercury's motion. The distinguished director of the Paris Observatory "has found that Mercury's mean motion has gone on diminishing; as if the planet were, in the progress of his revolutions, receding further from the sun. This is explained, if we suppose that there is, in the region of Mercury, a resisting medium which moves round the Sun in the same direction as the Planets move."—*Whewell's Hist. of the Ind. Sci.*, 3rd ed., vol. i, p. 560.—Perhaps no satisfactory determination of the facts can be reached without long continued observations.

[3] This Journal for July 1864, p. 13.

[4] Ib., p. 15.

the orbits of the innermost satellites. If the radius of gyration for Mimas and Enceladus was diminished in a ratio equal to the mean of those obtained for Mercury and the innermost satellite of Jupiter, it was originally 3·8720. Adopting this value and interpolating the two missing terms, we have the following elements conforming to our hypothesis:

SATELLITES OF SATURN.

Names.	Distances.	Rad. of Gyr. of Intervals.	Intervals.
I. Japetus,	64·359	54·9764	32·0070
I. ———	43 6203		
II. Hyperion,	25·029	22·9694	12·4154
II. Titan,	20·706		
III. ———	11·958	10·5540	4·8150
III. Rhea,	8·932		
IV. Dione,	6·399	5·7390	1·8670
IV. Tethys,	4·9926		
V. Enceladus,	4·0319	3·8720	&c.
V. Mimas,	3·1408	&c.	0·0000
	&c.	Limit=2·6862	

The ratio of the ascending series of intervals is 2·578+. These ratios in the systems of Jupiter and Saturn are to each other inversely as the orbital velocities of the two planets. The distance from the center of Saturn at which a satellite would complete its orbital revolution in the present period of the planet's rotation is 2·0075. The distance at which the nucleus originally separated from the surrounding mass has been found (theoretically) somewhat greater, as it ought to be, viz., 2·6862.

If, then, the arrangements of the Saturnian System should not be admitted as confirmatory of the empirical order of distances, we may at least conclude that it is not incompatible with it. The *exact* coincidences are of course produced by the interpolation of the two terms. With these, our formula gives three equations with three unknown quantities, and, consequently, whatever the distances of the known satellites, the roots of these equations, whether real or imaginary, must, in an algebraic sense, satisfy the conditions. At the same time it is easy to perceive that these algebraic results *might be* decidedly unfavorable to the proposed hypothesis.

The tendency in a rotating nebula to unequal angular velocities, resulting from the increased rapidity of condensation from the equator toward the center, may, perhaps, also account for the phenomena of spiral nebulæ. If, in a contracting mass of vapor, a free motion of the particles among themselves be established before the centrifugal force becomes equal to the centripetal, a spiral convergence, like that of No. 51 in Messier's catalogue, would naturally ensue.

Finally, if the original constitution of the Solar and Saturnian Systems was such as we have supposed, can any probable reason be given why the satellites of Jupiter should be found an exception?* It may be worthy of remark that if these bodies, or the rings from which they were derived, were originally double, the proximity of the members was such that they might be brought into collision by perturbation, while in the gaseous state. The ratio, moreover, of the ascending series of intervals is considerably less than in the case of Saturn, and much less than in the primary system.

ART. X.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxxv, p. 60, 1863.)

No. 281. *Carex conjuncta*, Boott, Illust. No. 282.
—— *vulpina*, Sullivant, Carey, Dewey.

Spica composita oblonga; spiculis ovatis sessilibus superné staminiferis et aggregatis, inferné laxis sæpe sub-ramosis et subremotis, bracteatis; fructibus *distigmaticis* ovatis acutis brevirostratis bidentatis stipitatis et interdum sub-cordatis margine subscabris divergentibus, squamam ovatam acutam paulo superantibus; spica plantaque pallido-viridi; culmo 1–3 pedali latetriquetri, cum foliis sub-radicalibus et longis.

This form was discovered some years since in Ohio, and named by Mr. Sullivant, after the European plant, C. vulpina of Linnæus. So it was held to be by Mr. Carey and others, and under that name described in this Journal, vol. viii, p. 348, 1849. Dr. Boott, however, thought it differed from C. vulpina, and gave it the name above. The figures in the Illustrations, No. 282, were taken from plants collected near Columbus, Ohio, and fully resemble those from Menard Co., Ill., now before me.

In the Illustrations, Dr. Boott gave the following particulars in which this plant differs from the European *C. vulpina*, viz: a very "acute-angled and flaccid culm," sheaths of the leaves transversely corrugated, spike more lax and pale, inflorescence of a pallid color, and its general appearance and habit. These or similar characters show the great difference between C. cephalophora and C. Leavenworthii, and doubtless should be considered important and distinguishing, even when, as in this case, the characters taken from the inflorescence are so nearly the same.

This species resembles C. sparganioides, but its *nerved* fruit separates it; as does the form of the fruit from C. stipata. The specimens of C. vulpina, *L.*, from England, Germany, and Swe-

* For some suggestions in regard to Mercury, see this Jour. for July, 1864. p. 13.

den, have an inflorescence more *compact* and *reddish brown;* and the plant generally has a *deeper green*, and is also *stouter.*

282. *C. glabra*, Boott, Illust. No. 229.

Spicis distinctis cylindraceis pedunculatis; terminali staminifera, interdum ad apicen vel basin pistillifera, vel in medio; spicis pistilliferis, 3–4, sub-laxifloris, bracteatis, infernè longo-pedunculatis et nutantibus; fructibus *tristigmaticis* ovalibus sub-inflatis et infernè teretibus, supernè conicis brevi-rostratis, bidentatis glabris nervosis, squamam oblongam sub-acutam vel lanceolatam multo superantibus; planta pallida et glabra.

Culm 1½ foot high, slender, erect, leafy toward the root; staminate spike slender, sometimes with a few fruit at the apex, in the middle or at the base; pistillate spikes 3–4, cylindric, slender, rather loose flowered, on slender peduncles with sheathing bracts which equal the culm nearly; stigmas 3; fruit oval, tapering below and conic above, smooth and glabrous, nearly twice longer than the oblong acutish scale which is white on the margin and green on the back: whole plant light green.

This plant is the well-known *glabrous* form of C. flexuosa, *Schk.*, the C. debilis, *Mx.*, and blended with it till Dr. Boott separated them in 1860. More than 40 years ago it was in our country commonly labelled from fig. 124, Schk.

283. *C. Magellanica*, Lam. 1789, and Schk., 1802. Boott, No. 199.
C. limosa, var. irrigua, Wahl. 1803; Dewey, 1826.
C. paupercula, Mx. 1803, and Torrey, 1843, and Boott.
C. irrigua, Smith, English Botany, 1845.

Spicis pedunculatis cylindraceis vel brevi-oblongis ferrugineis; terminali vulgo staminifera brevi, raro apicen pistillifera; spicis pistilliferis 1–3, subremotis recurvo-pedunculatis, bracteatis, laxifloris, interdum basin stameniferis; fructibus, *tristigmaticis*, longo-rotundis obovatis sub-triquetris vix rostratis ore integris stipitatis, squama ovato-oblonga acuta vel lanceolata brevioribus; culmis, foliis, bracteisque glaucis.

The stamens at the base of the fertile spikes have not been noticed by Torrey, Carey, and others generally who have collected the plant. They seem to me to be less common than they appeared to Dr. Boott, though I find them, and they were found by Schk. The American plant seems to be a larger and stronger plant than the European, even to the European botanists.

Though the resemblance of the figure of C. Magellanica in Schk. to C. limosa may have been noticed, it was reserved for Dr. Boott to prove, by examination of the specimens of Lamarck, their identity with common C. irrigua of Europe and North America. Lamarck's name, being the oldest, must be the accepted name of this species. It was briefly described in this

Journal, volume x, page 42, 1826, under the above name of Wahlenberg.

In cold marshes in the Northern and Western States, and Canada.

284. *C. rariflora*, Smith, Eng. Bot., 1845.
C. limosa, var. rariflora, Wahl., 1803 and Schk., 1812.

Spica terminali staminifera brevi; spicis pistilliferis 1–3, sæpe 2, linearibus brevibus laxifloris pedunculatis et nutantibus, bracteatis vix remotis; fructibus, *tristigmaticis*, ovatis oblongis vel ellipticis triquetris subcompressis obtusis apiculatis ore integris, squamam ovatam obtusam vel acutam sub-fuscam subæquantibus; planta glauca brevi et gracili.

White Mountains, N. H., on borders of Blue Pond—Dr. Barratt: northern parts of Europe and America. As a variety of C. limosa, this was very briefly described in this Journal with the preceding species.

In the *Summa* (p. 233, 1846) of the acute Fries, he has spoken of this C. rariflora, "quasi forma reducta," *as if a reduced form*, "scarcely a span high," of his preceding species, C. Stygia; but C. Stygia seems more nearly related to C. limosa, *L.*, than is C. rariflora. Hence, we find that Lang, in his *Scandinavian Carices*, admits both as proper species; and he will doubtless be approved by others.

Note. The following rare variety of C. limosa *L.*, was discovered the last summer by Rev. John A. Paine, of Utica, who has been actively and successfully engaged in looking up the *rare* as well as the common plants.

C. limosa, L., var. *Painei*, Dew.

Culm with the terminal spike staminate, and one pistillate spike of the common size and form on a slender or hair-like peduncle, from five to nine inches long, rising from near the root. In one instance there was a single pistillate spike on the culm and near the staminate spike, and the long radical pedunculate spike as before. The common form and the variety here described were found growing on different culms from the same root.

This is a very curious variety, and seems not to have been discovered before. Dr. Boott says of the lowest pistillate spike of C. Magellanica, "rarius subradicalis;" but this is on C. limosa and is truly radical, while he has figured that as subradical and short pedunculate instead of very long.

285. *C. mirata*, Dew.

Spicis 3–5, longo-cylindraceis inclusè pedunculatis longe-folioso-bracteatis; spicis staminiferis 1–3, sæpe 2, approximatis, in-

terdum ad basin vel erga apicen pauco fructiferis, cum glumis longis arctis attenuatis scabro-subulatis; spicis pistilliferis 1–2, laxifloris suberectis sub-remotis ad apicen vulgo staminiferis; fructibus parvi-ovatis longo-conicis vel lanceolatis vix inflatis nervosis vel striatis longo-stipitatis divergentibus rostratis, rostro profundi-fisso bicuspidato interdum bifurcato vel bidentato; glumis fructiferis lineari-lanceolatis scabro-subulatis, fructu superno spicæ brevioribus, fructum inferiorum æquantibus, atque fructus infimos plus duplo superantibus: culmo superne scabro, inferne obtusi triquetri et lævi; foliis bracteisque nodosis et margine scabris.

Culm 15–20 inches high, stout and stiff, triquetrous, obtuse-angled and smooth below the lowest bract, but rough above it, with long and broad leaves from the base and long leaf-like bracts, longer than the culm, and the lower the longer, both net-veined and very scabrous on the edges; staminate spikes 1–3, commonly 2, long-cylindric, near but the lower more remote, covered with long narrow bristle-form glumes attenuate and rough-subulate, with a few fruit at their base or toward the summit; pistillate spikes 1–2, often 1, long cylindric, suberect, exsert-pedunculate, at their summit staminiferous: stigmas three; fruit round and small ovate, long-conic, diverging, scarcely inflated, long-stipitate, with a beak deeply bifid, sometimes bicuspidate or bifurcate or bidentate with scales ovate lanceolate, rough-subulate, shorter than the fruit at the upper part of the spikes, equalling the fruit along the lower, and sometimes several, four to ten, of the lowest scales even twice as long as the fruit. Plant pale green.

In Greece, eleven miles west of Rochester and six south of Lake Ontario, in 1829, by Dr. S. B. Bradley. It was not named for a long time, as its anomalous characters made more specimens desirable. But no others have been discovered in the vicinity of this locality, which the clearing of the forest destroyed. In Belleville, Canada West, in 1863–4 by John Macoun, Esq. One form, sent by him, accords with the above description, especially in the varieties of the fruit, of the rostrum, and the very long scale of the fruit at the base of the lower pistillate spike.

Dr. Boott in Illust. No. 58, refers this species to C. aristata, *R. Brown*, by a mistaken[1] reference to my description of C. aris-

[1] His reference, "C. mirata, *Dewey*, Sill. Jour., xxvii, 240, v, 49, 48; Wood's Bot.," should have been *C. aristata*, Dewey, *Sill. J.*, xxviii, 240, vol. xlix, 48 p.; *C. mirata*, Dewey, *Wood's Bot.* Not only was the volume wrong, but *mirata* was unintentionally substituted for *aristata* by Dr. Boott, which changed the whole subject. Indeed the full description of C. aristata, Sill. J., vol. xxviii, 1835, referred to by Dr. Boott, was printed years before I had heard of C. mirata, proving that the description was of C. aristata evidently, and of no other, it may properly be added, as Dr. Boott implies in 1858.

If the "Note," Sill. Jour., xlix, 48, 1845, is noticed with care, it is obvious a *mistake* is made, because the last sentence *denies* what the preceding *asserts*. It will

tata, which has no connection with any other specimens, and by that self-destructive "Note," vol. xlviii, p. 49.

When No. 58 was published, Dr. Boott had not seen any specimen of C. mirata; but since that time he has seen two specimens of it, which I forwarded to him, and one of which was a present to him from Dr. Bradley. Circumstances led me to suppose he had changed his opinion in consequence; but his death, so unfortunate for this science, has prevented the publication of the 4th Part, which was to contain it and was known to be nearly ready for publication. What measures can be taken to secure its being printed and circulated?

Another form, judged by Mr. Macoun to belong to this species, has been provisionally named

C. mirata, var. minor, *Dew.*

Staminate spikes 2–3, often 3, long and short, and scales slightly larger; pistillate spikes 1–2, some shorter with stamens at the apex, or below it, or both, as in C. mirata, with fruit slightly more inflated, and its scales generally shorter than the fruit; plant older, with brown spikes and scales.

In Belleville, C. W.; Mr. Macoun.

Note. C. rostrata, *Mx.*, commonly obtained from ponds near the base of the White Mts. N. H., was found most abundant by Rev. Mr. Paine of Utica, in August last, at a pond or small lake near the well known Bald Rock or Mt. in the N. E. part of Herkimer Co., at the west foot of the Adirondack Mountains. Muhlenberg had not seen it, as he referred this name, *rostrata*, to his C. tentaculata. It was not recognized from the time of Michaux to 1840, when it was described in this Journal, vol. xxxix, p. 52.

C. lenticularis, *Mx.*, was found also by Mr. Paine along the chain of the "Eight Lakes," in Herkimer Co.

C. Houghtonii, *Torr.*, has been found, the last season, 60 miles north of Belleville, by John Macoun, Esq., an active and discriminating botanist. The same species has been found, the last two seasons, by Rev. Mr. Blake, of Gilmanton, N. H., and in Milford, north of Bangor, Me. With great pertinence was it named by Dr. Torrey.

be rectified by the insertion of C. aristata, so as to read, "For description and figure of C. aristata, *R. Br.*, refer to vol. xxv ., p. 240, of this Journal; tab. v, fig. 67. This species the fig. 57 finely represents, while it is very unlike C. mirata, as is that description in vol. xxviii. So certain is it, that the "Note" assumes the great difference between C. aristata and C. mirata, instead of uniting them.

ART. XI.—*On the Action of Ozone upon Insensitive Iodid and Bromid of Silver;* by M. CAREY LEA, Philadelphia.

A STATEMENT has recently appeared in the French scientific Journal *Les Mondes*, from which it has been copied,[1] to the effect that ozone is capable of giving sensitiveness to insensitive iodid of silver. Mr. J. P. Kaiser states that he has found that certain vapors, such as the vapor of benzine, exercise a powerful effect of this kind, converting one insensitive variety into one that was highly sensitive: that he attributed this effect to the ozone produced by the contact of this vapor with atmospheric air:—that he therefore experimented with air ozonized by a galvanic induction apparatus, and found the same results produced, but in a much more marked degree.

This statement was not altogether the first suggestion of the matter which I had seen. A French Photographic Journal narrated, a few months back, a case in which a photographer, whose plates had refused to afford any image at all, restored their sensitiveness and got them into satisfactory working order by simply generating ozone in the room. To this latter statement, I was disposed to attach little importance. But the explicit and circumstantial one just above referred to, was a different matter.

Those who put forth these statements do not seem to have been aware of their true importance. Not only would such a fact be of great theoretical interest, but its practical applications would be immense, especially in reference to out-door photography, the easy execution of which on wet collodion would at once have become practical. For plates could be coated with insensitive iodid of silver in ordinary light, (say, for example, after Mr. Sayce's formula). This plate could be placed in an ordinary dark slide; and a vessel containing the means of generating ozone could be placed in the camera. On withdrawing the dark shutter from the slide, the plate would be uncovered, and exposed to the ozonized air of the camera. In due time the lens would be uncovered, and the image allowed to fall on the now sensitive surface. The development would then be effected in one of Weiske's developing cases, and thus the whole process would be reduced to the utmost simplicity.

In view, therefore, of the interest which attaches to this question, I was led to make the following trials. In place of using ozone generated by electricity, as done by M. Kaiser, I preferred to generate it by chemical means, viz: by phosphorus, and also by the action of sulphuric acid on chameleon mineral. As the

[1] See British Journal of Photography, 1864, p. 392. Also Phot. Mittheilungen, Nov. 1864, in which it is suggested that ozone acts by destroying a trace of alkaline iodid present.

source of the ozone was not the same as that used by M. Kaiser, my results cannot be considered as strictly controlling his, but the action of ozone from the two sources is so similar that we should naturally expect similar results in the two cases.

A. Ozone by phosphorus.

The ozone was generated in a large bell glass, and the experiments were not commenced till paper impregnated with starch and alkaline iodid exhibited an immediate and strong ozone reaction.

I. *Action on Iodid of Silver.*

Paper was plunged into an ordinary negative bath, and dried. Strips were immersed in solution of iodid of potassium, and without leaving them in too long, were next thrown into clean water and washed.

(1.) A piece of this paper, thus imbued with washed insensitive iodid of silver, was placed in the ozone apparatus for two minutes. It was then exposed to diffuse daylight for six seconds. The application of an iron developer produced no darkening whatever. A longer exposure to light was also without effect. The paper was just as insensitive as before being exposed to the ozone.

(2.) The effects of a longer exposure to the ozone were next tried. The paper, prepared as before, was exposed for half an hour to the action of the ozone, and exposed to a moderate diffuse daylight for twenty seconds. The iron developer produced no effect whatever.

(3.) Same as (2), but exposed to light for thirty seconds. Result as before.

(4.) The paper was prepared as before, but, after immersion in the solution of iodid of potassium, the washing in water was omitted, and the strip was placed in the ozone apparatus just as it left the solution of iodid. The paper immediately changed to a deep chocolate brown, while still in the ozone apparatus. This effect was at once attributed to the action of the ozone on the free alkaline iodid, but to place the matter beyond doubt, the paper was thrown into a solution of hyposulphite of soda, which instantly bleached it.

It seems hardly likely that this reaction, so well known, could have been mistaken for an indication of sensitiveness to light. Perhaps, if the exposure to ozone had been conducted in diffuse daylight instead of in a dark room, a careless experimenter might have been misled, by the similarity of the chocolate-brown color produced to that so often occurring in photography, into a misapprehension of the agency at work, and might have supposed that the ozone had rendered the insensitive iodid of silver sensi-

tive to the diffuse light of the room. Such an error seems, however, unlikely.

(5.) Paper was prepared as before, was exposed fifteen minutes to the action of ozone, then one minute to the light of a strong argand gas burner, one half being protected from the light. An iron developer was then applied. No effect was produced, nor could the side exposed to the light be distinguished in any way, after the application of the developer, from that which had not been exposed.

[It was previously ascertained that a sensitized collodion plate placed in the same position as respects the same light, was powerfully impressed in 20 seconds, or one-third the time just mentioned.]

(6.) Paper was prepared in the same manner, but after placing in the solution of iodid, and washing, it was dried, and in this condition exposed to the ozone for fifteen minutes. Then to the same gas light for one minute, partly covered. Result same as number 5.

(7.) After treatment with silver and iodid, and washing, the paper was exposed to ozone for fifteen minutes, then to the same gas light as in (5) and (6) for one minute, partly covered. It was then developed with pyrogallic acid to which nitrate of silver and citric acid had been added, and this was kept on till the paper was blackened by the deposition of silver. Not the slightest distinction could be traced between the parts that had been exposed to light, and those that had been produced by thick yellow paper.

II.—*Action on Bromid of Silver.*

(8.) In order to give a greater variety to the experiments, the bromid of silver submitted to trial was not disseminated through paper as was done with the iodid in the foregoing trials, but was used in a collodion film. For this purpose a collodion was made somewhat similar in character to that proposed by Mr. Sayce, in which the bromid of silver is suspended in the collodion with its sensibility destroyed by the presence of a small excess of alkaline bromid. The proportions used were as follows:

Ether,	½ ounce.
Alcohol,	1 "
Pyroxyline,	10 grs.
Bromid of ammonium,	9 "
Nitrate of silver,	13½ "

These proportions leave a slight excess of bromid of ammonium present, which ensures that the bromid of silver is in the insensitive form.

(9.) A portion of this collodion was poured on glass and ex-

posed to a highly ozonized atmosphere for three minutes. It was then exposed to a large gas light for fifty seconds. On the application of an iron developer no darkening was observable.

(10.) In order to afford a term of comparison, a film of ordinary collodion was sensitized in the negative bath in the usual manner, and was exposed to the same light for twenty seconds. On the application of the same developer it was strongly darkened.

(11.) The same collodion as in (9) was exposed to the same ozonized atmosphere for 45 minutes. It was next exposed to the same gas light as (8) and (9) for thirty seconds. The application of the developer produced no darkening.

(12.) Same as (10), but exposed to light for one and a half minutes. Result as before.

(13.) Same, but exposed to light for three minutes. Result as before. In no case was the slightest reducing effect produced.

B. Ozone by chameleon mineral.

Action on Bromid of Silver.

The bromid of silver was used in the same form as before, viz: in collodion containing a small excess of bromid of ammonium. I should have mentioned before that the insensibility to light of this argento-ozonized collodion was first carefully tested and proved. Ozone was generated in a closed box by the action of undiluted sulphuric acid on chameleon mineral; the vessel containing which was set aside for a short time to let the purple vapors pass off. It was then set in the box, and the condition of the atmosphere was examined from time to time by appropriate test paper.

(14.) Before commencing with the ozone trials, a piece of glass was collodionized with ordinary collodion and sensitized in the negative bath. It was then exposed to a weak diffuse daylight for thirty seconds, after which it immediately darkened under the developer. This was to determine the action and power of the light in question.

(15.) The collodion containing insensitive bromid was exposed to a weakly ozonized atmosphere for ten minutes. It was then exposed to the same daylight as the ordinary sensitive plate mentioned in (14), and for the same time. An iron developer was then applied, but not the slightest indication of sensitiveness was observable.

(16.) The last experiment was repeated under the same conditions, but with a treble exposure to the action of the same ozonized atmosphere, viz: thirty minutes instead of ten. The exposure to light was the same, and the insensitiveness the same.

(17.) Instead of a weakly ozonized atmosphere, as in the last

two cases, a strong one was prepared, and a plate similarly prepared with insensitive argento-bromid collodion was placed in it ten minutes. It was then exposed to the same light and treated in the same manner as in the experiment immediately preceding, and with the same results:—not the slightest indication of sensibility.

In this last experiment, the atmosphere was ozonized sufficiently to render iodid of potassium and starch paper instantly blue. In the two previous, this reaction showed itself more gradually.

In remarking upon the experiments just detailed, I may observe in the first place that the ozone was unquestionably always present in such strength as to bring out its marked chemical effects. This was demonstrated not only by its action on the characteristic test paper, but also by its chemical action exhibited in experiment (4).

Again, the experiments were tried in the most varied ways. The ozone was produced in two different manners, and of varied strength. The surfaces to be tested were also exposed for widely varied times, from two minutes to forty-five.

The nature of the light was also varied, experiment having been made both with daylight and artificial light. The time of exposure to light was very various, and in several cases, after the first results had been noted, the paper or film still wet with developer, was carried into the light and exposed for some time, to see if any faint sensibility existed and would manifest itself by the prolonged action of light. No such result appeared. The experiments included both iodid and bromid of silver, and both paper (of which two different sorts were intentionally used) and collodion were used as the vehicles for the silver compound.

The result appears to show pretty clearly that ozone has no power of giving sensibility to insensitive iodid or bromid of silver formed in the presence of excess of alkaline iodid, whether the excess be left present, as in the bromid experiments, or be removed, as in those made with iodid. Or at least that this is true in respect to ozone produced in the two manners which I have described. If Mr. Kaiser has really succeeded in sensitizing plates in the manner described by him, I think that result might possibly be ascribable to some direct effect of the induced electricity which he employs, and not to the ozone produced. But the description of his experiments which has reached me in the *British Journal* is so brief (no details whatever are given) that the above can only be considered as a mere suggestion of an explanation of the opposite results obtained by him. The whole subject is an interesting one, and the effect of all the forms of physical force, especially electricity and heat, on the silver haloids might no doubt be advantageously studied.

ART. XII.—*Prize for applications of the Electric Pile.—Heterogeny.—Influence of Light on Proto-organisms.—Association for the Advancement of Meteorology.—On the intensity of action of the Solar disk.—Acclimation of Salmon in Australia.—Production of the Sexes.—Cutting of the Isthmus of Suez.—First idea of Electric Telegraphy.* Correspondence of Prof. J. NICKLÈS, dated Nancy, France, Oct. 20, 1864.

Prize for applications of the electric pile.—The prize of 50,000 francs, which was founded in 1852 by the Emperor Napoleon, has just been decreed to Ruhmkorff, for the induction apparatus known as "Ruhmkorff's coil," the mechanism of which we were among the first to make known.[1] The committee, consisting of Messrs. Pelouze, Rayer, Serres, Becquerel, H. St. Claire Deville, and others, was presided over by Dumas. We make the following extracts from his report:

"Mr. Ruhmkorff was at first a workman for some of our best constructors of physical apparatus, afterward had his own workshop, and finally became head of a house of constantly increasing celebrity.

"His education was gained, little by little, through reflection, study, and the lectures of certain professors heard as it were by stealth in his occasional hours of leisure. Modest, of unyielding perseverance, and of a self-devotion which has earned for him the highest encomiums, Mr. Ruhmkorff will ever remain the type of his class—a model for the numerous intelligent workmen who fill the higher order of workshops (ateliers de precision) of Paris. To those, who, like him, know how to control their desires, who faithfully strive for perfection in work and clearness in conceptions, who bend their attention to one object, and labor untiringly until a high superiority is gained and also for themselves the satisfactions of a ripe age, the compensation for the sacrifices and privations of youth will not be lacking in a country where, more than ever, merit finds recompense.

"Since 1851, Mr. Ruhmkorff has devoted himself to the construction and perfection of his apparatus, and he has ended by securing for it his own name, by giving it a scientific value which no one contests, and by rendering it of so great power as to become a means of numerous practical applications.

"The Ruhmkorff apparatus unites the two forms of electricity, which had been separated as by an abyss—the old machine electricity, characterized by a capability of producing sparks and by strong tension, and the electricity of the pile, characterized by very feeble tension and by its inability to produce true sparks.
* * * *

[1] This Journal, 1853, xv, 114; 1862, vol. xxxiii.

"If Franklin's discoveries placed beyond doubt the identity of mechanical electricity and lightning, there remained, nevertheless, among the phenomena which accompany storms, many circumstances the explanation of which was yet inaccessible to science. We must therefore regard as a valuble acquisition to meteorology the observation that the spark of the Ruhmkorff apparatus consists of two parts—an instantaneous line of light and an areola of measurable duration. The magnet divides the latter; a breath, or any body in motion, draws it out, and the electric spark thus divided continues its route in these two directions at once, as long as the passage of the current continues uninterrupted.

"In a vacuum, the electric spark develops light, and that developed by the Ruhmkorff spark takes different colors in different gases; it brilliantly illuminates fluorescent bodies, and divides itself into parallel bands separated by dark spaces perpendicular to the axis of the recipients. These colored luminous bands obey the action of the magnet which attracts or repels them, and which impresses upon them, at the pleasure of the operator, those movements of translation or of rotation by means of which De la Rive[2] has reproduced the appearances observed in the Aurora Borealis, justifying, thus, the analogy recognized between the electric light produced in a vacuum and that of the polar auroras.

"Glass tubes illuminated by the same means give out a light sufficiently bright to be used in mines that are liable to explosion: they also serve under water as a lamp for divers—and in surgery, to carry into the back part of the mouth, and other cavities of the body, a light which gives no sensation of heat.

"The spark of the Ruhmkorff apparatus inflames combustibles, and explodes gaseous mixtures; and it has thus supplied to Lenoir's gas-engine the means necessary for producing the successive inflammations to which it owes its mechanical power.

"The working of quarries, the boring of tunnels, the explosion of heavy charges in mines, give to-day regular employment to the Ruhmkorff apparatus.[3] Mines had been exploded previously by the aid of the pile; but the Ruhmkorff apparatus has left all other modifications far behind, by reason of the small number of elements which it requires—three instead of one hundred—the power of its spark, and finally the possibility of exploding eight or ten mines at once. In the expedition to China, in 1860, the Ruhmkorff apparatus was used to blow up, by means of eight mines simultaneously exploded, the principal fort of the Peiho, as well as the iron stockade at the bottom of the river."

Dumas then reviews the principal applications of electricity to Mechanics, such as the *automatic break* of Achard, the weaving-

[2] This Journal, 1853, vol. xvi. [3] This Journal, 1854, vol. xviii.

looms, the pentagraph of Caselli, the writing telegraph of Prof. Hughes, etc. He then spoke of illumination by electricity, and mentioned the electric regulators of Staite, Serrin, and Dubosc, the illumination of light-houses by induction currents, observing that successful experiments have been made with it by the Light-house Board: at Havre, upon Cape de la Hève it erected, several years ago, an electric lighthouse equivalent to 3000 Carcel burners, by the side of one lighted with oil; the electric light was readily distinguished, by its brilliancy and intensity, from its neighbor which appeared red.

Under *Medical electricity* there was a reference to the observations of Dr. Duchenne of Boulogne and those of Middledorf, of which we have given an account when treating of Galvanocaustic, and according to which, by means of platinum wires brought to incandescence, the tissues may be divided, and the removal effected of polypi and tumors from organs that are deeply seated or otherwise difficultly accessible.

A new competition will take place, five years hence, to which all applications of electricity will be admitted, whether to medicine, the mechanic arts, or industry, without distinction of origin or of nationality.

Heterogeny.—The discussion which has existed for several years between the partisans of spontaneous generation and their opponents, is entering upon a new phase. The two parties desire to unite and experiment together, for it is in that direction that a beginning must be made. Thus it has happened, as in politics, at first fighting, then, conferences or conventions.

Whatever be the opinion on this great subject of generation, it is impossible that new and instructive facts should not be brought out by this convention, in which chemists of known skill are assisted by such physiologists as Pouchet and Joley. There is every reason to believe that each party holds some truth, and that their differences have arisen from their different methods of experimenting. At any rate, there is no opportunity for temper in the debate.[4]

Influence of light upon the production of proto-organisms.—One element of the solution of this question of heterogeny resides in the sun's light, which produces some effects that are not known to result from artificial light or heat, as has been already made known by Messrs. Pouchet and Ch. Morren. Some new facts in

[4] Since the above was written, several meetings have been held at the Garden of Plants in Paris. Nothing has resulted from this convention except the very sad fact that theological considerations seem to have got the better of the attentive observation of facts, just as in the time ef Galileo. There is, then, nothing to do but to observe that the savants have all agreed that reason, which doubts, observes and studies, has nothing in common with that kind of faith which believes without examination and without proofs.

their support have been observed by Professor Montegazza at the University of Pavia.

Two female frogs were quickly killed, by the destruction of the spinal marrow, and placed in two glass vases each of which contained 115 cubic centigrams of well-water perfectly transparent and free from all foreign bodies. One of these vases was left in diffused light, the other was placed in a box which did not permit a single ray of light to reach it.

The experiment was continued seventeen months. It is necessary, before giving the results of this long trial, to state that every time that the two vases were compared, they remained exposed to the air for an hour or more, and that the air was renewed at each observation. Germs could, then, easily fall in; and yet the results were very different in the two cases.

The following are the observed results:—

1. Two identical bodies exposed to free air may present very different phenomena of putrefaction, according as they are exposed to the influence of the light, or shut off from it.

2. The chemical and biological phenomena of the two forms of putrefaction are very different, that is, we have in each case some special chemical products, and some peculiar animal and vegetable productions.

3. In darkness, there was a marked tendency to the production of vegetable organisms and very simple infusoria; the frog that underwent putrefaction while shut off from the light produced only some Mucedines, Monads and Vibrios; while the other afforded a very complicated fauna—Bacteriums, Vibrios, Spirellas, Monads of different species, Amœbas, Kerones, Alysum, Enchelides, Trachelius,—and finally Infusorians still undescribed, much resembling the Zoosperms of the Tritons.

4. The abundance and superior organization of the Infusoria depend much more upon the progress of the putrefaction than upon the amount of putrescible matter. The more simple species always appear first.

5. The production of species of Bacterium takes place many times during the course of a long putrefaction.

6. When the liquid presents a new fauna, the new species are from the outset represented by a number of individuals at once; from one day to the next, they are simultaneously produced.

7. In the course of a long putrefaction, there are some generations which endure for some days; others exist for a much longer time.

8. Rapid changes in the chemical composition of a putrescible liquid are always, or nearly always, followed by new sets of animal and vegetable microscopic life.

9. When circumstances are little favorable to heterogeny in a very long putrefaction, there may be intervals of time of greater or less length, in which the liquid presents no organism. Who-

ever should content himself with observing at such a moment, might say that there had been no generation; while some days before, or some days after, there had been, or there would be, a very abundant production of vegetables, or of animals, or of both at once.

From all the comparative tables, I select an observation made on November 20th, after more than seven months of putrefaction. Temperature at the time 10° C.

In the light.—The liquid had a slight odor of boiled meat.—On the surface many very lively Kolpodes, a great quantity of Vibrios, some Alysca.—Some Infusoria which strikingly resemble the Zoösperms of Tritons.—At the bottom, some dead Kolpodes, and some in the act of multiplying by division into two or four individuals. Many Vibrios; some Monads.

In the dark.—The liquid had a very strong odor of mushrooms. —No organisms; the whole mass liquid.

Association for the advancement of Meteorology.—This association, founded at the commencement of the year 1864, under the presidency of Le Verrier, is a great success. It meets periodically at the Observatory of Paris. It has just founded prizes for the encouragement of meteorological studies, and especially the study of the general movements of the atmosphere. All meteorologists are admitted to competition, without distinction of nationality.

The following are some of the details of the programme:

"According to the generally received opinion, the storms of the coasts of France come, already formed, from the Atlantic. An extended series of meteorological observations made over this vast sea, is, then, the necessary basis of the work proposed for the principal prize. This prize will be of 4000 francs. The memoirs must be delivered to the Secretary of the Society before December 31st, 1865.

"A sum of 3000 francs will be divided between the authors of the best observations made at sea, or in places little known in a meteorological point of view.

"Finally, two prizes, of 500 francs each, are offered for the best memoirs upon the application of Meteorology to agricultural questions. The prizes of 300 and 500 francs may consist of instruments for observation."

Upon the intensity of action of different parts of the solar disk.—With regard to the late researches by Secchi—according to which the calorific radiation of the center of the solar disk is greater than that of the borders, nearly in the ratio of 2 : 1, Mr. Volpicelli writes that the fact was very exactly observed in 1614 by Luc Valerio, a mathematician of Naples, author of a work, *De centro gravitatis solidorum*, and of another *De quadratura parabolæ per simplex falsum.* He was a professor in the Roman University, and has been called the Archimedes of his age.

In one of his letters to Galileo, Luc Valerio considers the rays proceding from the central part of the solar disk as the more active.

Analogous facts have been observed by Mr. Roscoe, according to whom the center of the disk exerts a more intense chemical action than the borders. He has also observed that the south polar zone is more active than the north.

Acclimation of Salmon in Australia.—Recent experiments carried forward by the Acclimation Society have shown that it is possible to transport to distant countries the eggs of fertile fishes. One of its members, Mr. Millet, having observed that melting ice diminished the pulsations of the young fish of the Salmonidæ and delayed the hatching of the eggs, took the idea that this method would serve for the transportation of eggs of the Salmonidæ to Australia and Tasmania. The plan has succeeded, in spite of damaged eggs; a very large number have arrived there in a healthy state, and have been deposited in the rivers. Starting from London on January 15th, 1864, the eggs arrived at Melbourne on April 5th, and at Hobart Town, (Tasmania,) on the 23d. Everything indicates success.

Similar attempts have been made in the French possessions in Algeria, the rivers of which are very barren of fish. Eggs of salmon and trout have been carried there from Huningue (Haut-Rhin), where, as we have previously seen, are found the principal basins for pisciculture. In spite of the differences of climate, these eggs have arrived safely, and have hatched in the basins prepared for them; they already begin to people the rivers. It is a result of no moderate interest to see the salmon of the Rhine and the trout of the Vosges transported to Africa, Australia, and Tasmania, and living and becoming acclimated there.

Production of the sexes.—What are the causes of the production of the sexes? This question, which has occupied the earnest attention of physiologists, has been thoroughly studied by Mr. Thury, according to whom the product is always of the male sex when the fertilization of the ova occurs at complete maturity, and is always female when it takes place at a less advanced period.

There is a very simple way of solving this problem. It is to select for experiment species that come to maturity in succession, and, that during a single impregnation, fertilize the whole series of ova which detach themselves from the ovary during a period of eight, ten, twelve, fifteen, or even eighteen days. We know, indeed, that, in the case of the hen, a single coupling suffices for the fertilization of five, six, or seven eggs which she is about to lay and which are arranged in her ovary in the order of their maturation. Now, in such a case, if the theory is exact, the first egg laid ought always to produce males and the others females without any possibility of the inversion of this order.

This is very near what has been observed by Messrs. Coste and Gerbe:

A hen, separated from the cock at the time of her first laying this year, gave five fertile eggs in the space of eight days.

The egg laid on March 15th produced a male; that on March 17th a male; that on the 18th a female; that on the 20th a male; that on the 22d a female.

A characteristic fact in this experiment is the production of a male after a female, which ought not to have taken place according to the theory. But is it only a simple exception? Or is it necessary to consider the fact a radical objection? We may learn by and by on this point, from the researches in which Mr. Gerbe is now engaged.

On the occasion of the preceding note, Flourens recalled an experiment which he made, thirty years ago.

"Aristotle had observed that the pigeon ordinarily lays two eggs, and that of these two eggs one commonly produces a male and the other a female. He wished to know which was the egg that gave the male, and which the one that produced the female. He found that the first egg always gave the male, and the second the female. I have repeated this experiment as many as eleven times in succession, and eleven times in succession the first egg gave the male and the second egg the female. I have seen again that which Aristotle saw."

Cutting of the Isthmus of Suez.—The almost certain success of the canal across the Isthmus of Suez fixes attention, more than ever, upon other projects of the kind. The cutting of the isthmus of Malacca and of that of Darien await only the completion of the Suez ship canal. In France, they are talking of uniting the Atlantic Ocean with the Mediterranean by a ship canal which would borrow part of its route from the old Southern Canal. In Holland, a society is incorporated, under the title of a "Company for the cutting of the isthmus of Holland," to establish a canal joining Amsterdam and the North Sea by a direct route, while, at the same time, the Dutch government has undertaken to establish another navigable route from Rotterdam to the sea.

A new project is presented, which, if realized, will complete the series of maritime communications in the north of Europe. It is proposed to establish a canal, navigable for ships of war and commerce, between the North Sea and the Baltic, to avoid making the passage of the Danish islands. This project, often thought of, is now very seriously considered.

Finally, they are speaking of cutting one other isthmus, and this time it is Spain which has the honor. It is proposed to pierce the Spanish isthmus in such a way that Gibraltar will be an island. The canal is to start from Trafalgar and end at Andalusia. This canal, which would cost hardly a hundred millions of francs, has for its object to prevent more than 4000 vessels

every year from lying to before the strait of Gibraltar without power to get out.

This project is just now submitted to the examination of the Spanish government, as well as to some others, such as the English and French governments.

First idea of an electric telegraph.—This first idea was brought forward in the 16th century in an anonymous work published at Rouen at that time. The question has already been discussed here. A discovery has just been made of a letter of a clerk, the abbè of Barthélemy, well known as a literary man. He writes to Madame du Dessaud, April 8th, 1772:—

"I think often of an experiment which would give us happiness. They say that with two clocks whose hands are magnetized it is sufficient to move the hands of one for those of the other to take the same direction, so that causing one to strike twelve the other will strike the same hour. Suppose that we could perfect artificial magnets so that their power would communicate itself from here to Paris: you would have one of these clocks, we would have another; upon the dial we should have the letters of the alphabet. . . . You perceive that we could thus facilitate the operation; the first movement of the hand might sound a bell which would announce that the oracle was about to speak."

Bibliography.—Among recent publications by Hachette, at Paris, are the following:

Les Assolements et les Systèmes de Culture, par M. Heuzey; 1 vol., 8vo, 540 pp.—This work contains the lectures delivered by Mr. Heuzey at the Imperial School of Agriculture at Grignon, of which he is one of the most distinguished professors. The principles which are here set forth are applicable to all countries which are to be put under cultivation.

Le Médecin des Salles d'Asile, par Dr. Cerise; 2d ed., 8vo, 200 pp.—The "Salles d'asile" of France are charitable institutions for the education of poor children. After a short historical account of the institution itself, the author proceeds to set forth the principles which should govern physicians in their supervision of these infant schools and all which relates to the care to be taken of a miscellaneous collection of children.

Abrégé de Géographie commerciale et industrielle, avec un tableau des monnaies, poids et mesures de tous les pays, par M. Gardon; 4th ed., 8vo, 400 pp.—While referring to all countries, the author deals especially with France, each department of which he describes fully as regards statistics, agricultural, mineralogical, metallurgical, manufacturing and commercial industry, importation, exportation, navigation, etc.

La terre avant le Déluge, par Louis Figuier; 4th ed., 8vo.—This is a popular work designed to initiate the people of the world in the grand phenomena of geology. It is illustrated with many colored geological maps, and more than 300 figures and ideal views of landscapes of the ancient world.

De l'Electricité considérée comme cáuse principale d'l action des eaux minérales sur l'organisme, par Dr. Scoutettin; 8vo, 420 pp. The material which mineral waters hold in solution being often in insufficient pro-

portions to account for their health-giving effects, the author has recourse to another explanation, viz: the electrical action which these waters may exert upon the human organization. We find in his work not merely a simple ideal explanation, but, on the contrary, the recital of numerous experiments made by the author upon the principal mineral waters of Europe.

L'Astronomie au XIXe siècle, par A. Boillot, 12mo, 340 pp. This work, written for universal use, gives a very faithful picture of the progress which astronomy has made from ancient times up to our day. Mr. Boillot is a very distinguished mathematician and makes astronomy his favorite study. His work has been received in France with much favor.[5]

Nancy, Oct. 20th, 1864.

ART. XIII.—*Discovery of Emery in Chester, Massachusetts;* by CHARLES T. JACKSON, M.D., Geologist and State Assayer.

IT has been said, in England, that "a good mine of emery is worth more to a manufacturing people than many mines of gold." Such being the case, it affords me great pleasure to be able to announce the discovery of an inexhaustible bed of the best emery in the world in the middle of the State of Massachusetts, in Chester, Hampden county, quite near to the Western Railroad, which with its ramifications leads to the largest armories and manufactories of metallic articles in this and the adjacent States.

For more than two years, the existence of important beds of magnetic iron ore, originally discovered by Dr. H. S. Lucas, has been known, and endeavors were made by that gentleman to organize a company for the purpose of smelting these ores. In consequence of this agitation, I was employed by John B. Taft, Esq., on the 19th of August, 1863, to examine the locality and to make report of my results to him.

On examination of my specimens of minerals, after returning to Boston, and my notes for sectional profiles of the rocky strata containing the iron ore, I found that the minerals, margarite and chloritoid, in talcose, hornblende, and mica slate rocks, indicated the occurrence of emery, the association of the rocks and minerals being identical with conditions known to exist in the localities of emery in Asia Minor.

[5] The following titles of literary works are also contained in the letter of Mr. Nicklès:

Histoire de la Littérature française, par M. Demogeot; 5th ed., 12mo, 680 pp.

Eléments de Rhetorique, par M. Filon; 12mo, 300 pp.

Les grandes scènes de la nature, par M. de Lanoye; 12mo.

Les Champs d'or de Bendigo (Australie), par M. Perron d'Arc; 12mo, 200 pp. A description of the Australian placers.

Souvenirs d'un Sibérien, par Rufin Piotrowsky; 12mo, 200 pp. This is a very touching recital of the misfortunes which affiict the unhappy Poles, for whom Europe has only barren sympathy.

I therefore called the attention of the owners of the property to these facts, and directed search to be made for emery, and that every mineral resembling it should be sent to me for examination. Little attention was paid to this prediction at the time, and not until I had invited Dr. Lucas, who resides in Chester, by personal representations and solicitations, to make the required search, the characters of emery being fully described to him.

On his return to Chester he soon learned that the miners were complaining of the great hardness of the supposed iron ore, and that no less than forty drills were dulled in boring a single hole for blasting. He then sent me pieces of this hard rock, in the belief that it was the emery I had predicted. On examination it was found to scratch quartz and topaz readily, and to have all the properties of emery; a chemical analysis proved it to be identical with the emery of Naxos.

The owners, resident in Boston, being notified of this discovery, went with me to the locality on the 11th of October last, when a full exploration of the premises was made. There are several large beds of rich magnetic iron ore at this locality, and the emery being magnetic (as it always is) had caused it to be mistaken for magnetic iron ore, and many tons of it had been smelted with the carbonate of iron and hematite in the Berkshire county iron furnaces, without a suspicion, notwithstanding its refractory nature, that the ore was emery, with only a small admixture of iron ore.

The principal bed of emery is seen at the immediate base of the South Mountain, where it is four feet wide, and cuts through the mountain near its summit, at an angle of 70° inclination or dip to the eastward. Its course is N. 20° E., S. 20° W., and its known extent four miles. Near the summit of the mountain the bed expands to more than 10 feet in width, and in some places is even 17 feet wide.

The alternations of rock in two sections are as follows, beginning to the eastward:

1. *a*, Mica slate; *b*, 15 ft. soapstone or talcose rock; *c*, 2 feet crystallized talc; *d*, talcose slate; *e*, 1 ft. granular quartz; *f*, chlorite slate; *g*, 4 ft. *Emery; h*, chloritoid and margarite; *i*, magnetic iron ore; *j*, hornblende rock highly crystalline.

2. *a*, Mica slate; *b*, 6 ft. magnetic iron ore; *c*, talcose slate; *d*, 6½ ft. magnetic iron ore; *e*, chlorite slate; *f*, hornblende rock, crystallized; *g*, 7 ft. *Emery*, chloritoid and margarite; *h*, magnetic iron ore; *i*, hornblende rock.

The elevation of the upper outcrop of this bed above the immediate base of the mountain is 750 feet. There are curious rounded masses of remarkably pure emery three feet in diameter in this bed entirely invested with a coat of delicate rose-colored margarite, and a thick layer of bright green chloritoid,

the investing coat being from half an inch to two inches in thickness. It is found extremely difficult to break up these masses of solid emery, drilling holes in them for blasting being very slow and laborious, and no grip can be had on their rounded sides by the sledge. A heavy drop hammer will be required to break them to pieces—or they may be cracked by fire if heat does not injure the emery.

A branch of Westfield river separates the South from the North mountain, a hill nearly 750 feet high, through the summit of which the great emery bed also cuts. On this hill the emery is more largely crystalline and less mixed with magnetic iron ore. It is more like corundum, but still contains the combined protoxyd of iron, characteristic of true emery. Its specific gravity is from 3·75 to 3·80, while that from the South mountain is from 4·02 to 4·37; Naxos emery being from 3·71 to 3·72, according to my trials of it, in comparison.

On the North mountain, three large beds of rich magnetic iron ore, distinct from the ore accompanying the emery, occur, the ore yielding 54¼ per cent of metallic iron. This ore is mined, and is smelted into bar iron by forge fires, and is also sold to mix with the hematites and carbonates of iron worked at the Lenox and Stockbridge furnaces.

Digesting the South mountain emery in fine powder with nitro-muriatic acid and sulphuric acid for a long time, it was found that 73 per cent of it was wholly insoluble in acids; and on microscopic examination the grains were seen to be translucent, and exactly like the Naxos emery prepared in the same way; but these translucent grains are readily taken up by the magnet. I therefore infer that protoxyd of iron is a chemical constituent of true emery.

Chemical analysis of the coarsely crystalline emery of the North Mountain, Chester. Sp. gr. 3·75. H.=9.

Alumina,	46·50
Protoxyd of iron,	44·00
Titanic acid,	5·00
Silica & loss,	4·50
	100·00

Emery of the South hill.[6] Sp. gr. 4·02. H.=9.

Alumina,	45·50
Protoxyd of iron,	43·00
Silica & titanic acid,	11·50
	100·00

Regarding the oxyd of iron which can be dissolved out from emery by acids as accidental, and that which cannot be so removed as an essential constituent, we shall have for the compo-

[6] The highest specific gravity of any sample from the South mountain was 4·3734.

sition of three samples of emery analysed, after digestion in acids,

	Chester, 1.	Chester, 2.	Naxos best selected emery.
Alumina,	60·4	59·05	62·3
Protoxyd iron,	39·6	40·95	37·7
	100·0	100·00	100·0

If this view is adopted, emery must be ranked as a distinct species, and not as a mere granular form of corundum or sapphire.

In conclusion, I would state that practical trials of the Chester emery, in several of the large armories and machine shops of this and the adjoining States, have proved it to be fully equal in value to the well known emery of Naxos, which I have no doubt it will wholly supplant in this country, and that it will ere long become an article of export to Europe, either in its native form, or in a manufactured state.

It may be proper to add, that John B. Taft, Esq., of Boston, in behalf of his associates, owners of the emery mine, has the sole management of the business connected with the mine.

I would express my obligations to Mr. J. L. Smith for the valuable information contained in his articles on the emery of Asia Minor and on the associated minerals of the emery localities, published in vols. x. and xi. of this Journal. Also to Dr. H. S. Lucas, of Chester, for kind assistance in the field.[7]

82 Somerset St., Boston, Dec. 12th, 1864.

Art. XIV.—*On the Solubility of the Sulphate of Baryta in Sulphuric Acid;* by Prof. J. Nicklès.

The sulphates of baryta, strontia, and lime, are slightly soluble, as is known, in boiling sulphuric acid. I have found that they are soluble in *cold* acid when in a nascent state.

To obtain this result, it is only necessary to throw a little chlorid of barium, or of strontium, into a sufficient quantity of monohydrated acid: the chlorid is by degrees decomposed, chlorhydric acid escapes, and the sulphate produced dissolves in the surrounding acid.

This is especially true for the chlorid of barium; but to succeed, the acid should be concentrated, and it is best to have the chlorid well dried and in powder. On adding water to the solution thus obtained, the sulphate of baryta falls as a white precipitate.

The chlorid of strontium gives the same results, and the acid solution affords a similar precipitate with water, though less in amount, since less is dissolved.

[7] On page 87, the date 19th of August should be 19th of October.

The sulphate of lime is still less soluble in sulphuric acid, and the solution takes several days to become limpid. Moreover, water does not then becloud it; a light precipitate is obtained with alcohol.

The solubility of these sulphates in sulphuric acid, is then the reverse of their solubility in water, except for the sulphate of strontium, which, in both cases, is a mean between those of the other two.

Nancy, Oct. 20, 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On a new saccharimeter.*—WILD has described a new saccharimeter which appears to possess great advantages over the well-known instrument of Soleil. In Wild's apparatus the incident light is polarized by means of a Foucault's calc-spar prism, which is capable of being turned round its axis, the amount of rotation being measured by means of a vernier applied to a small circle graduated to single degrees. The analyzer consists of a modification of Savart's polariscope, that is to say, of two crossed plates of quartz, each 20mm. in thickness, cut at an angle of 45° with the optical axis. The fringes produced are viewed with a small astronomical telescope of low power (objective 33mm. focal length, ocular 24mm. focal length). The telescope is adjusted for parallel rays, has cross wires, and a Nichol's prism in front of the eye piece. The tube containing the solution of sugar is of course placed between the polarizer and analyzer. The author instituted comparative experiments to test the degree of accuracy attainable with Soleil's instrument and with his own, employing for the purpose a solution containing 298 grams of cane sugar to the litre. The mean error of a single compensation in Soleil's instrument amounted to 1°, when the eye was properly protected, while it reached 1½° by direct observation in an ordinary room without exclusion of side light. From this it results that in the most favorable case there is with Soleil's instrument an uncertainty of ±13·9 gr. in a litre of the solution, or rather more than one per cent. With Wild's instrument the error of observation with white light amounted to 12′, which corresponds to an error of ±2·78 gr. of sugar in a litre of solution, and consequently to a degree of accuracy five times greater than with Soleil's apparatus. When this instrument was directed toward an Argand gas burner in a dark chamber, and a dark red glass was placed between the eye and the ocular, the error of observation amounted to only 3′. Finally, when a homogeneous soda flame was employed, the error amounted to only 1′. In the first case, the error in the quantity of sugar would amount to ±0·7 grs.; in the second, to only ±0·2 gr. in a litre.[1]—*Pogg. Ann.*, cxxii, 626.

W. G.

[*Note.*—Wild's Saccharimeter, independently of its purely technical

[1] Wild's Saccharimeter can be procured of Hoffman, optician, 3 Rue de Buci, Paris.

uses, will form a most acceptable addition to physical cabinets. To the chemist it offers the most precise method of determining the rotatory polarizing powers of liquids at present known, provided that homogeneous light be employed, since the characteristic lithium, sodium and thallium lines afford perfectly definite degrees of refrangibility. In Broch's measurements of the rotatory polarization in quartz by means of the fixed lines in the spectrum, the mean error of a single measurement varied from about 6′ to about 30′, according to the refrangibility of the light. In a series of measurements undertaken to determine the degree of accuracy with which the change of tint could be determined in plates of quartz placed between two Nichols' prisms and examined with white light, I found, as might be expected, that the error of observation increased as the intensity of the incident light diminished, the minimum error of estimation being about 15′. Hence it appears probable that the degree of accuracy attainable in Wild's instrument might be still farther increased by using a condenser so constructed as to give an intensely brilliant beam of parallel and homogeneous rays.—W. G.]

2. *On the mechanical energy of chemical action.*—Schröder van der Kolk has communicated an interesting and suggestive memoir upon the application of the theory of mechanical energy to chemical processes, especially in connection with the relations of chemical affinity to heat. We shall endeavor to give some idea of the train of reasoning in this paper, referring however to the original for more ample illustration. The author in the first place cites the opinion expressed by H. Sainte-Clare Deville, in his paper on dissociation, that all chemical compounds may be resolved into their elements by a sufficiently high temperature. In some cases the separated atoms reunite at a lower temperature; in other cases they remain separate. This gives us from this point of view two classes of bodies, and the author connects the phenomenon with the facts observed by Favre and Silbermann that certain combinations are effected with evolution, others with absorption of heat. The relation between the two classes of phenomena appears from the following considerations. If we conceive of a body at the temperature of 0°, and then apply heat, the body at a definite temperature will have absorbed a definite quantity of heat. This heat is applied partly in raising the temperature, partly in molecular changes, and partly in external work. The entire quantity of heat continually increases with the temperature, and heat is always taken up both in raising the temperature and in changing the state of aggregation from the solid to the liquid, and from this to the gaseous condition. To find the quantity of heat in the body after the process, we must subtract the quantity which is consumed in external work. The quantity remaining is termed by Thomson the mechanical energy of the body. Starting as we have done from 0°, we obtain in this manner only the accumulation of energy in the body from 0° to the particular temperature under consideration, and not its absolute quantity. Every body has therefore in any definite condition a definite quantity of energy. If now we consider two substances, as, for example, oxygen and hydrogen, which combine with each other under the influence of the electric spark, we have in each, before combination, a definite quantity of energy. During the combination, heat is evolved, and if we now cool the vapor of water

which is formed to the temperature of the components before combination, the steam will contain a quantity of energy less than that of its components by the quantity which is given out in the combination. We here suppose the combination of H and O to take place without external work. When the vapor of water is decomposed at the same temperature, just as much energy must be added as became free during the combination. Two cases may then occur: either the body has more energy than its components, or else the contrary is true. In the first case heat is given out in the decomposition, in the second, it is absorbed. It follows that heat must become free when we heat a body of the first class to the point of decomposition, and the components will not unite on cooling, since the quantity of energy in them is not sufficient for the formation of the compound at the same temperature. This could only take place in two cases: either the body must take up heat from the surrounding medium, which, however, has at most an equal temperature, or else the compound must become suddenly colder. So far as known, no such cooling occurs. From the above we obtain the following law: Bodies which evolve heat in being decomposed by heat are not again formed in the subsequent cooling. This law may be tested by the results of the experiments of Favre and Silbermann. These physicists found that protoxyd of nitrogen, NO, peroxyd of hydrogen, HO_2, chlorous and chloric acids, ClO_3 and ClO_5, evolve heat during decomposition, while the elements separated do not recombine on cooling. The same is true for the chlorid, iodid and sulphid of nitrogen, which are suddenly decomposed on heating, with evolution of heat, and the components of which do not recombine. The above law is also true when dimorphous or polymorphous bodies change their physical condition. Thus, aragonite on heating passes into calc-spar with evolution of heat; the converse change does not take place on cooling. Fused sulphur crystals (System V) on cooling become crystallized in System IV with evolution of heat; the converse change does not take place. Various other illustrations are given, and the author generalizes the law already stated as follows: When bodies on heating pass into a different physical state, with evolution of heat, the previous state is not resumed on cooling. It is of course possible that in certain cases the purely chemical affinity between the molecules may be so powerful that combination will ensue, heat being abstracted from the surrounding medium. The converse proposition is as follows:

If heat be absorbed in decomposing a body by means of heat, the contrary action will take place on subsequent cooling.

This proposition cannot be demonstrated theoretically, and is therefore not to be regarded as proved. It is frequently found by experiment to be true: thus, carbonate of lime, according to Favre and Silbermann, absorbs heat when decomposed by heating, and has therefore less energy than its components together. CO_2 and CaO recombine on cooling. On the other hand, much heat is evolved in slaking lime, and CaO, HO has therefore much less energy than its components. On heating, the hydrate is decomposed, and is again formed on cooling. In the formation of CO_2 and HO heat is evolved, and the compounds have therefore less energy than the components. The elements only unite at a high temperature, and the decomposition takes place at a still higher temperature.

On cooling, the components ought to recombine, but in point of fact this does not happen, though the components have much more energy than the products. The explanation of this depends upon the action of the purely chemical affinities, which at lower temperatures are not sufficient for the formation of the compound. Two conditions are in general necessary for the formation of a compound. First, a degree of chemical force or affinity sufficient for the combination, and secondly, the necessary energy. A single cause is insufficient. The author here distinguishes between reversible and unreversible processes. When a body changes under the action of heat, and with evolution of heat, it passes into a new condition of less energy, and therefore cannot in itself pass back into its original state. This is the unreversible process, and occurs most frequently. When heat, on the contrary, is absorbed, the new body has more energy, and may therefore on cooling pass of itself back into its previous condition. This is illustrated by the latent heat of fusion and evaporation. Other consequences of this theory are these; the heat of combination remains the same whether the compound be formed suddenly or by degrees; the heat of combination of a compound body is in general less than that of its components taken singly. The following examples serve to illustrate still further the application of mechanical energy to the explanation of chemical changes. An electric spark causes combination in two ways. Certain bodies, H and O, H and Cl, CO and O, &c., combine suddenly in unlimited quantities by means of a single spark, and with evolution of heat. Others, like N and O, combine only gradually; no heat is evolved, and the combination ceases with the cessation of the spark. The formation of ozone is also a case in point. In the first case, the energy of the components is greater than that of the compound. Sufficient energy is present, but not sufficient affinity. The electric spark here increases the affinity; some atoms of H and O combine, and the heat produced causes a further combination, since heat within certain limits increases affinity. In the case of the combination of N and O to form nitric acid, the components have less energy than the compound. Then the spark furnishes the requisite energy, and each spark yields only a definite amount of work. The case of NH_3 forms an exception which for the present must remain unexplained. The author also applies the theory to the catalytic action—so called—exercised by platinum upon H and O, but not upon N and O. In conclusion, the author takes ground against the assumption often made that the heat of combination is to be taken as the measure of chemical affinity. He considers that this heat, regarded as difference in energy, is the measure of the stability of the compound. Thus, water with the heat of combination expressed by 29413 has more stability than chlorhydric acid with a heat of combination of 23783 units. In each case, besides the requisite energy, the requisite affinity must be present. Thus, in a simple mixture of O and H no combination takes place until the affinities are sufficiently increased, although the necessary energy is always present. A further argument against considering the heat of combination as the measure of affinity is to be found in the fact that in certain combinations heat is absorbed, so that in these the chemical affinity would be negative.—*Pogg. Ann.*, cxxii, 439. W. G.

3. *On the replacement of hydrogen in ether by chlorine, ethyl and oxethyl.*—Lieben and Bauer have studied the action of monochlorinated and bichlorinated ether upon zinc-ethyl and ethylate of sodium, and have obtained results of much theoretical interest. By the action of chlorine upon ether, at ordinary temperatures, the author obtained a mono-chlorinated ether with the formula, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4Cl\end{matrix}\right\}O_2$. The action of zinc-ethyl upon this compound gives rise to two products which have respectively the formulas, $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.Cl\end{matrix}\right\}O_2$, and $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, and in which, as will be seen, one or two atoms of hydrogen in the radical ethyl are replaced by one or two atoms of ethyl itself. When mono-chlorinated ether is heated with an alcoholic solution of potash, or with ethylate of sodium, an oily liquid is formed, which boils at 157° C., is heavier than water, and has a most refreshing and agreeable odor. The formula of this body is, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$, so that it must be regarded as ether in which one atom of hydrogen in the ethyl is replaced by chlorine and one atom by the radical, $C_4H_5O_2$, which the authors term oxethyl. By the continued action of ethylate of sodium upon the last mentioned substituted ether, the authors obtained an oil lighter than water, boiling at 168° C., and having the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$. In this, two atoms of hydrogen are replaced by two of oxethyl. Finally, the action of ethylate of sodium upon this body, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, gives rise to a new liquid, boiling at 148° C. and presenting the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$. The new substances described are evidently the types of a large class of similar substituted bodies, and deserve in themselves an attentive study, especially with reference to physical properties and to products of decomposition.—*Comptes Rendus*, lix, 445. W. G.

4. *On a new mode of preparing oxygen.*—Robbins has given a method of preparing oxygen which is particularly interesting from a theoretical point of view, and which may hereafter be of much practical value. The process is based upon an experiment due to Schönbein, and consists in pouring dilute sulphuric acid upon a dry and powdered mixture of three equivalents of peroxyd of barium and one equivalent of bichromate of potash. Ozone and antozone are given off simultaneously, and unite to form common oxygen, which escapes with effervescence.—*Cosmos*, xxvi, 386. W. G.

II. GEOLOGY.

1. *On buried stems and branches in Illinois;* by J. S. Bliss. (From a communication to the Editors, dated Columbus, Wisconsin, Nov. 7.)—While passing through the county of Adams, in the State of Illinois, four years since, I learned of the existence of a well, which had been dug the year before, from which small branches of trees and twigs had been obtained. I repaired to the spot, and found the well to be twenty-five feet in depth; and was informed that, at a depth of twenty-three feet, the owner

came upon a layer of *fine black soil*, full two feet in thickness, in which were found branches an inch in diameter; I procured a shovel, and obtained several specimens three-eights of an inch through, in a tolerable state of preservation. Just above this bed of *soil* the material was clay.

In another well, three miles from this, the same kind of soil was met with, after passing through clay for twenty-seven feet from the surface. In the soil twigs were found; but on being brought to the air they crumbled to pieces, though the *bark* and *heart* were plainly to be seen.

Four miles from the first mentioned well, there was another, still more remarkable, which was 30 feet deep. At a depth of 27 feet, the diggers came square on a log, twelve inches in diameter; they cut it off on both sides of the well, and on taking it out pronounced it to be *Black walnut;* but I think that it was probably some other kind of timber, that had been dyed in consequence of having been submerged, and that this caused the mistake. In the same soil was also found a *Buckthorn* in a tolerable state of preservation. The top of the tree was lying southeast.

Another fact, I have noticed in Wisconsin, is that stones protruding through the surface, such as those known as *hard-heads*, present their perpendicular sides to the north, northeast, and northwest. This is not confined to any small portion of the State, but to quite an extended region.

2. *On the Geology of Eastern New York;* by Professor JAMES HALL and Sir WILLIAM E. LOGAN. (Read before the Natural History Society of Montreal, Oct. 24, 1864, by T. STERRY HUNT.)—Professor James Hall and Sir William Logan spent a few days together last summer in examining some points of the geology of Eastern New York, and propose to continue their examinations next season, when we may expect from them a detailed account of their results. Their principal object was to compare the rocks of that region with some of those of Eastern Canada; and I have now permission, in the absence of these gentlemen, to lay before this Society some of the results of this exploration.

The shales of the Hudson River group, which are seen for a considerable distance north and south of Albany, disappear a few miles east of the Hudson, and are succeeded by harder and coarser shales, sometimes red or green in color, and passing into green argillaceous sandstones. These various strata, which are associated with concretionary and shaly limestones, are now recognized as belonging to the Quebec group. The line of contact between this and the much more recent Hudson River group has nowhere been clearly seen in this region, but the two series are readily distinguished by their differences in color, texture, and hardness,—differences which were formerly supposed to depend upon the partial metamorphism of the eastern portion, when this was looked upon as a part of the Hudson River group. The green sandstones and conglomerates of Grafton Mountain, formerly looked upon as a portion of the Shawangunk conglomerate, are recognized as belonging to an outlying portion of the Sillery formation. This mountain Professor Hall had found in a previous exploration (1844–45) to have, at a point farther south, a synclinal structure, and it probably lies in three low synclinal axes. The Sillery formation scarcely extends south of Rensselaer County.

Canaan Mountain is also apparently synclinal, and, while limestones appear in the valleys on each side of it, consists chiefly of slates, the highest beds being a hard green sandstone, sometimes shaly, without any of the conglomerates of the Sillery, although boulders and angular fragments of these are found in the adjacent valleys. To the east of this, Richmond Mountain, in Massachusetts, presents in its upper portion a compact green slate, passing upward into a harder rock similar to that of the summit of Canaan Mountain. To the southward, as far as Hillsdale, the sparry limestones of the Quebec group appear in the valleys, while the hills are of slate. Proceeding thence westward toward the river, only the lower portions of the Quebec group are met with, until we come upon the rocks of the Hudson River group.

Washington Mountain is also of slate, flanked by limestone, all of the Quebec group, and is probably synclinal in structure. The valley to the south of the mountain exhibits limestones, apparently alternating with slates. Columbia and Duchess counties appear to be mainly occupied by the shales of the Quebec group, with broad exposures of its limestones, until we approach the river to the westward, when the shales of the Hudson River group are met with, extending a considerable distance below the city of Hudson. The gneiss of the Highlands occupies the southeast part of Duchess County.

From Fishkill the explorers proceeded to Coldspring, crossing what Mather called the Mattewan granite, but which they found to be an altered sandstone. Soon after this they came upon the great gneiss formation of the Highlands of the Hudson, which continues beyond Peekskill. They failed to find the sandstone described by Mather as coming out at this place; nor was anything representing the Potsdam sandstone detected in approaching the Highlands from Fishkill, nor elsewhere along their northern limits. Near to Peekskill, in the valley of the creek, was found a low ridge of black slate, supposed to belong to the Quebec group, and a similar slate was observed along the north side of the Highland range, not far from the gneiss. The gneiss of the Highlands presents all the aspects and characteristics of that of the Laurentian system, as seen in northern New York and in Canada.

Further examinations are necessary to determine the extension to the northeast of the Laurentian rocks of the Highlands, and also the succession of strata to the southeast of them. The recognition of the Sillery and of the Quebec group in this region are great and important facts of its geology, and not less so the identification with the Laurentian system of the gneissic district of the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs. This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestones of Orange County and of New Jersey underlie uncomformably the Lower Silurian strata.[1]—*Can. Nat. and Geol.*

[1] See Am. Jour. Sci., [2], xxxii, 208, and also Kitchell's 2nd Annual report on the Geology of New Jersey, (1855,) page 134 and onward.

3. *Action of Glaciers;* by J. RUSKIN.—What a river carries fast at the bottom of it, a glacier carries slowly at the top of it. This is the main distinction between their agencies. A piece of rock which, falling into a strong torrent, would be perhaps swept down half a mile in twenty minutes, delivering blows on the rocks at the bottom audible like distant heavy cannon,[1] and at last dashed into fragments, which in a little while will be rounded pebbles (having done rough damage to everything it has touched in its course)—this same rock, I say, falling on a glacier, lies on the top of it, and is thereon carried down, if at fullest speed, at the rate of three yards in a week, doing, usually, damage to nothing at all. That is the primal difference between the work of water and ice; these further differences, however, follow from this first one.

Though a glacier never rolls its moraine into pebbles, as a torrent does its shingle, it torments and teases the said moraine very sufficiently, and without intermission. It is always moving it on, and melting from under it, and one stone is always toppling, or tilting, or sliding over another, and one company of stones crashing over another, with staggering shift of heap behind. Now, leaving out of all account the pulverulent effect of original precipitation to glacier level from two or three thousand feet above, let the reader imagine a mass of sharp granite road-metal and paving-stones, mixed up with boulders of any size he can think of, and with wreck of softer rocks, (micacous schists in quantities, usually,) the whole—say half a quarter of a mile wide, and of variable thickness, from mere skin-deep mock-moraine on mounds of unsuspected ice—treacherous, shadow-begotten—to a railroad-embankment, *passenger*-embankment—one eternal collapse of unconditional ruin, rotten to its heart with frost and thaw, (in regions on the edge of each,) and withering sun, and waste of oozing ice; fancy all this heaved and shovelled, slowly, by a gang of a thousand Irish laborers, twenty miles down hill. You will conjecture there may be some dust developed on the way?—some at the hill-bottom? Yet thus you will have but a dim idea of the daily and final results of the movement of glacier moraines;—beautiful result in granite and slate dust, delivered by the torrent at last in banks of black and white slime, recovering itself, far away, into fruitful fields, and level floor for human life.

Now all this is utterly independent of any action whatsoever by the ice on its sustaining rocks. It *has* an action on these, indeed; but of this limited nature, as compared with that of water. A stone at the bottom of a stream, or deep sea-current, necessarily and always presses on the bottom with the weight of the column of water above it—plus the excess of its own weight above that of a bulk of water equal to its own; but a stone under a glacier may be hitched or suspended in the ice itself for long spaces, not touching bottom at all. When dropped at last, the weight of the ice may not come upon it for years, for that weight is only carried on certain spaces of the rock bed; and in those very spaces the utmost a stone can do is to press on the bottom with a force necessary to drive the given stone into ice of a given density (usually porous); and, with this maximum pressure, to move at the maximum rate of about a third of an inch in a quarter of a hour! Try to saw a piece of marble

[1] Even in lower Apennine. "Dat sonitum saxis, et torto vertice torrens."

through (with edge of iron, not of soppy ice, for saw, and with sharp flint sand for feldspar slime,) and move your saw at the rate of an inch in three-quarters of an hour, and see what lively and progressive work you will make of it!

I say "piece of marble;" but your permanent glacier-bottom is rarely so soft—for a glacier, though it acts slowly by friction, can act vigorously by dead weight on a soft rock, and (with fall previously provided for it) can clear masses of that out of its way, to some purpose. There is a notable instance of this in the rock of which your correspondent speaks, under the Glacier des Bois. * * * * *

[Mr. Ruskin continues with further explanation of his views, in the course of which he expresses the opinion that "the Glacier des Bois has not done more against some of the granite surfaces beneath it, for these four thousand years, than the drifts of desert sand have done on Mt. Sinai;" and "it never digs a hole" like the bed of a lake. He closes with a paragraph containing the following statement as to his early tastes and studies.]

I find it difficult to stop, for your correspondent, little as he thinks it, has put me on my own ground. I was *forced* to write upon Art by an accident (the public abuse of Turner) when I was two-and-twenty; but I had written a "Mineralogical Dictionary" as far as C, and invented a shorthand symbolism for crystalline forms, before I was fourteen: and have been at stony work ever since, as I could find time, silently, not caring to speak much till the chemists had given me more help.—*Reader, Nov.* 26, 1864, *the communication dated Denmark Hill, Nov.* 21.

4. *Geological Survey of California;* J. D. WHITNEY, State Geologist. —PALEONTOLOGY, Volume I: *Carboniferous and Jurassic Fossils*, by F. B. MEEK; *Triassic and Cretaceous Fossils*, by W. M. GABB. 244 pp. Small 4to, 1864. Published by order of the Legislature of California.—The principal results of the geological survey of California, up to the present time, have been briefly stated in our last volume at p. 256. Their number and importance are sufficient to show that the survey could not have been in better hands. Through the labors of Professor Whitney and his assistants, we are beginning to have some definite knowledge of the geology of the western border of the continent,—of the distribution of its rock-formations, of the age and structure of its mountains, of the origin of its metallic veins, of the action of rivers and glaciers in educing the features of the surface, besides many other points of geological and economical interest. The Report on the general Geology, as far as the subject is completed in the yet unfinished survey, will, we understand, be soon published. The volume just issued is the first of the two on the Paleontology of California. It treats of the fossils of the Carboniferous formation, the oldest fossiliferous beds yet observed, and of the Triassic, Jurassic and Cretaceous formations, leaving the Tertiary for the second volume.

The Carboniferous fossils make but a meagre list, the rocks of the age having small extent. Twelve or fourteen species are all that are described. Three of them are referred by Mr. Meek, though with some hesitation, to the widely spread *Lithostrotion mammillare, Productus semireticulatus* and *Spirifer lineatus.*

The Triassic species, described by Mr. Gabb, are 28 in number; the Jurassic, described by Mr. Meek, 12; the Cretaceous, described by Mr. Gabb, nearly 300, the larger part being new, and all pertaining, according to Mr. Gabb, to the Upper or White Chalk. Some of the general facts respecting the extent and character of the formations have already been published by us in the article in our last volume above referred to.

A volume of plates will soon be ready for delivery. It will contain figures of all the new species. The plates are lithographic, and thirty-two in number; and, as we know from an examination of many of them, are excellent in drawing and engraving.

The work is brought out in the first style of the art, both as to paper and printing. We know nothing superior, in these respects, from any American press.

5. *Oil region of Pennsylvania.*—[Extracts from a communication by IRA SAYLES, dated Meadville, Crawford Co., Pa., Nov. 13, 1864.]—As shown in the Geological Map of Pennsylvania by Professor Rogers, the surface-rocks of the higher country in the oil region of western Pennsylvania are mostly Carboniferous, while those of the valleys appear to be Devonian of the Chemung and Portage groups. In the borings, a whitish sandstone, called the *first sand-rock* is met with at a depth of 70 to 200 feet; and wells penetrating only through this bed generally yield a thick oil. 100 to 200 feet below this, lies the *second sand-rock*, of similar aspect to the first; and from beneath this there is usually another "show of oil," a heavy oil, though less so than the former. From this rock proceed nearly all the wells of the Alleghany river, while the wells of French Creek, with few exceptions, descend only to the *first* sand-rock. Below the *second*, 100 to 200 feet, there is a *third* sandrock, and from beneath this comes the pure, limpid, light oil. To this depth descend most of the "flowing" or fountain wells, four of which, when first opened, (the Empire well, one of them,) deluged the region around with 3000 barrels of oil per day. The wells of Oil Creek are mostly in this rock.

The valleys have been selected in nearly all cases for the borings under the impression that breaks in the hills indicate chasms in the rocks beneath. But there appears to be no reason why oil should not be reached also by borings through the higher land. There are evidently two kinds of sources; open cavities affording the free flowing wells, sometimes intermittent; and the oil-bearing shales giving a slower yield, as if by a gradual percolation from rocks that partially retain it; and the latter at least should be encountered as well through the hills as the valleys, if the boring is carried down to the same level. Moreover, there is no certainty that fissures or chasms in the rocks may not underlie the hills.

It is a little remarkable that the oil from beneath the upper sandstones should be the heavier, while that from beneath the lowest is the lightest. Although there is no evidence of any connection between the oil repositories at these three different levels, the sources of the same level often communicate. This has been abundantly proved, by a sudden diminution in the supply of an older well in consequence of sinking a new one near by.

The quantity of oil produced now does not materially differ from that of two years ago—viz: nearly 6000 barrels daily. The number of wells

has been increased in that time; but about as many old ones have been abandoned as new ones opened. Fewer of the new wells are great *spouting* wells than formerly. Pumping wells are very constant in their yield.

6. *Petroleum in California.*—A petroleum region on the coast of Santa Barbara, near Buenaventura, in Southern California, has recently been visited by Prof. Silliman, Jr., from whose Report we extract a few facts. Ten miles North of Buenaventura there is a mountain ridge, 2000 feet in height, extending for 13 miles from east to west, which consists in part of bituminous shales supposed to be of either the Cretaceous or Tertiary period. The dip of the layer is 70° to 80° to the north. From these shales, mineral oil comes out in many places, and at some points very abundantly. One of the wells is 30 feet in diameter, and is full of a tar-like oil, boiling with the escape of marsh-gas. There is also an area of asphaltum three-fourths of a mile long by half a mile wide, exuding tar and rock-oil at numerous points, besides several other oil springs, the places of discharge in all exceeding twenty. The range of bituminous shales extends to the south, occurring at intervals for a hundred and fifty miles, and also to the north as far as Glenroy, in Santa Clara County, or about 80 miles from San Francisco.—*Extract from Prof. Silliman's Report.*

III. BOTANY AND ZOOLOGY.

1. *Dioico-dimorphism in the Primrose Family.*—Mr. John Scott, late of the Edinburgh Botanic Garden, has communicated to the Linnæan Society an elaborate paper, entitled, *Observations on the Functions and Structure of the Reproductive Organs in the Primulaceæ*, which has recently been published in the 8th volume of that Society's Journal of Proceedings. Mr. Scott has followed up Mr. Darwin's well-known researches by many additional experiments, has confirmed his results, and brought out others of no small interest. The paper is long, and, on the whole, clear and satisfactory, but with occasional vagueness or obscurity of language, from the attempt of an acute, but perhaps rather untrained mind to indicate the bearings of the subject further than they have been clearly made out. A good summary appended to the paper sets forth the principal points.

Of 54 species or forms of Primula which he has examined (many of them only in the herbarium),—

"36 are truly dimorphic, presenting both long- and short-styled forms; 13 in which the long- or short-styled forms, respectively, have alone been observed by me; and 5 species and one variety with non-dimorphic characteristics, i. e., presenting stamens and pistils of an equal length.

"The allied genera *Hottonia* and *Aretia* have also truly dimorphic species; whereas other allied genera, *Dodecatheon*, *Soldanella*, and *Cortusa*, are very generally characterized by species presenting the structural characteristics of the long-styled form only, without, however, any decreased fertility arising from their hermaphrodite conjunctions.

"The general differences of the two sexual forms may be thus briefly summed up:—First, the long-styled forms have pistils equalling in length

the tube of the corolla; stigmas usually larger and rougher; stamens attached to, or frequently below, the middle of the corolla-tube, whose diameter is thus expanded upward; pollen-grains generally smaller and more transparent. Secondly, in the short-styled form the pistil is short, not rising above half way up the corolla-tube; stigma generally smoother and depressed on the summit; stamens attached to the mouth of the corolla-tube, causing an abrupt expansion; pollen-grains generally larger and more opaque. According to all the trials, these structural differences are accompanied by equally remarkable functional differences,—the pollen of the long stamens being alone adapted to fertilize the long pistils, and the pollen of the short stamens to fertilize the short pistils. By applying, on the other hand, either form pollen to own form stigma, i. e. effecting a homomorphic union, the degree of fertility relatively to the above, or heteromorphic union, is greatly decreased. Analogous, though less striking, functional differences, however, occur without any appreciable change of structure, as shown by the *P. verticillata*, e. g., yielding a much higher grade of fertility by its diœcious than its hermaphrodite conjunctions. Such an instance from a genus whose members are so generally characterized by a sexual dimorphism, naturally leads me to regard it as indicative of the acquirement of similar characteristics. An objection to this view may be urged from the occurrence of species which, having no immediate affinity with any structurally dimorphic species, nevertheless present individuals incapable of fertilization by own pollen, though perfectly susceptible to reciprocal fertilization, either with another individual of the same species, or one of a distinct species. To this category, at least, those who disbelieve in the genetic affinities of organic beings will no doubt refer the case of *P. verticillata*, and simply regard it as further illustrative of our ignorance of the conditions upon which sterility, in its varied grades, depends. Those, on the other hand, who believe in the existence of these genetic relations will look with an intelligent interest upon these functional peculiarities of the *P. verticillata*, and regard them, mayhap, as the primary indications of a tendency to assume those remarkable sexual characteristics of the correlated species, and thus presenting an illustration of incipient dimorphism.

"The usual differences in the fertility of the heteromorphic and homomorphic unions will be best appreciated by giving the mean results from the unions of several species. Thus, taking the five heteromorphic and homomorphic unions given above, namely, *P. Auricula*, *Sikkimensis*, *cortusoides*, *involucrata*, and *farinosa*, we see, from the mean results of their combined products, that for every 100 seeds yielded by the heteromorphic unions, only twenty-four are yielded by the homomorphic unions —the heteromorphic thus exceeding the homomorphic unions in about the proportion of five to three! I have also shown the remarkable fact that the pollen of a distinct species will produce a much higher grade of fertility than an ordinary homomorphic union, i. e., a flower's own pollen!

"It is well known that A will fertilize B, and B will not fertilize A. I have given instances of this law with Primulas. I have also shown the new and remarkable fact, that of the two forms of the same species the pollen of the one, but not of the other, will fertilize a distinct species!

For example, the long-styled *P. Palinurii* can be fertilized readily by pollen of the long-styled *P. Auricula;* yet, after numerous trials, I have failed to effect a single union between the long-styled form of the *P. Palinurii* and the short-styled *P. Auricula.* How utterly inconsistent, then, are such facts with the teachings of those who would have us believe that an absolute causal relation exists between the sterility from hybridism and systematic affinity! On the other hand, how unequivocally do these cases show us that the greater or less facility of one species to unite with another is, as Mr. Darwin has sagaciously argued, 'incidental on inappreciable differences in their reproductive systems. And that there is no more reason to think that species have been specially endowed with various degrees of sterility to prevent them crossing and blending in nature, than to think that trees have been specially endowed with various and somewhat analogous degrees of difficulty in being grafted together in order to prevent them becoming inarched in our forests.'[1]

"Probably the most remarkable result from my observations is that when the dimorphic species cease to be dimorphic, their reproductive functions are greatly modified. Thus, in the case of the Cowslip, for example, we have seen that an ordinary homomorphic union yields about fourteen seeds per capsule, the heteromorphic about twenty-four seeds per capsule, whereas the form with stamens and pistils of an equal length yields, when fertilized with its own pollen, thirty-four seeds per capsule! Thus the non-dimorphic form by own pollen exceeds, first, the homomorphic unions in the proportion of 5 to 2, and secondly, the heteromorphic in the proportion of 3 to 2! Again, from the four different unions of the long- and short-styled forms with the non-dimorphic form, the seed-results in each case fall considerably below an ordinary homomorphic union: thus the mean results of the unions of the non-dimorphic with long- and short-styled are six seeds per capsule, whereas the pure homomorphic unions of the latter give an average of thirteen seeds per capsule—that is, as two to one!

"Connected with these are the remarkable changes in the fertility of the [differently] colored varieties of the Primrose, the red variety yielding no seed when fertilized by pollen of either yellow or white varieties: the reciprocal crosses of these, i. e. the pollen of the red variety applied to the stigmas of the yellow and white, are also absolutely sterile! On the other hand, fertile unions may be effected by the reciprocal crossing of the yellow and white varieties, though in every case we have found that the average seed-result of such unions is considerably under that of the pure unions of these forms.

"Whether or not the ultimate tendency of dimorphism is a complete separation of the sexes, I think we have the clearest testimony that dimorphism has not always been a genealogical characteristic; and furthermore, that the two forms did not *per saltum* assume these structural and physiological characteristics. I here allude to the evidence afforded by the non-dimorphic Cowslip—namely, the resumption of perfect hermaphrodism, and the occasional production of intermediate stages between this and the normally dimorphic. These, taking us back in the

[1] Origin of Species, 3d ed., p 299.

genealogical line, show us an original non-dimorphic progenitor, and the graduated plan by which it gave rise to a dimorphically characterized race."

The most noteworthy points are:—1. That, in a genus which has many species with dimorphous flowers, there are some which present, so far as known, only one of the two forms (say the long-styled, which is perfectly fertile *per se*), as well as others with stamens and pistil of equal length. 2. That the Red Cowslip, a clear variety of the common one, has become non-dimorphic, and, with this change of structure, has become much more fertile of seed than the heteromorphic unions of the Common Cowslip. And the Auricula offers a similar case. But, on the other hand, a non-dimorphic variety of *Primula farinosa* is less fertile. These changes in the genetic system of a species seem very remarkable. 3. Still more remarkable is the result of fertilizing the Red Cowslip with its ancestor the common yellow form; this union, and also that of the Common Cowslip fertilized by the Red, being very sterile;—thus supplying that *desideratum* which has been called for as a test, "a physiological species produced through variation." 4. That the two forms of a dimorphous species hybridize with very different degrees of facility with distinct species. And that such a cross sometimes yields more seed than either homomorphic unions with own pollen. 5. Mr. Scott confirms (though it is not stated in his summary) Mr. Darwin's statement, that the long-styled forms artificially fertilized with their own pollen are about twice as fertile as the short-styled similarly fertilized with its own pollen; while, as is well known, when left untouched, the short-styled is most fertile, owing to the relative position of the anthers and stigmas. So that the advantage of position in the latter is counterbalanced by an increased differentiation of pollen and stigma with respect to their mutual action, Nature thus rendering the short-styled equally dependent with the long-styled on external agencies,—thus keeping it in existence, and more completely effecting the great object of dimorphism,—the intercrossing of individuals.

We may here append the remark that the Thymelæaceous genus *Leucosmia* is dimorphous, and some species of *Drymispermum* exhibit one if not both of the two forms. A. G.

2. *Observations upon Dimorphous Flowers;* by H. von Mohl; in *Bot. Zeitung*, Oct. 1863, translated in *Ann. Sci. Nat., Bot.*, April, 1864.—These observations are principally upon that case of dimorphism in which, besides the ordinary hermaphrodite (but often infertile) flowers, there are other and surely fertile ones, of simplified structure, apetalous, or rather cryptopetalous, and which, "their development being as it were arrested in the bud," and fertilized without opening, we had long ago termed "precociously fertilized." Some remarks were made upon them in this Journal, in November, 1862, (p. 419), stating that Nature, in this case, takes as much pains to secure self-fertilization as she does in the other case of dimorphism (represented by *Primula*, *Houstonia*, &c.,) to secure cross-fertilization. This is the conclusion which Mohl also reaches after a careful investigation of the common instances, viz: in *Specularia*, *Impatiens*, *Viola*, *Oxalis*, &c., clearly showing that such flowers, with a possible exception in violets, must needs be self-fertilized. The record of his

own observations is preceded by a good history of what was known upon the subject, from the beginning made by Dillenius down to the communication of Michalet in the *Bull. Soc. Bot. de France*, 1860. That the little tympanum which covers the minute anthers and stigmas of *Specularia perfoliata* until after fertilization, is a rudimentary corolla, however, was shown by Dr. Torrey, in a paper read before the Lyceum of Natural History, New York, in 1830, but not published.

One of the most interesting things to notice, and a curious confirmation of the intention to self-fertilize in the cases here considered, is the unusual paucity and the greater activity of the pollen in the anthers of the small and closed flowers. In those of *Oxalis Acetosella*, as Mohl informs us, the larger anthers contain only about two dozen, the smaller scarcely a dozen pollen-grains,—only a few times more numerous than the ovules they are to fertilize, while in the normal blossoms they are in countless excess,—the economy in number being in proportion to the sureness that they will do their work. Moreover, these pollen-grains never leave the anther, but, (as was observed by D. Müller and by Michalet, but misunderstood, and neatly shown by Mohl,) send out their tubes to seek the stigmas, adjacent, indeed, but often at a considerable relative distance, these tubes directing their course with the same precision, and in the same mysterious manner, apparently, that they ordinarily do when having reached the ovarian cavity they seek the orifice of the ovules. This is well seen in *Viola*, where, though it is barely possible that extraneous pollen might sometimes be introduced, yet, in view of the immediate contiguity of the stigma to the line of dehiscence of the two more fertile anthers, through which the pollen-tubes are emitted, close-fertilization here cannot be doubted. So Mohl concludes that, "considering that these little flowers are constantly fertile; that in some plants they are the only ones that are fertile, while in others they are far more plentiful than the large flowers, and that upon these the propagation of the plant mainly depends, it results that it is not a general law in hermaphrodite flowers that Nature should favor fecundation by the pollen of another flower in preference to its own pollen." Mohl proceeds to state that self-fertilization is equally secured in some non-dimorphic flowers, and instances *Fumariaceæ* as a case where this is "absolutely necessary, the transport of pollen from one flower to the stigma of another appearing to be absolutely impossible, on account of the special structure of the corolla, and of the intimate connection of the exterior [interior?] petals which enclose the anthers and stigma." We had thought the same, although the nectariferous sacs should have made us pause; but Mr. Darwin called our attention to the great facility with which the cap or united tips of the inner petals would be pushed off to either side by a bee visiting the blossom and seeking honey from its nectary. To this it sufficed, as we thought, to answer, that pollen was actually falling from the anthers upon the stigma they surrounded a full day before the tips of the outer petals opened, i. e., before there could be any access whatever from without. The reply to this seeming demonstration duly came in Mr. Darwin's capital discovery that, in *Linum* and other cases, the pollen may be less potent or even impotent upon its own stigma, while that from another flower is prepotent! Still these little precociously-fertilized flowers, of various and diverse fam-

ilies, stand as veritable exceptions to the universality of the law which Darwin has ably deduced.

What causes the pollen-grains to emit their tubes without contact with the stigma, and indeed at a considerable distance from it, is an utter mystery. Mohl remarks here an analogy with the case of *Asclepias*, and that in *Viola*, *Specularia*, &c., as Brown has stated for *Asclepias*, there is no liquid poured out by the stigma which might somehow place the pollen in relation with the stigma.

Finally, Mohl calls attention to an early remark of Linnæus, that, in the year 1753, a considerable number of plants raised in the Upsal garden, for which the Sweedish climate was too cool, flowered clandestinely, yet produced fruit. We may note that in the Cambridge Botanic Garden, *Oxybaphus nyctagineus* copiously fruits from the bud in cool and cloudy weather: so do one or two Pavonias for a considerable part of the season. And, several years ago, *Nyctaginia capitata* of Texas seeded abundantly all one summer without expanding a single flower, or even producing a full-grown flower bud. The next year it blossomed freely.

Mohl justly notes that these diminutive bud-fruiting flowers are often produced earlier in the season, as in *Specularia*, *Impatiens*, &c., but sometimes later than the normal blossoms, as in *Oxalis* and *Viola*. And he shows that the former conform (he looking upon such flowers as preponderantly female) to Knight's hypothesis, (drawn from cucumbers and melons,) that a high temperature favors the development of male flowers, while a lower temperature is more propitious to female blossoms, —a view which Thury took as the starting point of his interesting investigation upon the production of sexes in the animal kingdom. But Naudin has appended to the French translation of Mohl's paper, an important note, in which he declares that his sedulous observations upon *Cucurbitaceæ* for the past ten years are far from confirming Knight's theory; that in this family male and female blossoms appear independent of conditions of temperature, but intimately connected with peculiarities of temperament, which vary from species to species, from race to race, or even among individuals of the same race. And he notes especially (what we suppose applies generally) that the races of melons, squashes, and gourds which have long been cultivated in northern Europe are comparatively more precocious, and need much less heat for maturing their fruit, than those of the same species which have been recently brought from tropical regions. Also that the same *Cucurbitaceæ* arrive at adult age, i. e., to the age for flowering and fruiting, far sooner under the hot and cloudless sun of the south of France than under the cooler and obscurer skies of Paris, where they vegetate richly, but hardly blossom. Naudin remarks, also, that many monoicous *Cucurbitaceæ* tend to become dioicous, without assignable cause; and, as especially fatal to Knight's view, that *Coccinia diversifolia*, cultivated simultaneously at Paris and at Hyères, in a great number of individuals, has produced only male blossoms at Paris, but an abundance of both sexes (monoicous) at Hyères. A. G.

3. *Najas major*, *Ruppia maritima*, *&c.*, *discovered at Salina*, *N. Y.* —The September number of Dr. Seemann's *Journal of Botany* contains a *Revision of the Genus Najas*, by Professor Braun of Berlin. To the section *Eunajas* three species are assigned, viz., *N. major*, All., *N. muri-*

cata, Del., of Egypt, and *N. latifolia*, Braun, from the lake of Valencia, Venezuela. The section *Caulinia* has *N. flexilis*, which is found here and there in the North of Europe, and several varieties in the West Indies, Venezuela, &c., *N. arguta*, H.B.K., in New Granada, *N. minor*, All., in Europe, Egypt, and India, *N. fasciculata*, Braun, in India, and *N. graminea*, Del., in India, Egypt, &c. *N. major*, to which our attention is at present directed, under several varieties, is widely distributed throughout the Old World, in slightly brackish or fresh water, but it was also detected by Chamisso in the Sandwich Islands. What is equally remarkable, Prof. Braun has seen specimens collected in Florida by Cabanis.

Shortly before Prof. Braun's article came to hand, *Najas major* was discovered in Onondaga Lake, first by Judge G. W. Clinton, on the northern border of the lake (between Salina and Liverpool), and soon after by Mr. John A. Paine, Jr., on its western side, growing luxuriantly and abundantly, and quite fruitful. The marshy borders of the lake, as is well known, are brackish, at least in the vicinity of the salt-works, and abounding in maritime and submaritime plants. To the species formerly known there, the above gentlemen have added *Chenopodium glaucum*, *Leptochloa fascicularis*, and we believe some others. But a more interesting discovery, of a water-plant not before met with away from the sea coast, was recently made by Mr. Paine, he having detected *Ruppia maritima* in Onondaga Lake, along with *Najas major*, equally in a fruiting state.

A. G.

4. *Spontaneous return of hybrid plants to their parental forms.*—We have been prevented from preparing, as we wished to do, abstracts of the two rival essays upon Hybridity in Plants, viz., that of Mr. Naudin, of the Jardin des Plantes, and of Dr. Godron, of Nancy. The latter was published in full in the *Annales des Sciences Naturelles*, 4th series, volume xix, and also a part of the former, the whole memoir of Naudin, with details of the experiments and figures, having been accepted for publication in the *Mémoires* of the French Academy. The prize for which the essays competed were awarded to Naudin. A brief account of the two essays appeared a year ago in the Natural History Review. As respects one, the principal question, to use the summary of the Review, "the general result is, that Dr. Godron says: simple hybrids are absolutely sterile. Mr. Naudin says: hybrids are fertile whenever their anthers contain well organized pollen. Of 30 to 40 hybrids experimented upon, three-fourths were found to produce seeds capable of germination."

Upon another point of equal interest, Naudin's testimony is explicit, he maintaining that hybrid plants, however constant at first, tend in subsequent generations to a separation of the two specific elements, which are, as he expresses it, rather intermixed than truly combined, so that they would at length resolve themselves into the two parental types, or, by failure on one side, return to the one on the other. Naudin returns to this subject in a note in the *Flore des Serres* for July, 1864, where he gives the result of his experiments upon our common sorts of thorn-apple, *Datura Stramonium* and *D. Tatula*. These have more commonly been taken for varieties of one species; but their specific distinction has been maintained, especially of late, by various arguments. According to Naudin

they are truly distinct species, which do not sensibly vary. One always exhibits green stems and pure white flowers; the other, dark-purple stems and violet-tinged flowers. These two thorn-apples Mr. Naudin crossed, in 1855, and obtained a hundred or more hybrids, both *Tatula-Stramonium* and *Stramonio-Tatula*, both just alike, and exactly intermediate between the two species in the coloration of the stems and flowers. They had, however, the peculiarity of a gigantic size, attaining at least twice the size of their parents, and a tendency to sterility, which was manifested in the failure of all the flower-buds which were produced at the first forkings of the stems. The later flower-buds opened, however, and were perfectly fertile, the pods being as large and as full of good seeds as those of either parent. In 1861, these seeds (of *Stramonio-Tatula*) were sown, and produced a second generation like the first. Seeds of this crop were sown in 1862, and twenty-two seedlings were preserved for experiment. Five of these developed as *D. Stramonium* in all its purity, i. e., had green stems, white flowers, only the ordinary height, and all the flowers fertile, ripening their pods in the first forks, three months earlier than did their immediate parents. Nine individuals returned as completely to *D. Tatula* as the five did to *D. Stramonium*. Two others seemed to be *D. Tatula*, and were equally reduced in size and fertile from the first forks, but they still showed in their paler coloring a trace of the other ancestor. The remaining six of the twenty-two showed somewhat more of it, both in color and in the tallness and lateness of fructification; expressed numerically, Naudin estimates that they were, say, nine-tenths *D. Tatula* and one-tenth part *D. Stramonium*. "Here, then," says Naudin, "is a hybrid completely intermediate between the two parent species, which, left to itself, fecundated by its own proper pollen, is spontaneously dissevered at the second generation, dividing its offspring between the two species. And it is to be remarked that the division is very unequal, *D. Tatula* taking the lion's share, totally or nearly reclaiming seventeen individuals out of the twenty-two. This unequal division is common in hybrids, and sometimes goes to the extreme, one of the parents wholly disappearing from the hybrid progeny, which thus passes over entirely to the other species."

As to crossing species and their hybrids, in view of obtaining diverse and particular varieties, Naudin shows, accordingly, that, to obtain the desired results, the hybridizer should take pains to cross those mongrels which tend toward one parent with those which tend to the other, thus opposing the tendency to return to the specific types, which would otherwise surely take place. By doing this for a few generations, the two hereditary tendencies become, as it were, thoroughly confused, and a multitude of new variations break out in all directions, from which the amateur may select what he likes for preservation and further experiment. This is according to a principle first expounded by that excellent investigator, the late Louis Vilmorin. A. G.

5. *Flora of the British West Indian Islands;* by A. H. R. Grisebach, M.D.; Professor of Botany in the University of Göttingen. London, Lovell Reeve & Co. 8vo, parts VI and VII, 1864.—These parts, extending from p. 507 to p. 789, complete this most valuable work, which, as was intended, closes with the Vascular Cryptogamia. A full index of the

species is appended, and a list of colonial names. The preface gives an account of the circumstances under which the work was undertaken, and of the materials which the author has so sedulously and promptly elaborated. West Indian botany was very difficult and confused: "Almost all the principal authors who have written on West Indian plants belong to the last century, and consequently to the Linnæan school, and a general synopsis of West Indian plants has never before been attempted, not even by Swartz, whose Flora contains descriptions of his new species only, with a few remarks on allied forms." Moreover, the British West Indies offer only the separate fragments of a larger flora; and Trinidad, as its geographical situation indicates, naturally belongs to the flora of Venezuela and Guiana. The northern Bahamas might be supposed to have a vegetation very like that of East Florida, from which they are separated by the Gulf Stream; but this seems not to be the case. "Jamaica, again, from its mountainous character and more distant position, most of the leeward islands, from being wooded volcanos, and the majority of the windward ones, with a dry climate and a low calcareous soil, form three divisions of this tropical archipelago, which show as many peculiarities. Thus the whole of the British West Indies, as comprised in this flora, may be divided into five natural sections, each with a distinct botanical character." Altogether, they amount to about 15,900 English square miles, or nearly twice the area of Wales. But yet Hayti alone is nearly twice, and Cuba nearly thrice, as large as all the British Islands together, and not only far richer in vegetation, but far less explored, the publications of Jacquin, Swartz, &c., having been almost confined to the British possessions; so that it was with old species mainly, that Dr. Grisebach had to deal, those which were "the foundation, indeed, of our scientific knowledge of the flora of tropical America." And these "have so often been misunderstood that their synonyms are far more numerous than their numbers." A general West Indian Flora being out of the present question, we learn with interest that Dr. Grisebach is preparing a special paper on the geographical range of the West Indian plants, including the capital island of Cuba, which Mr. Charles Wright has so industriously and successfully explored through its length and breadth, and is expecting still further to explore.

We may here add the remark that Mr. Wright's third distribution of phænogamous plants and ferns—the fruits of his labors for the last three years—has just been made, consisting, in the fullest sets, of about 1800 species or numbers, (including some which have been redistributed under old numbers,), and that the greater part of them have already been determined by Dr. Grisebach, the now recognized authority in West Indian botany. An equally rich field probably remains in Hayti, to tempt an adventurous explorer. The Bahamas, also, bare as they seem to be, "have proved very interesting," and are thought to promise still many novelties.

The series of Natural Orders adopted in this work being different from the ordinary ones, and especially from that of the other colonial floras, mainly owing to the intercalation by Dr. Griesbach, of the apetalous or reduced forms among the polypetalous orders—where many of them clearly belong,—the author has added a tabular arrangement more in accordance with the Candollean series, and with that employed in the

new *Genera Plantarum* of Bentham and Hooker, which will be the natural companion of these colonial floras. These volumes are all written in English; and it is interesting to see how nearly at home the Göttingen Professor makes himself in a language foreign to him. A. G.

6. *Florula Australiensis: a Description of the Plants of the Australian Territory*, by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MÜLLER, M.D., F.R.S. & L.S., etc., vol. II, *Leguminosæ* to *Combretaceæ*. 1864. pp. 521, 8vo.—That the second volume of this important work has been brought out so soon after the first, may be attributed, not merely to the indomitable industry and force of the author, but also to the fact that four-fifths of its pages are devoted to the great order *Leguminosæ*, to which Mr. Bentham has previously devoted much attention. In fact, to him the reformation of the order from the state in which DeCandolle left it, is almost wholly due. He first indicated, we believe, the floral characters which distinguished the Cæsalpineous from the Papilionaceous suborder, settled the tribes of the latter upon their present basis, and, besides various special memoirs, revised the *Mimoseæ* several years ago in a complete monograph. The huge genus *Acacia* is most largely Australian, where, indeed, of all Phænogamous genera it is most numerous in species; and the Phyllodineous Acacias are almost exclusively Australian. The present flora describes 293 species of *Acacia*, of which 271 are of the Phyllodineous section. These are so neatly analyzed by a tabular key, of the sort which Mr. Bentham makes so much of,—that the crowd of specimens in the herbarium may be most readily determined and arranged. *Chrysolalanceæ* are here recombined with *Rosaceæ*. *Saxifrageæ* (we know not why the form *Saxifragaceæ* is refused, and *Saxifrageæ* therefore used as the name both of an order and of a tribe) are well made to follow immediately after *Rosaceæ;* then come *Crassulaceæ*, of course; and *Droseraceæ*, retained as a good order. The volume is smaller than its predecessor, closing with *Combretaceæ*, not to sever the *Myrtaceæ*, so largely represented in Australia. A. G.

7. *Upon a form of budding in some Insect Larves.*—In the last number of the *Zeitschrift für wissenschaftliche Zoologie* are two articles of interest,[1] upon a form of multiplication by budding occurring in some larves of dipterous insects, and also containing a description of a new genus and species of Cecidomyians founded on one of the insects in which it was observed. It seems that Nicolaus Wagner published, in the memoirs of the Kazan Institute for 1862, an account[2] of a larval multiplication by a sort of internal budding, and that C. E. v. Baer confirmed the discovery last year in the 6th vol. of the St. Petersburg Bulletin, supplementing it with the full history of the larve, of its development into a perfect insect, and of the deposit of a few large eggs thus fructified. In July of 1864, Dr. Meinert published in the Danish *Naturhistorisk Tidsskrift* an account

[1] Weitere Erläuterungen über die von Prof. Nic. Wagner beschriebene Insectenlarve, welche sich durch Sprossenbildung vermehrt, von Fr. Meinert. Aus dem Danischen mit Bemerkungen übersetzt von C. Th. v. Siebold.

Die ungeschlechtliche Vermehrung der Fliegenlarven von Prof. H. Alex. Pagenstecher. Zeits. f. Wiss. Zool. xiv: 4 Oct., 1864.

[2] This was noticed in the Zeitschrift f. Wiss. Zool., xiii, 513, but the writer of this has been so situated that it wholly escaped him, and he has not yet seen that notice.

of the same facts as observed by him, and also a description of the perfect insect as a new genus and species, with the name *Miastor metraloas*. The first of the two articles cited above, is a translation of a part of Dr. Meinert's paper, with some remarks by Dr. Siebold, the editor, which, he says, was just completed when he received the second article. This latter is a well illustrated and detailed account by Dr. Pagenstecher of a similar larval multiplication occurring in another species of dipterous insect.

It will be sufficient here, after calling attention to these articles, to give the principle fact confirmed in them. This is, that an insect larve has been seen to have egg-like bodies within the abdominal cavity; that these have been seen to develop into larves, which finally bursting out became free and resembled in every way the stock larve; that then, or perhaps after a second similar larve-brooding, the larves developed into a perfect dipterous insect which laid eggs and started anew the circle of species-life; and lastly, that this is seen in more than one species.

There are some points not altogether agreed upon by the observers above referred to. One is as to the place and the material at and from which these larves are formed in the larve-stock. Dr. Meinert describes them as formed from the granular fatty mass of the abdominal cavity, which he regards as a sort of remnant of the embryonic yolk division. Dr. Pagenstecher, on the contrary, says he saw none of these fat cellules in the way of developing into eggs, and thought these latter wholly different from any cells approaching their size, and sums up as follows: "In some unknown part of the body, cells are set free, which, without being specially fructified, assume the character of eggs. These develop into embryos that exist in a sort of pupa state within the mother-body, and develop at the expense of the parent structures. Finally they break forth, the larve-stock dying, and become independent themselves."—pp. 414–5. W. C. M.

IV. ASTRONOMY.

1. *Discovery of a new Planet, Terpsichore*, (81).—Mr. Tempel, at Marseilles, discovered on the 30th of September a planet equal in brightness to a star of the 10th magnitude. The following elements are given by Mr. Tietjen:

Elements of Terpsichore (81).

T	=	1864, Oct. 23·0, B. m. t.	i	=	8° 45′ 43″·0.
M	=	351° 31′ 52″·0,	φ	=	7 32 35 ·4.
π	=	23 40 12 ·1,	μ	=	765″·494.
☊	=	2 48 29 ·3,	log. a	=	0·444043.

2. *New Comet.*—Another Comet was discovered on the 9th of September by Mr. Donati, at Florence. It was very faint, and seen with difficulty. Mr. Celoria, of Milan, gives the following elements, computed from observations of Sept. 9th, 11th, and 13th:

T	=	1864, July, 27·8829 m. t. of Milan.
☊	=	175° 11′ 57″·4,
π	=	190 10 34 ·7,
i	=	44 56 53 ·6,
log q	=	9·787184, motion retrograde.

The comet, according to these elements, passed its perihelion before those heretofore known as Comet I, 1864, and Comet II, 1864.

3. *Numbers of stars in the northern hemisphere.*—In the last edition of his *Wunder des Himmels*, Prof. Littrow gives a summary of the numbers of stars that are in Argelander's charts of the northern hemisphere.

From N. declination,	0°	to 20°,	110,987 stars.
"	"	20° to 40°,	105,082 "
"	"	40° to 90°,	108,131 "

Classified according to magnitude there are,

Mag.	No. of Stars.	Mag.	No. of Stars.
1–1·9	10	6–6·9	4,328
2–2·9	37	7–7·9	13,593
3–3·9	128	8–8·9	57,960
4–4·9	310	9–9·	237,544
5–5·9	1,016		

There are, besides these, 60 nebulæ and 64 variable stars.—*Heis' Wochenschrift, Sept. 28th.*

4. *On the age of the moon's surface;* by J. Nasmyth.—The views I entertain on the subject in question are these, namely, that, as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface-structure ages before the earth had ceased from its original molten condition. And as the moon in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapor envelope, ere the earth had cooled down so as to permit the ocean taking up its final position *as an ocean*, this mighty vapor envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

Such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind the next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.—*Proceedings Manchester Lit. and Phil. Soc.*, Nov. 15, 1864.

5. *On the probable error of a Meridional Transit-observation, by the eye-and-ear, and Chronographic method;* by Edwin Dunkin, Esq.—At the close of an elaborate discussion of the observations made at the Royal Observatory at Greenwich, in 1853 and 1857, presented to the Royal Astronomical Society in May, 1864, Mr. Dunkin says:

In laying before the Society the preceding abstracts, which are the results of a great mass of computations, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

(1.) In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^s{\cdot}078$, while that of a *complete* transit over the seven wires is $\pm 0^s{\cdot}029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^s{\cdot}051$, and that of a *complete* transit over the nine wires is $\pm 0^s{\cdot}017$.

(2.) There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

(3.) In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

(4.) In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

(5.) The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*" transits, is $\pm 0^s{\cdot}048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0{\cdot}034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental. H. A. N.

5. *Shooting Stars in November*, 1864.—Arrangements were made at various places in the United States to watch for the shooting stars on the mornings of Nov. 13th and Nov. 14th. The sky was entirely overcast in most places, and nearly so in all. Through openings in the clouds, which would last a few minutes, occasional stars were seen, but whether there were more than usual it is impossible to determine. There was certainly nothing like the great display of 1833.

In San Francisco the sky was overcast. It is possible that some parts of California were favored with clear skies, and that we may yet obtain observations from them. Mr. Quetelet writes that a similar clouded sky prevented observation in Brussels. H. A. N.

6. *Supplementary note to the article on the Nebular Hypothesis.*—The form of Mars, as determined formerly, did not seem to agree with what we might expect from the primitive fluidity of the planet. The attention of astronomers having been recently called to the ellipticity of Mars, it seems now to be improbable that it has so large an ellipticity as has been heretofore received. (*This Journal*, [2], xxxviii, 436.) We thus see that this unconformable case is likely to yield to the requirements of the nebular hypothesis. We may venture to add that it is extremely improbable that Mars has an ellipticity differing much from that of the earth. D. T.

VI. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *National Academy of Sciences.*—The Report of the National Academy of Sciences, made by the Academy to the General Government, for the year 1863, has been published. It forms an octavo volume of 118 pages, with several maps. It contains only such Reports as had been made by committees of the Academy on the call of the General Government, with a brief statement of the operations of the Academy during the year 1863. The more important contents are a Report on the protection of bottoms of iron vessels from corrosion, etc.;—Report of the chairman of the Compass committee appointed to make experiments for the correction of local attraction in vessels built partly or wholly of iron;—Report of the committee appointed to examine the "Wind and Current Charts" and "Sailing Directions" issued from the Naval Observatory.

At the last meeting of the National Academy, held in August at New Haven, Ct., three vacancies in its list of members were filled by the election of Dr. J. C. Dalton, of New York, Leo Lesquereux, of Columbus, Ohio, and S. F. Baird, Assistant Secretary of the Smithsonian Institution.

2. *American Philosophical Society.*—The *Magellanic premium* of a gold medal has been awarded by the Philosophical Society to Mr. Pliny Earle Chase, for the discovery of numerical relations between gravity and magnetism.

3. *Medals of the Royal Society.*—At the Anniversary meeting of the Royal Society, on Wednesday, the last day of November, the Copley medal was awarded to Charles Darwin, Esq., F.R.S., "for his important researches in Geology, Zoology, and Botanical Physiology;" and the Rumford medal to J. Tyndall, F.R.S., for his researches on the Absorption and Radiation of Heat by Gases and Vapors.

4. H. R. Schoolcraft.—This distinguished traveller, and investigator of the manners, history, and language of the American Indian tribes, died at Washington on the 19th of December, in the seventy-second year of his age.

5. F. W. Struve, the celebrated astronomer of Pulkowa, Russia, died on the 23d of November last. His son, Otto Struve, was appointed his successor last year.

6. *Thoughts on the Influence of Ether in the Solar System, its relation to the Zodiacal Light, Comets, the Seasons, and periodical Shooting stars.* By Alexander Wilcocks, M.D. Extracted from the Transactions of the American Philosophical Society.—Dr. Wilcocks advances the hypothesis that there is a constant circulation of ether induced by the heat of the sun; that the cold ether descends near the poles of the sun, and that heated ether ascends from the sun's equator, or rather from a parallel of solar latitude just north of the equator; that this heated current as it rises is compressed by the cold ether into a thin conical sheet; that the ether bears with it matter capable of reflecting light, and thus causes the zodiacal light; that the tails of the comets are lighter than the ether, and arise for that reason from the sun, except when near the poles of the sun, when they are made to descend in the vortices of descending ether, so as to present double and multiple tails; that twice in the year, in August and November, the earth plunges through the sheet of warm

ether, causing a warm period in each month, the dog days and the Indian summer; and that in the middle of these two periods it causes the return of the August and November meteors.

Dr. Wilcocks has brought together and framed into a plausible hypothesis many facts, some of them hitherto not connected with any theory. But we do not believe that astronomers and physicists will be likely to accept his explanation. What he says relative to the seasons, and to the shooting stars seems peculiarly open to criticism. Why, for instance, the two periods of heat (supposing them to have an existence,) should not, according to his hypothesis, be equi-distant from the 5th of June and 5th of December, the two days when the earth is in the sun's equator, he does not explain. For the middle day of one period he names the 11th of August, which is 67 days from the 5th of June, while that of the other, which he calls the 12th of November, is only 24 days from the 5th of December. In what way the periodical star-showers could possibly be produced by such an ether we find it difficult to imagine. H. A. N.

7. *Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year* 1862. xx and 434 pp., 4to, with 49 diagrams and charts. Washington, 1864.—Besides the General Report on the Survey and its maps, there is the usual Appendix of Special Reports relating to various collateral physical observations. The subjects include—Tide tables for mariners; Cotidal lines for the Gulf of Mexico; Longitude of America from Europe, by Prof. Peirce; Lunar tables by the same; Longitudes in Maine, Alabama and Florida, by B. A. Gould; various series of Magnetic observations; Solar spots; Bessel's periodic functions; calculation of the depth of the Pacific from the earthquake wave of Simoda; Origin of the Florida Reef, by E. B. Hunt, etc.

8. *Eulogy on Joseph S. Hubbard;* by B. A. Gould. 44 pp., 12mo. —This eulogy was written in compliance with the call of the National Academy of Sciences, which in its constitution provides for such a tribute in commemoration of each member that may be removed by death. Prof. Gould had in his friend a congenial and inspiring theme. High mathematical powers combined with astronomical tastes, an energetic, investigating mind, and a frank and genial disposition tempered by a true Christian spirit, had accomplished much for science. The tribute is admirable as a biographical notice, appreciative, eulogistic but not beyond justice, and of special value as a chapter in the history of American astronomy.

9. *Chambers's Encyclopedia.*—A new volume—the Sixth—of this popular Encyclopedia, a work well filled out and illustrated in the departments of science, as well as in other varied branches of knowledge, has just been issued by the American publishers, J. B. Lippincott & Co., of Philadelphia. The volume (826 pp.) carries the alphabet nearly through N.

10. *Abstracts of Meteorological Observations made at the Magnetical Observatory, Toronto, Canada West, during the years* 1854 *to* 1859, *inclusive.* 136 pp. 4to. Toronto, 1864. *Results of Meteorological Observations made at the Magnetical Observatory, Toronto, during the years* 1860, 1861 *and* 1862. 84 pp. 4to. Toronto, 1864.

11. *Review of American Birds;* by S. F. Baird.—The publication and issue of this work has already reached the 148th page.

12. *Monograph of the Bats of North America;* by H. ALLEN, M.D.—Smithsonian Miscellaneous Collections, No. 165. Washington. 86 pp., 8vo, with numerous wood-cuts.—The species included in this important monograph are those of North America north of Mexico.

13. *American Journal of Conchology.*—A prospectus has just been issued by G. W. Tryon, Jr., of Philadelphia (625 Market st.), proposing the publication of a Quarterly Journal of Conchology, and soliciting subscriptions. Price ten dollars a year; single numbers three dollars. We commend it to all interested in the progress of science.

The Plurality of the Human Race; by G. POUCHET. Translated from the French by H. J. C. Beavan. London, 1864. Published for the Anthropological Society.

The Phenomena of Hybridity in the genus Homo, by Dr. PAUL BROCA. Published for the Anthropological Society.

Reptiles of British India; by ALBERT C. L. GUNTHER. Published by the Ray Society. 1864.

A History of the Spiders of Great Britain and Ireland; by JOHN BLACKWALL, F.L.S. Published by the Ray Society. 1864.

Metamorphoses of Man and the lower animals (in French); by A. DE QUATREFAGES. Paris, 1864.

Beschreibung und Eintheilung der Meteoriten; von GUSTAV ROSE. 162 pp., 4to, with 4 plates. Berlin, 1864.

Meteorologische Waarnemingen, of the Netherland Meteorological Institute, for 1863. 326 pp., 4to. Utrecht, 1864.

PROCEEDINGS OF THE AMERICAN ACADEMY OF ARTS AND SCIENCES, Vol. VI.—Page 97, On the Aluminates of Baryta; *W. P. Dexter.*—p. 106, Embryology of Asteracanthion berylinus, &c., with a plate; *A. Agassiz* [noticed at p. 130, vol. xxxviii, of this Jour.]—p. 114, On the velocity of light and the Sun's distance; *Lovering.*—p. 143, On the observed motions of the Companion of Sirius; *T. H. Safford.*—p. 166, Analysis of a Dacotah Meteorite; *C. T. Jackson.*—p. 169, On the new form of the Achromatic Object-glass introduced by Steinheil, with a plate; *Bond.*—p. 177, List of new Nebulæ seen at the Observatory of Harvard College; *Bond.*—p. 182, On Streptanthus and the plants referred to that Genus; *A. Gray.*—p. 188, Revision and arrangement (mainly by the fruit) of the North American species of Astragalus and Oxytropis; *A. Gray.*—p. 241, On the causes of Insanity among women; *Storer.*—p. 242, On the R. A. of the pole star as determined from observation; *T. H. Safford.*—p. 251, On a process of organic elementary analysis by combustion in a stream of oxygen gas; *C. M. Warren* [see last vol. of this Jour., p. 387].—p. 263, Observationes Lichenologicæ; *E. Tuckerman.*—p. 288, Investigation on the best approximate representation of all the mutual ratios of k quantities by those of simple integers; *Oliver.*

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA. No. 4, Sept. and Oct. 1864.—Page 181, Limits and relations of the Raniformes; *E. D. Cope.*—p. 183, Description of a Gar-pike supposed to be new; *A. Winchell.*—p. 186, New genera and species of N. A. Myriapoda; *H. C. Wood, Jr.*—p. 187, Note on the Paralepidoids and Microstomatoids, and on some peculiarities of Arctic Ichthyology; *T. Gill.*—p. 189, Synopsis of the Cyclopteroids of eastern N. A.; *id.*—p. 194 and p. 214, Synopsis of the Pleuronectoids of California and Northwestern America; *id.*—p. 199, on the affinities of several doubtful fishes; *id.*—p. 208, Note on the family of Stichæoids; *id.*—p. 211, Notes on shells, with descriptions of new fossil species and genera; *T. A. Conrad.*—p. 224, On the characters of the higher groups of Reptilia squamata, and especially of the Diploglossa; *E. D. Cope.*—p. 231, On a blind Silurid, from Pennsylvania; *E. D. Cope.*—p. 234, Fasti Ornithologiæ; Obituary of P. L. S. Müller; *J. Cassin.*

PROCEEDINGS OF THE ESSEX INSTITUTE, SALEM, MASS.—Vol. III, includes years 1860 to 1863. Vol. IV commences with the year 1864. No. 3 of Vol. IV, (July, August and September,) contains the conclusion of *J. A. Allen's* Catalogue of the Birds of Massachusetts, and a paper on the habits of some species of Humble Bees, by *F. W. Putnam.*

JOURNAL OF THE PORTLAND SOCIETY OF NATURAL HISTORY, Vol. I, No. 1. Portland, Maine.—This number is occupied by E. S. Morse's paper on the Terrestrial Pulmonifera of Maine, noticed in the last volume of this Journal (p. 303).

THE

AMERICAN

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[SECOND SERIES.]

ART. XVI.—*On Terrestrial Magnetism, as a mode of Motion;* by PLINY EARLE CHASE, M.A., S.P.A.S.[1]

IN a note to a former communication, I expressed my belief that the British Astronomer Royal would find in the mechanical action of the sun's rays, the precise "occasional currents" for which he was seeking, as the probable cause of magnetic storms. Mr. Airy has recently sent me a copy of his very interesting paper (*Trans. Roy. Soc.*, 1863, Art. XXIX), and its perusal has greatly strengthened this belief.

All of my meteorological views rest upon the hypothesis, that the atmospheric changes, whether of humidity, temperature, pressure, electricity, or magnetism, are purely mechanical; and that, being controlled by the laws of motions, their proper explanation does not require the assumption of any peculiar magnetic or electric fluid, but that a single homogeneous, elastic, and all-pervading æther may be both the source and the receptacle of all the various forms of force. In its principal features, this theory harmonizes with the now generally accepted belief in the mechanical origin of light and heat, but in its details it involves some new and interesting special applications, which I have endeavored partially to develop.

It will be readily seen, by a reference to my communication of April 15 (*Proc. A. P. S.*, ix, 367, *et seq.*), that the *mechanical* action of the currents to whose *electric* action Ampère ascribed the origin of terrestrial magnetism, produces two opposite spirals in the air and æther,—the lower moving from the

[1] From the Proceedings of the American Philosophical Society, Oct. 21, 1864.

poles to the equator, and against the earth's rotation; the upper from the equator to the poles, and in the same direction as the earth's rotation; the two being connected by innumerable currents of convection, or threads of ascending and descending particles. It will also be evident that at every place there are two principal sets of such double spirals, one with an axis perpendicular to the earth's radius vector, producing a maximum disturbance in the early afternoon, and the other more stable and uniform, with an axis passing through the nearest poles of greatest cold. In addition to the mutual perturbations of these two principal polarizing currents, the rolling of the luni-tidal attraction-wave produces at every instant a greater or less derangement,[3] and I find that the ratio of the lunar-barometric to the lunar-magnetic disturbance, (4·384), is nearly identical with Mr. Welsh's determination of the moment of magnetic inertia (4·4696; *Phil. Trans.*, cliii, 297). From a variety of considerations, it appears that the mechanical polarity or magnetic force thus engendered is a third proportional to two other forces, which may be called, respectively, central and tangential.

The communication which was presented at the meeting of Oct. 7, in its exhibition of the first numerical relationship that has even been pointed out between the barometric and magnetic fluctuations, showed that A : B : : B : M, a proportion in which A represents a central, B a tangential, and M a magnetic force.

I find a similar proportionality in each of Mr. Airy's summary tables (Op. cit., p. 627, *seq*). Thus, in his "Table II, Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical disturbance," the Mean Disturbance is

Westerly force.	Northerly force.	Nadir force.
− ·00023=M.	− ·00146=T.	− ·00057=C.

Here the proportion T : C : : C : M gives for M a

Theoretical value, ...	− ·000222
Observed "	− ·000228
Probable error, ...	·000080

"Table III. Algebraic Sums of Magnetic Fluctuations (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which Records were made by the three Instruments."

Theoretical value of M, ...	−·000287
Observed " "	−·000257
Probable error, . . .	·000068

[3] Besides the great disturbing agencies, whose effects may perhaps be determinable by mathematical prediction, every transient local accumulation of heat or cold will exert an influence. Everything that can produce currents or eddies in the atmosphere, may also be presumed to affect the æther, and the inconceivable rapidity of the æthereal motions, as manifested in the velocity of the waves of light and heat, will account for the extreme sensitiveness of the magnetic needle.

Tables V and VI exhibit an approximation to the proportion, C : T : : T : M, but the approximation does not come within the limits of probable error. As no attention is paid in these two Tables to the positive and negative signs, we could not reasonably expect so satisfactory results as in Tables II and III.

"Table VIII. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including all Days of Record of Great Magnetical Disturbance." The proportion C : T : : T : M, gives for M a

Theoretical value, ...	·001218
Observed "	·001203
Probable error, ...	·000066

"Table IX. Sums, without regard of sign, of Coefficients of Magnetic Irregularity (in terms of Horizontal Force) for each Year, from 1841 to 1857, including only those Days of Great Magnetic Disturbance, in which records were made by the three Instruments."

Theoretical value of M, ...	·001187
Observed " "	·001150
Probable error, ...	·000081

In addition to these numerical coincidences, the following points in Mr. Airy's paper appear to me to be specially noteworthy.

1. "The Aggregate for the Westerly Force . . . . (taken in comparison with that for the Northerly Force), appears to show that, on the whole, the direction for the Disturbing Force is 10° to the east of south;" p. 628. This indicates a line of mean disturbance about midway between the magnetic meridian (which at London, is about N. 24° W.), and the solar meridian, or midway between the meridians of decussation in the two sets of principal spirals, to which I have referred.

2. "Sometimes two waves in one direction correspond nearly with one in the other direction. . . . . A more frequent relation appears to be, that the evanescence of one wave corresponds with the maximum of the other;" p. 635.

3. "The most striking particulars in the last line (of Tables VIII and IX) are the following:

"First, the almost exact equality of the Mean Coefficients of Irregularity in the three elements. . . . . With reference to their physical import, I think it likely that the equality of Coefficients of Irregularity may hereafter prove to be one of the most important of the facts of observation.[3]

"Second, the near agreement in the number of Irregularities for Westerly and for Northerly Force.

[3] This approach to equality appears to be still more important, in view of the proportionality—C : T : : T : M—P. E. C.

"Third, the near agreement in the number of Irregularities for Nadir Force with half the number of Irregularities for Westerly or for Northerly Force;" pp. 641–2.

4. Tables X and XI (pp. 643–4) show that the disturbances are greatest in the winter months and in the night hours. Table X also appears to indicate minima of fluctuations and inequalities in months when there is the greatest uniformity of temperature, and maxima when the changes of temperature are greatest and most frequent.

5. Tables XI and XII furnish the materials for the following synopsis:

Forces.		Sums of Wave disturbance. +	Sums of Wave disturbance. −	Sums of Irregularities.	Mean Wave disturbances.	Average departure from mean ∓	Mean Irregularity ∓	Frequency of Storms.
Westerly force.	Time of max.,	20 *h.*	10 *h.*	10 *h.*	20 *h.*	13 *h.*	15 *h.*	9 *h.*
	Time of min.,	10 *h.*	21 *h.*	23 *h.*	10 *h.*	2 *h.*	1 *h.*	23 *h.*
	Amt. of max.,	·1170	·2491	·1976	+·00142	·00104	·00162	126
	Amt. of min.,	·0165	·0 . 3	·0346	−·00165	·00056	·00074	51
Northerly force.	Time of max.,	5 *h.*	12 *h.*	8 *h.*	5 *h.*	20 *h.*	15 *h.*	8 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	22 *h.*	1–2 *h.*	23 *h.*	23–1 *h.*
	Amt. of max.,	·1407	2·17	·1754	+·00038	·00168	·00144	186
	Amt. of min.,	·0038	·0674	·0441	−·00319	·00093	·00077	57
Nadir force.	Time of max.,	7 *h.*	14 *h.*	10 *h.*	0 *h.*	3 *h.*	0 *h.*	10 *h.*
	Time of min.,	22 *h.*	1 *h.*	23 *h.*	17 *h.*	22 *h.*	23 *h.*	1 *h.*
	Amt. of max.	·3976	3138	·1241	+·00570	·00363	·00180	86
	Amt. of min.	·0355	·0306	·0177	−·00380	·00157	·00074	19

"The Soli-tidal character of the principal characteristics of the occasional Magnetic Storms, as to frequency, magnitude, inequalities of wave disturbance, and Irregularities, is seen clearly in this Table." (Table XII) p. 645. There are subordinate maxima and minima, the consideration of which will become interesting, when the laws of the principals have been well ascertained and defined.

6. "In regard to the Wave-disturbance: for Westerly Force, the aggregate is + from 17^h to 6^h, − from 7^h to 16^h; for Northerly Force, the aggregate is + from 3^h to 5^h, − from 6^h to 2^h; and for Nadir Force, the aggregate is + from 23^h to 10^h, −from 11^h to 22^h;" p. 644.

7. Mr. Airy presents some conclusive considerations, "showing that the observed disturbances cannot be produced by the forces of any suddenly created galvanic current or polar magnet," and remarks as follows, respecting his theory: "Its fundamental idea is, that there may be in proximity to the earth something which (to avoid unnecessary words) I shall call a Magnetic Ether; that under circumstances generally, but not always, having reference to the solar hour, and therefore, probably, depending on the sun's radiation or on its suppression, a

current from N.N.W. to S.S.E., approximately, or from S.S.E. to N.N.W. (according to the boreal or austral nature of the ether), is formed in this Ether; that this current is liable to interruptions or perversions of the same kind as those which we are able to observe in currents of air and water; and that their effect is generally similar, producing eddies and whirls, of violence sometimes far exceeding that of the general current from which they are derived;" p. 646.

8. "And in the relation between E. and W. disturbances and vertical disturbances, there is a point which well deserves attention. When a water-funnel passed nearly over the observer, travelling (suppose) in a N. direction, he would first experience a strong current to the E., afterward a strong current to the W. (or *vice versa*), and between these there would be a very strong vertical pressure in one direction, not accompanied by one in the opposite direction; thus he would have half as many vertical as horizontal impulses. This state of things corresponds to the proportion which we have found throughout for the magnetic disturbances, and to the relation found in Article 18. I may also add that the rule at which we have arrived, that the waves of vertical force are few, but that their power, when they do occur, is very great, seems to correspond with what is reported of the whirlwinds of great atmospheric storms; which, violent and even frequent as they may be, occur very rarely at any assigned place;" p. 647.

I add a few considerations from Maj. Gen. Sabine's discussions. (*Phil. Trans.*, cliii, Art. XII.)

9. "The westerly deflections at Kew . . . have a decided double maximum, with an intervening interval of about eight or nine hours.

. . . The conical form and single maximum which characterizes the *easterly* deflections at Kew, belong also to the easterly deflections in all localities in North America, where the laws of the disturbances have been investigated. But . . . at Nertschinsk and Pekin . . . the conical form and single maximum characterize the *westerly* deflections, whilst the easterly have the double maximum. . . . At the two Asiatic stations, the aggregate values of the *westerly* deflections decidedly predominate, whilst in America the *easterly* deflections are no less decidedly predominant; and at Kew, . . . the amount of deflection in the two directions may be said to be balanced;" p. 282.

10. The differences of the weekly from the annual means of declination, indicate "with a very high degree of probability, an *annual variation*, whereby the north end of the magnet points more toward the east when the sun is north, and toward the west when the sun is south, of the equator;" p. 291.

11. The residual errors in the monthly determinations of the Horizontal Force and of the Dip "are thoroughly confirmatory of a semi-annual inequality, having its epochs coincident, or nearly so, with the sun's passage of the equator;" p. 303.

12. There appears to be "an increase of the Dip and of the Total Force, and a deflection of the north end of the Declination magnet toward the West, in both hemispheres, in the months of October to March, as compared with those from April to September. . . . The greater proximity of the earth to the sun in the December compared with the June Solstice most naturally presents itself as a not improbable cause; but we are as yet too little acquainted with the mode of the sun's action on the magnetism of the earth, to enter more deeply into the question at present;" p. 307.

I have neither the leisure nor the ability to undertake an exhaustive analysis of the results thus brought together; but I present them as well worthy of a profound mathematical investigation, as confirmatory in very striking and minute particulars of my mechanical hypotheses, and as furnishing new and strong presumptive evidence of that marvellous simplicity of force to which many independent branches of modern physical research so strongly point. This evidence is strengthened by the existence, as I have shown elsewhere, of the tidal law of sines in the solar-diurnal variation of the magnetic needle, by the magnetic effect of the daily barometric rotation tide, as exhibited in the convergence of lines of equal barometric disturbance toward the hours of high barometer and their divergence from the hours of low barometer, by many points of resemblance between the daily and yearly variation curves, both of temperature and of magnetism, and by certain considerations confirmatory of the reasonable presumption that there may be lunar monthly magnetic tides, somewhat analogous to those which I have pointed out in the barometer. (*Proceedings of the Roy. Soc.*, June 16, and of the *Am. Phil. Soc.*, June 17, 1864.)

Besides the differential or tidal action of the moon, there is a slight tendency to diminish the weight of the air that is nearest the moon, and to increase the weight of that which is most remote. In proportion as this tendency is exerted in conjunction with, or in opposition to, that of the sun, the mean solar-diurnal magnetic currents should be increased or diminished. Slight as the disturbing influence is, and modified as it must be by various causes, both occasional and periodic (*e. g.* the earth's rotation, the cyclical revolution and consequent varying latitude of the moon at the commencement of each new month, the oscillations in the aerial rotation-spheroid produced by lunar attraction, the changes in the average temperature of day and night at different seasons and different years, &c.), it may yet, perhaps, be dis-

cernible in comparing the results of a long series of careful and delicate observations. The accompanying tables are deduced from such a comparison of the St. Helena records.

TABLE I.

Solar and Lunar Daily Magnetic Tides, in parts of Force.[4]

Solar and Lunar Hours.	Horizontal force.		Vertical force.		Total force.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·00	·000	·000	·000	·000	·000
0	+1099	+006	−022	−005	+95	+005
1	+0911	−003	+229	+027	+82	−001
2	+0623	−011	+446	+031	+60	−005
3	+0368	−014	+593	+044	+40	−006
4	+0133	−020	+638	+072	+20	−007
5	−0080	−014	+608	+041	+01	−006
6	−0270	−007	+611	+050	−15	+001
7	−0394	−004	+545	+028	−26	+001
8	−0465	000	+300	−012	−36	−002
9	−0511	+022	+219	−011	−41	+018
10	−0530	+032	+074	−017	−45	+025
11	−0522	+031	−011	−037	−45	+022
12	−0481	+019	−100	−003	−43	+016
13	−0449	+017	−165	+005	−41	+015
14	−0405	+013	−224	+019	−38	+014
15	−0376	−009	−289	+048	−36	−001
16	−0352	−011	−345	+051	−35	−002
17	−0329	−008	−398	+029	−34	−003
18	−0298	−009	−465	+013	−32	−006
19	−0154	−004	−513	−011	−20	−005
20	+0130	+003	−582	−053	+03	−005
21	+0470	+006	−491	−050	+34	−002
22	+0803	+013	−427	−054	+63	+004
23	+1019	+009	−214	−067	+85	−001

TABLE II.

Lunar-Monthly Magnetic Tide of Horizontal Force.

Moon's Position.	Mean Daily Fluctuations of Horizontal Force at St. Helena.[5]				Moon's Position.	Mean Daily Fluctuations of Horizontal Force at St. Helena			
°	1844.	1845.	1846.	1844-6. Average.	°	1844.	1845.	1846.	1844-6. Average.
0	61·31	53·75	40·88	51·98	180	58·81	53 91	40 86	51·19
15	61·66	53·29	41·24	52·06	195	59·39	53·64	40 27	51·10
30	62·20	52·85	41·73	52·26	210	59·35	53·57	40·70	51·20
45	62·32	53·61	41·81	52·58	225	58·88	52 91	40·74	50·85
60	62 56	52·40	40·66	51·87	240	59·66	52·80	39·79	50·75
75	62·76	52·36	41·34	52·15	255	60 35	52·92	40·70	51·32
90	61·82	52·80	41·54	52·05	270	60·50	53·43	40 65	51·51
105	61·37	53·11	40·42	51·63	285	60 89	53·81	41·10	51·94
120	60·47	53·43	39·97	51·29	300	61·64	52 82	41·41	51·96
135	59·42	53·60	40 46	51·16	315	61·80	53·10	41·34	52·08
150	60·23	53·01	40·14	51·13	330	62·14	53·45	42 39	52·66
165	60·54	53·46	39·72	51·24	345	62·16	52 97	41·85	52·32

[4] The first decimal figures are placed, for convenience, in an upper line.

[5] The value of one scale division is ·00019 of the horizontal force, in 1844 and 1845, and ·00021 in 1846.

Table I is compiled from Maj. Gen. Sabine's Tables 36, 37, 50, 51, 52, 53 (*St. Helena Observations*, ii). It is especially interesting as showing the influence of the opposition of attraction to rotation in producing low solar tides at 10 or 11 P. M., the prompt and direct influence of the sun upon the æthereal currents in the production of a high tide at noon, the double maxima and minima in each of the lunar tides, the additional confirmation of the analogies that I have heretofore pointed out between the spheroids of attraction and rotation, the opposition of the solar and the resemblance of the lunar zenith and nadir effects, and the evidence in the partial "establishment" of the moon's tides that most of her magnetic influence is exerted indirectly on the æther, through the intervention of atmospheric attraction-currents.

Tables II and III were formed by taking the mean of the hourly averages, on the twenty-four days in each lunar month which are most nearly indicated by the angular positions given in the first column. Each of the tabular numbers for 1844 and 1845 represents the average of two hundred and eighty-eight hourly observations; each of the numbers for 1846, the average of two hundred and sixty-four observations, with the few exceptions of holidays and other omitted days, for which the missing numbers were interpolated. Table II indicates a tendency to mean lunar influence between 90° and 105°, and between 270° and 285°, the influence increasing when the moon acts either in conjunction with the sun, or directly upon con-

TABLE III.

Lunar-Monthly Magnetic Tide of Vertical Force.

Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.[6]				Moon's Position.	Mean Daily Fluctuations of Vertical Force at St. Helena.			
°	1844.	1845.	1846.	1844-6 Average.	°	1844.	1845.	1846.	1844-6. Average.
0	48·42	48·51	43·56	46·83	180	47·06	47·77	46·54	47·12
15	48·21	48 55	43 90	46·89	195	47 96	48·02	46·29	47·42
30	47 33	48 53	44·55	46·80	210	48 14	48·26	45·88	47·43
45	47·33	48·25	43·96	46·51	225	47·49	48·26	46 39	47·38
60	47 45	48·47	43·69	46·54	240	48·40	48·60	45·32	47·44
75	47·62	47·88	41·22	46·57	255	48 16	48·52	44 64	47·11
90	47·65	47 43	44 77	46·62	270	48·00	48 08	44 54	46·87
105	47 62	46·92	45·31	46·62	285	47 93	47·70	44·61	46·75
120	47·53	47·42	45 65	46·87	300	48 06	48·26	44·91	47·08
135	47·76	47 40	47·30	47·49	315	48·17	48·56	44 95	47·23
150	47·55	47 50	47·23	47·43	330	48·49	47·91	43·78	46·73
165	47·52	47·70	47·02	47·41	345	48·18	47·44	43 99	46·54

densed air and *vice versâ*. It also shows the existence of disturbances, which may be accounted for by some of the causes to which I have already referred. Table III exhibits apparent tendencies to diminution of force near the syzygies, and to increase of force a day or two after the quadratures.

[6] The value of one scale division varies from ·00051 to ·00091 of the vertical force.

Table IV is a compendium of the tidal differences in the two preceding tables. It shows the effect of temperature in producing maxima and minima when the coolest and warmest portions of the earth are submitted to the direct action of the moon (at or near 240° and 45°), low temperature producing a minimum of horizontal force, with a maximum of vertical force, and *vice versâ*. From the variations of horizontal force $\left(\frac{\Delta X}{X}\right)$ and vertical force $\left(\frac{\Delta Y}{Y}\right)$ given in this table, table V is formed, the mean variations of total force $\left(\frac{\Delta \varphi}{\varphi}\right)$ being

TABLE IV.

Lunar-Monthly Magnetic Tide. Differences from Monthly Means.

Moon's Position. ☽	Horizontal Force.			Vertical Force.			Means.	
	1844.	1845.	1846.	1844.	1845.	1846.	H. F.	V. F.
0	+ ·38	+·54	− ·02	+·59	+ ·51	−1·56	+·30	−·16
15	+ ·73	+·08	+ ·34	+·38	+ ·55	−1·22	+·38	−·10
30	+1·27	−·36	+ ·83	−·50	+ ·53	− ·57	+·58	−·19
45	+1·39	+·40	+ ·91	−·50	+ ·25	−1·16	+·90	−·48
60	+1·63	−·81	− ·24	−·38	+ ·47	−1·43	+·19	−·45
75	+1·83	−·85	+ ·44	−·21	− ·12	− ·90	+·47	−·42
90	+ ·89	−·41	+ ·64	−·18	− ·57	− ·35	+·37	−·37
105	+ ·44	−·10	− ·48	−·21	−1·08	+ ·19	−·05	−·37
120	− ·46	+·22	− ·93	−·30	− ·58	+ ·53	−·39	−·12
135	−1·51	+·39	− ·44	−·07	− ·60	+2·18	−·52	+·50
150	− ·70	−·20	− ·76	−·28	− ·50	+2·11	−·55	+·44
165	− ·39	+·25	−1·18	−·31	− ·30	+1·90	− ·44	+·42
180	−2·12	+·70	− ·04	−·77	− ·23	+1·42	−·49	+·13
195	−1·54	+·43	− ·63	+·18	+ ·02	+1·17	−·58	+·43
210	−1·58	+·36	− ·20	+·31	+ ·26	+ ·76	−·48	+·44
225	−2·05	−·30	− ·16	− ·34	+ ·26	+1·27	−·83	+·39
240	−1·27	−·41	−1·11	+·57	+ ·60	+ ·20	−·93	+·45
255	− ·58	−·29	− ·20	+·33	+ ·52	− ·48	−·36	+·12
270	− ·43	+·22	− ·25	+·17	+ ·08	− ·58	−·17	−·12
285	− ·04	+·60	+ ·20	+·10	− ·30	− ·51	+·26	−·24
300	+ ·71	−·39	+ ·51	+·23	+ ·26	− ·21	+·28	+·09
315	+ ·87	−·11	+ ·44	+·34	+ ·56	− ·17	+·40	+·24
330	+1·21	+·24	+1·49	+·66	− ·09	−1·34	+·98	−·26
345	+1·23	−·24	+ ·95	+·35	− ·56	−1·13	+·64	−·45

obtained by the formula $\frac{\Delta \varphi}{\varphi} = \cos^2 \theta \frac{\Delta X}{X} + \sin^2 \theta \frac{\Delta Y}{Y}$. I have taken $\theta = -22°$; one scale division of horizontal force = ·000194; one division of vertical force = ·000792; which are almost identical with the values employed by Gen. Sabine in the computation of his tables of hourly variation in solar and lunar total force.

In a similar manner I have computed Table VI, showing the average hourly variations, both in solar and lunar total force, in each of the three years which have furnished the data for most of my deductions. The first decimal figures are placed in an

upper line, as in Table I. Perhaps the principal utility of this table may be found in some future extension of these investigations, but even now it is interesting, inasmuch as it exhibits the probable influence of periodic causes in shifting the hours of the daily maxima and minima, and as it lends added weight to the preceding tables, by showing that the monthly tide is more regular than the daily tide.

TABLE V.

Lunar-Monthly Magnetic Tide of Total Force.

Moon's Position.	Mean Daily Fluctuations at St. Helena, in parts of Total Force.			
☽	1844.	1845.	1846.	1841-46. Average.
0	+·00013	+·00015	−·00018	+·00003
15	+·00017	+·00007	−·00008	+·00005
30	+·00016	·00000	+·00007	+·00008
45	+·00018	+·00009	+·00002	+·00010
60	+·00023	−·00008	−·00026	−·00002
75	+·00028	−·00015	−·00008	+·00003
90	+·00013	−·00013	+·00007	+·00002
105	+·00005	−·00014	−·00006	−·00005
120	−·00011	−·00003	−·00010	−·00008
135	−·00026	·00000	+·00017	−·00003
150	−·00015	−·00009	+·00011	−·00004
165	−·00010	+·00001	+·00001	−·00003
180	−·00044	+·00009	+·00015	−·00007
195	−·00024	+·00007	+·00003	−·00005
210	−·00023	+·00009	+·00005	−·00003
225	−·00038	−·00002	+·00012	−·00009
240	−·00015	·00000	−·00016	−·00010
255	−·00006	+·00001	−·00009	−·00005
270	−·00005	+·00005	−·00011	−·00004
285	·00000	+·00007	−·00002	+·00002
300	+·00014	−·00004	+·00006	+·00006
315	+·00018	+·00004	+·00005	+·00009
330	+·00027	+·00003	+·00010	+·00013
345	+·00024	−·00010	+·00003	+·00006

It seems not improbable that the mutual planetary perturbations which are sufficiently powerful to affect their orbital revolution, may also exert an appreciable influence on their æthereal spheroids, and that numerous cyclical magnetic variations may be thus produced. The disturbance of Jupiter is by far more important than that of any other planet, its mean attractive energy being nearly a third proportional to those of the sun and moon.[7] The annual fluctuations are very great, the intensity being about $\frac{1}{1\,1\,4}$ when Jupiter is nearest the earth, and less than half as great, or only about $\frac{1}{2\,4\,7}$, when most remote. The combined operation of the tropical revolutions of Jupiter, the moon's apsides, and the moon's nodes, should produce a series of disturbances corresponding very nearly in duration with General

[7] If we take as our unit the moon's attraction for the earth $\frac{M}{D^2}$, the sun's will be about 177, and Jupiter's $\frac{1}{1\,1\,4}$.

Sabine's magnetic "decennial period," and Schwabe's period of solar spots.

The law of varying attraction suggests a plausible explanation for the approximate mean proportionality of the barometric to the tidal and magnetic variations. For the ratios of attraction of any planet when in solar conjunction, at quadrature, and in opposition, vary as $(n+1)^2$, n^2, and $(n-1)^2$, respectively, the attraction at the mean distance being nearly a mean proportional between the maximum and minimum attractions. The barometrical fluctuations are occasioned by variations in the gravitation

TABLE VI.

Solar and Lunar-Daily Tides of Total Force.

Solar and Lunar Hours.	1844.		1845.		1846.	
	Solar.	Lunar.	Solar.	Lunar.	Solar.	Lunar.
	·000	·000	·000	·000	·00	·000
0	+83	+009	+88	-009	+066	+004
1	+83	+011	+97	-014	+104	+015
2	+67	-005	+85	-016	+097	-004
3	+49	-004	+63	-008	+076	+018
4	+28	-024	+41	-012	+054	+017
5	+15	-032	+20	-001	+031	+024
6	+02	-027	+02	+002	+008	+028
7	-11	-016	-11	+010	-014	+022
8	-22	-006	-24	+003	-026	+022
9	-31	+001	-35	+034	-036	+022
10	-35	+008	-39	+041	-043	+024
11	-37	+029	-46	+031	-047	+021
12	-37	+017	-48	+021	-053	+008
13	-38	+021	-43	+012	-048	+011
14	-39	+045	-44	+012	-040	-001
15	-32	+021	-42	+006	-038	-029
16	-30	+001	-39	+006	-043	-033
17	-29	-004	-39	+006	-041	-027
18	-22	-012	-37	-008	-042	-020
19	-24	-006	-36	-016	-041	-019
20	-16	-014	-24	-008	-027	-011
21	+07	+017	+03	-006	-008	-021
22	+38	+022	+32	-002	+029	-008
23	+64	+016	+63	-008	+063	-010

of the air toward the earth's center,—the tidal motions, by the influence of distant heavenly bodies,—and the magnetic, according to my hypothesis, by the oscillations of the air and æther in their efforts to restore the unsettled equilibrium. The three disturbances, therefore, must evidently have nearly the same mutual relations as if they were produced by three forces, one centripetal, and the other two centrifugal, the two latter being nearly equal in amount but diametrically opposed in direction. This leads us at once, theoretically, to the general formula with which we started empirically,

$$A : B :: B : M,$$

and strengthens the conviction that there are none of the phe-

nomena of terrestrial magnetism which cannot be explained, either by the instantaneously received and instantaneously transmitted impressions which are made directly upon the æther by attraction, heat, or rotation,—by the more sluggish oscillations of the air, which originate from the same sources,—or by the combination of the two.

Every particle is exposed to the influence of these several impressions, the tidal waves of the solid earth having a range, according to Prof. Thomson's calculations (*Phil. Trans.*, cliii, 574), at least two-fifths as great as if the globe were entirely fluid. There is, therefore, good reason to hope, that by the application of mechanical laws to the several phases of the æthereal undulations which produce the phenomena of light, heat, electricity, polarity, aggregation, and diffusion, we may obtain a clearer understanding, not only of all the meteorological changes, but also of seismic tremors, crystallization,[*] stratification, chemical action, and general morphology.

If density is a functional product of superficial magnetism and central attraction, the resemblance of the formula $B=\sqrt{AM}$, to the expression of Mr. Graham's experimental law of molecular diffusion, $\text{Time}=F\sqrt{\text{Density}}$ (*Proc. Roy. Soc.*, No. 56, p. 616–7), and to the general theoretical formula of which Mr. Graham's is a corollary,

$$\text{Velocity}=F\sqrt{\frac{\text{Elasticity}}{\text{Density}}}$$

may be something more than accidental.

If we assume the atmospheric density as our unit, $D'=1$, and represent the aerial and æthereal elasticities by E', E'', respectively, the proportion

$$\tfrac{1}{5} : 192{,}000 :: \sqrt{\frac{E'}{D'}} : \sqrt{\frac{E''}{D''}}$$

gives an approximate value for the density of the kinetic æther, $D''=\cdot 00000000000108\,\frac{E'}{E''}$. The magnetic and barometric fluctuations may perhaps furnish the necessary data for determining the unknown ratio $\frac{E'}{E''}$.

[*] The phosphorescence that is often observed during the process of crystallization, and the auroral displays in frosty air, are perhaps owing to analogous vibrations. Every chemical, as well as every physical, action produces a ponderable disturbance of equilibrium, which must give rise to æthereal oscillations, to which, in their simplest form, we give the name of electric, magnetic, or galvanic currents.

ART. XVI.—*On the construction of the Spectroscope;* by LEWIS M. RUTHERFURD.

I KNOW of no good substitute for bisulphid of carbon as the dispersive agent in the spectroscope. Flint glass, besides being expensive, when in large masses and of good quality, possesses but half the dispersive power, and the specimens of the denser glass which I have seen tarnish so rapidly, and have so high an index of refraction as to be practically useless. Having devoted much time to the construction and management of bisulphid of carbon prisms, it is quite possible that the results of my experience may be useful to those who may wish to fit up a spectroscope with such prisms, and perhaps I shall best attain the object by describing my own instrument.

The two principal telescopes are provided with objectives of 1·6 inches aperture and 19 inches focal length. The slit or collecting telescope has but one motion about a vertical axis at the side of the platform and just in front of the objective, enabling it to command all parts of the platform. The observing telescope has two motions, one about the central axis of the instrument, and the other about a second vertical axis, which by means of a slide, capable of being clamped, can be placed under the last surface of any prism on the platform; thus commanding by one motion the whole spectrum.

Before the slit is a prism for the comparison of different spectra, and the observing telescope is provided with eye-pieces of various powers. The first circuit consists of six prisms which are of brass faced with plates of glass, cemented with glue and molasses. These are each of about the angle of 60° and present an aperture of 2·9×1·8 inches. The faces to receive the glass are carefully ground to a flat surface and the glass quite thick and free from veins has been selected with reference to the flatness and parallelism of the sides.

Since however it is scarcely possible to find glass with parallel surfaces, care has been taken so to place the glass that the inclination of its faces is perpendicular to the axis of the prism. After grinding the prisms, the bases were so adjusted by filing that the refracting surfaces are rigidly perpendicular to the plane of the platform. This once done removes all necessity for foot screws, which complicate the prisms and add to the expense. The surface to be glazed was washed with an alkaline solution to remove all grease, and then in dilute nitric acid, finally washed in pure water and allowed to dry spontaneously. After being warmed, the prisms were so placed that the surface to be glazed was uppermost and in a horizontal position: the glass, having been cleaned, after the manner of a plate for photo-

graphic purposes; also warmed and both surfaces to be in contact dusted with a fine camel's hair brush, was placed in position upon the prism, a hot and fluid mixture of glue and molasses was then applied with a fine brush around the edges of the glass, whereupon a uniform and very thin film of the cement was introduced between the glass and the prism by capilarity. The prism was left untouched, until the cement had hardened so as to admit of its being removed and placed glass downward, for at least a day, when the next surface was treated in like manner. After the expiration of another day, I generally put on another coat of cement, much thicker in consistency than the first. In five days more the prism was ready to be filled through an orifice in the top, to which is fitted a ground stopper which is rendered perfectly tight by a little molasses. It may be thought that my description is needlessly particular, but I have mentioned nothing which experience has not shewn to be necessary to the permanence or performance of the prism.

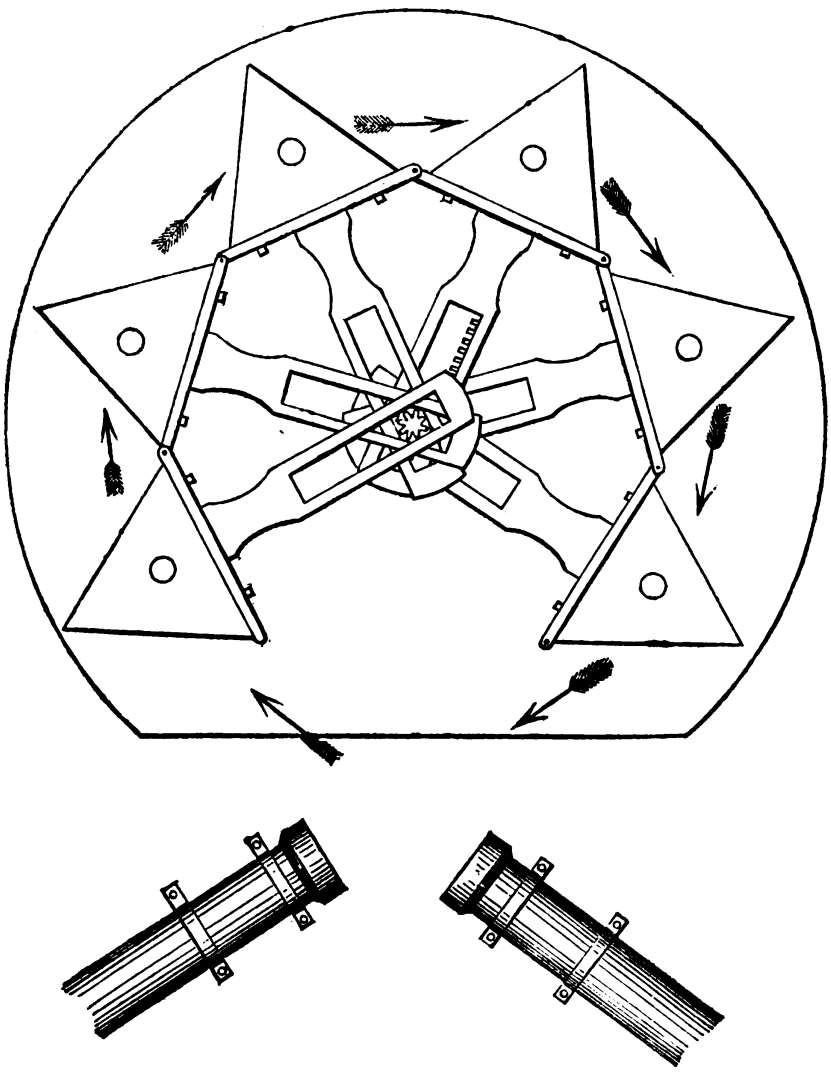
I soon discovered that, after I had made a good prism, its performance would be uncertain, and I finally traced the difficulty to a want of equal density in the bisulphid of carbon, and this peculiarity I have observed not only in the fluid of commerce, but quite as much so in that specially redistilled for the purpose. The fact of this unevenness of density is found in two ways. If a good prism which, with a high power, refuses to define the soda line, (a more stringent test than solar lines,) is violently shaken and then placed in position, it will for a few minutes define beautifully, but gradually settle into its former condition. By covering the aperture of the prism, except a small portion of the upper part, and bisecting the soda line with a spider's web in the eye-piece, all parts of the instrument being clamped, then covering all but the lower portion of the prism, it will be found that the soda line has been carried to a notable extent toward the violet end of the spectrum.

This want of homogeneity in the bisulphid of carbon is entirely different from the disturbance of density by thermal variations. It is a permanent feature of some specimens of the fluid, and is most observable when the prism has been longest at rest in equable temperature. I have one such prism, filled nearly two years since, which defines beautifully for a short time after being well shaken, but soon returns to a poor condition. The difference between the indices of refraction of the upper and lower strata is quite a measurable quantity. My mode of overcoming this obstacle is to filter several pound bottles of the bisulphid of carbon into a long glass jar, having a faucet at the bottom and a ground stopper at the top. After remaining undisturbed two days, the liquid arranges itself according to its density, and I fill the prisms from the faucet, being careful not to shake the jar.

Careful and repeated measures give for the index of refraction of the soda line with the prism first filled from the bottom, 1·62376, and with the ninth prism, filled with the fluid near the upper portion of the jar, 1·62137.

In order to obtain fine definition, it is necessary that the prisms should be placed at the angle of least deviation for the ray under observation. To make the adjustment with several prisms, or to change it when made, is so laborious and troublesome a task as almost to amount to a prohibition of the use of a powerful battery for practical and extended investigations. To remedy this

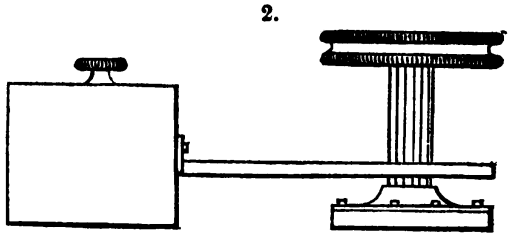
1.



evil I have devised and executed a mode by which I effect the adjustment of all the prisms by one motion of a milled head. An inspection of the accompanying Fig. 1, which represents the

system of prisms without the button, as seen from above, will shew the manner in which this adjustment is accomplished. Upon the glass plate which forms the platform of the instrument, and in the center of the system, is cemented a brass plate, in a cavity of which revolves without shake a pinion provided at the top with a milled head, as seen in figure 2. The prisms are all hinged together at the corners: and from the back of each projects at right angles a brass bar provided with a slot, which, embracing the revolving standard accurately, retains each prism in such a position that, whether nearer or more distant from the center, its back is always perpendicular to the radius connecting it with the center of the standard. The slot of the third prism is provided with teeth which gear into the pinion, so that by turning the milled head this prism is forced to approach or depart from the center: but, from the construction, this cannot take place without imparting a similar motion to each of the other prisms, and thus, at will, their backs are made tangents to a larger or smaller circle, which is the adjustment sought.

2.



This mechanism is capable of adjusting six, or any smaller number, of equi-angled prisms. The outer spiral, when more than six are used, must be adjusted by hand.

New York, Dec. 10, 1864.

ART. XVII.—*Contributions from the Sheffield Laboratory of Yale College.* No. VIII.—*On crystallized Diopside as a furnace product;* by GEORGE J. BRUSH.

SOME months since, Mr. Joseph C. Kent, Director of the Cooper Iron Works at Philipsburg, New Jersey, sent to me an exceedingly beautiful crystallized furnace product, which he had observed in one of his furnaces. It consisted of a group of colorless to grayish-white prismatic crystals, associated with a grayish vitreous mass of the same substance, intermingled with fragments of anthracite and traces of graphite and metallic iron. Many of the isolated crystals were transparent and colorless, while others were grayish white and transculent; some of the larger ones were over half an inch in length by one sixteenth of an inch in diameter. Mr. John M. Blake has determined them to have the form of rhombic prisms with brilliant reflecting surfaces. He obtained for the acute angle, in four measurements,

with the reflecting goniometer, 86° 50′, 86° 52′, 87°, and 87° 12′. The prism was truncated at each of its obtuse angles by a plane of nearly the same width as the planes of the rhombic prism. The terminal planes were observed on only a few of the smaller crystals, and as they were microscopic were not measured. There is a tendency to cleave apparently in the direction of the rhombic prism, but no reflected image could be obtained from the more or less conchoidal surfaces thus produced.

The hardness of the crystals was about 5·5, and the specific gravity was found to be 3·16 (determined on 138 milligrams of substance). Lustre, vitreous and brilliant. Before the blowpipe in the forceps the substance fuses easily with intumescence to a colorless glass, giving at the same time an intense soda flame.

It is partially attacked by chlorhydric acid, emitting the odor of sulphuretted hydrogen; and entirely decomposed by fusion with carbonate of soda. Analyses made by Mr. Peter Collier, assistant in this Laboratory, gave the following results:

	I.	II.	III.	Mean.	Oxygen.	
Silica,	49·95	49·86		49·91	26·61	28·95
Alumina,	5·03	5·00		5·01	2·34	
Lime,	23·71	23·55		23·63	6·75	14·57
Magnesia,	17·25	17·42		17·33	6·93	
Ferrous oxyd,	0·39	0·41		0·40	·09	
Potash,	....	....	1·42	1·42	·24	
Soda,	....	....	2·16	2·16	·56	
Calcium,	0·30	0·33		0·31		
Sulphur,	0·24	0·26		0·25		
Manganous oxyd,	tr.	tr.		tr.		
				100·42		

Analysis I and II were made by fusion with carbonate of soda, with addition of nitre to oxydize the sulphur to sulphuric acid. The alkalies (III) were determined by Smith's method. The small amount of sulphur was calculated as sulphid of calcium. The oxygen ratio of the mean shows the relation of the protoxyd bases to that of the silica and alumina, considering the latter to replace silica, to be 14·57 to 28·95 or 1 : 2, giving the formula $\dot{R}^3(\dot{\ddot{A}}l, \tilde{S}i)^2$. This is the composition of aluminous diopside, and corresponds very nearly with that described by Hunt from Bathurst, in Canada.[1] The crystalline form, as determined by the observations and measurements of Mr. Blake, shows further its relation to pyroxene, and, taken with the chemical composition, leaves no doubt as to the identity of the crystals with the diopside variety of that mineral. Diopside has been previously observed as a furnace product by v. Kobell,[2] and Hausmann,[3] from iron furnaces at Jenbach, near Schwatz, in the Tyrol, and at Gammelbo in Sweden.

[1] Rept. of Geol. Canada, 1863, p. 467. [2] Münchener gelehrte Anzeigen, xix, 97
[3] Liebig & Kopp, Jahresbericht. 1851, 767.

In regard to the occurrence and formation of this interesting product, Mr. Kent has kindly communicated to me the following facts: "I had one of our large furnaces (21 feet diameter and 55 feet high) in blast for over three years. At the end of the third year the fire brick under the flues commenced burning away, but, unwilling to put the furnace out when it was working well below, I kept on for some weeks, and then took off the blast without 'blowing out,'—as by 'blowing out' the top of the furnace would have been damaged. I then drew the stock out from below, and when the greater part of it had been withdrawn, threw water in to cool the interior and quench the fire. This, of course, left a solid mass in the hearth and exactly in the centre of the furnace, just above the tuyeres, say three and a half feet from the bottom, I found a mass of these crystals clustered within a space of about four cubic feet. I did not find them in any other part of the furnace, and had never before seen or found anything like them."

New Haven, Dec. 9, 1864.

ART. XVIII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Continued from p. 58.)

§ 8. *The condition of the primitive Nebula.*

Whatever may be the distribution of matter in the nebula, and however the particles may move, the beautiful theorem—the magna charta of the nebular theory—obtains, i. e. *the nebula possesses the invariable plane of maximum areas*, or if ω represents the angular velocity, r the projection on the invariable plane of the radius vector drawn out from the center of gravity, m the mass, and A the projection of the area swept over by r in the unit of time, we have, C being a constant,

$$\Sigma m\mathrm{A}=\mathrm{C}; \quad . \quad . \quad . \quad . \quad (5)$$

or, as the *centrifugal force* γ of the particle m with respect to the principal axis is

$$\gamma=\omega^2 r \quad . \quad . \quad . \quad . \quad . \quad (6)$$

while, at least for a very small unit of time,

$$\mathrm{A}=\tfrac{1}{2}\omega r^2, \quad . \quad . \quad . \quad . \quad (7)$$

we have also $$\Sigma m r^{\frac{3}{2}} \gamma^{\frac{1}{2}}=2\mathrm{C}. \quad . \quad . \quad . \quad (8)$$

On account of the resistance of the ether, C will not be quite constant, but decrease in time; still it is apparent that, as a first approximation, we may neglect this resistance by considering C strictly constant.

Now, by the mutual attraction of the particles, the nebula is continually becoming more dense, or r is continually decreasing; hence, by (8), *the centrifugal force of any particle in the nebula is continually increasing.*

But the force of gravity at the surface is likewise constantly increasing; for we may without materially erring conceive the mass below the particle to remain constant, but then *gravity* is inversely as the square of the radius, or *rapidly increasing with the progress of condensation.*

But *these two forces determine the figure of the Nebula.* However irregular the figure may be at first, we see that the moulding forces, by constantly increasing, will at length shape the nebula accordingly. From Plateau's Experiments (see above, § 6, result 1, 2) we know this shape to be a flat ellipsoid. Laplace[1] has demonstrated that but *one single oblate ellipsoid of revolution* will be produced by these forces, *i. e.*

$$z^2 + m(x^2 + y^2) = a_1^2, \quad . \quad . \quad . \quad (9)$$

the plane x, y, coinciding with the *invariable plane*, being the equator, z the *axis of rotation* $= 2a_1$ and

$$m = \frac{1}{\sqrt{1+\lambda^2}}, \qquad \lambda = tg\,\theta, \qquad \sin\theta = e, \quad . \quad (10)$$

e being the *eccentricity* of the meridian; hence the equatorial semi-axis

$$a = a_1\sqrt{1+\lambda^2}. \quad . \quad . \quad . \quad (11)$$

Assuming, for a moment, the nebula to be homogeneous, we can determine the eccentricity by the *density* δ, and the *angular velocity* ω (i. e. by (6) proportional to the square root of the centrifugal force g at a unit of distance). Laplace found, if the mass of the whole nebula be M, and its moment of inertia E, that

$$E = \tfrac{4}{15}\pi\delta a_1^5(1+\lambda^2)\sqrt{g} = \frac{\sqrt{g}}{5}a^2M; \quad . \quad . \quad (12)$$

$$M = \tfrac{4}{3}\pi\delta a_1^3(1+\lambda^2) = \tfrac{4}{3}\pi\delta\frac{a^3}{\sqrt{1+\lambda^2}}; \quad . \quad . \quad (13)$$

$$g' = \frac{25E^2}{M^{\frac{10}{3}}}\left(\tfrac{4}{3}\pi\delta\right)^{\frac{1}{3}}; \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\text{arc}\,(tg = \lambda) = \frac{9\lambda + 2g'\lambda^3(1+\lambda^2)^{-\frac{2}{3}}}{9+3\lambda^2}; \quad . \quad . \quad (15)$$

which last equation he shows to have *but one* single positive real root, so that λ or e has but one value, if g, the centrifugal force, and δ, the density, are given.

But the density is certainly *not* constant throughout the whole

[1] Mécanique Céleste, Liv. iii, chap. III, § 21.

nebula; but as this nebula is a gaseous body, δ will be determined by γ and gravity and, just as in the case of our earth, become uniform in the successive homothetic ellipsoidal shells included between any two successive surfaces. Hence we may consider δ as a function of the equatorial axis of such surface, and, as the density is increasing toward the center, we may, instead of the general law

$$\delta = f(a), \quad . \quad . \quad . \quad . \quad (16)$$

take the law assumed by Laplace for the interior of our earth,

$$\delta = \Delta - c\,a. \quad . \quad . \quad . \quad (17)$$

Plana has shown[1] that this law is the most probable in the case of the earth. Prof. Forchhammer of Copenhagen has lately shown[2] how this law accounts for one of the principal circumstances relating to the succession of geological strata.

It must finally be borne in mind that the nebula may have the various elements at the same place, because the laws of diffusion of gases will apply to the gaseous nebula. Thus far the chemical analyses of meteorites and the spectral analysis of the sun, moon and planets have corroborated this conclusion; still we must not hastily conclude that *some* differences may not obtain,

[1] Note sur la densité moyenne de l'écorce superficielle de la terre. *Astronomische Nachrichten*, 1851, vol. xxv, No. 828. For the density at the center of the earth, he finds 16·3010.

[2] In'edning til en Række af Forelæsninger Stoffernes Kredslöb i Naturen (on the circulation of the elements in nature.) *Nordisk Universitets-Tidskrift*, viii, 1 hefte. Copenhagen, 1862. p. 68-81.

As an instance, *Forchhammer* describes the circulation of *Lime:* first the smaller diurnal orbits between plants and animals and the soil; next the greater annual circulation between land (washed by rain) and the sea; and, being here deposited in large strata through the agency of marine animals, these are again, after long geological periods, put into circulation, especially by the inorganic powers of nature, for these again to recommence a new cycle.—Now, where did the lime at first come from? Forchhammer thinks that as granitic rocks (specific gravity 2·7) are less dense than the dark trap-rocks, (on Bornholm, sp. gr. up to 2·92,) they would, during the first period of the igneous earth, float upon the trap; thus the first solid shell would be formed of granitic rocks, i. e. *free from lime*—consequently unfit to support life, not fossiliferous. By the next revolutions this rock was dislocated and broken through by the underlying trap-rocks containing lime and iron as silicates; the atmosphere being so rich in carbonic acid and being dissolved in the then hot waters would decompose these silicates, and thus bring lime into circulation. Organic life can now first commence—and the first fossiliferous rocks appear. This beautiful idea is further substantiated by the fact that volcanos, after a longer period of rest, commence their eruption with emitting trachytic, i. e. granite-like, masses almost free from lime—which are later succeeded by the *heavy* black lava, containing both lime and iron.

The richest deposit of gypsum, the Triassic period, is succeeded by the extraordinary limestone formation of the Jurassic period, thus giving another link in this chain of inductions, for gypsum, being more soluble, will more rapidly circulate, and thus occasion a greater deposit of limestone.

These views of our illustrious teacher show us what patient investigation yet may accomplish; we see the cause for the succession of granite and trap—see why organic life could not commence earlier than it does—see the cause for abundance of limestone during the Jurassic period, etc.—in the simple circumstance that granitic masses, being lighter than trap, were exterior to the latter in the igneous globe.

as the diffusion certainly is limited by the sinking of the denser particles. In a nebula from which a whole *cluster* of solar systems has been formed, we may therefore expect to find considerably different elements. We thus decline the imputation of Rutherfurd that homogeneity of original diffuse matter "is almost a logical necessity of the nebular hypothesis," and cannot see any real objection to this hypothesis, if, as he says, "we have now the strongest evidence that they (the stars) also differ in constituent materials" (this Journal, 1863, vol. xxxv, p. 77).

In regard to the signification of δ we must remark that, in the following, we use the letter δ to represent *the mean density of the nebula* from the centre to the distance r, while in (17) δ indicates the density of the shell at the very distance r. As (17) is only adduced to serve for a comparison, this course is legitimate. But it is easily demonstrated, that, at least for a spherical nebula, this law (17), if true for the individual shell, will also be true for the mean density of all shells inside of it. For, the actual density varying according to (17), the mean density of the interior body from $r=0$ to r is found to be

$$d = \Delta - c' . r, \quad \ldots\ldots\ldots \quad (18)$$

where $c' = \frac{3}{4}c$. This law[4] is evidently the same as (17).

§ 9. *Attraction in the Nebula.*

As the nebula now may be considered made up of homothetic oblate ellipsoidal shells, individually of constant density, and as we know (from *Méc. Cél.*, liv. iii, chap. I, § 2,) that such a shell does not exert any attraction on a point within, we find *the attraction at any point in the nebula determined by the attraction of the ellipsoid whose surface passes through that point.*

This force, at the point x, y, z, is given by the following formulæ (from *Méc. Cél.*, liv. iii, ch. I, § 4), independent of the law of the density (17), and merely depending on the proved uniformity of the density in each separate shell.

If we put

$$Q = \frac{3m}{2\lambda^3}\left[\tan^{-1}\lambda - \frac{\lambda}{1+\lambda^2}\right], \quad \ldots\ldots \quad (19)$$

then the components X, Y, Z, of the attraction (positive toward the origin) are

$$X = Q.\frac{x}{a_1^3}; \qquad Y = Q.\frac{y}{a_1^3}; \quad \ldots \quad (20)$$

$$Z = \left(Q - \frac{\lambda^3}{1+\lambda^2}\right).\frac{z}{a_1^3}. \quad \ldots\ldots \quad (21)$$

[4] The "Density" in Trowbridge's article (this Journal, xxxviii, 354, 1864), is different, because referring to the density at different periods of time.

All of these three components act to *condense* the nebula; but X and Y also determine the revolution of the particles, while Z has no such influence, all motions in the direction of the axis of z mutually destroying each other, because x, y is the invariable plane. Composing X and Y we get

$$R = Q.\frac{r}{a_1^3}, \quad \ldots\ldots\ldots \quad (22)$$

and *directed toward the axis of rotation*; $r^2 = x^2 + y^2$.

Substituting the first (13) ($m = M$) in (22) we obtain

$$\left.\begin{aligned} R &= \mu.\delta r, \\ \text{where} \qquad \mu &= \frac{2\pi}{\lambda^3}\left[(1+\lambda^2)\ \text{arc}\ (tg = \lambda) - \lambda\right]. \end{aligned}\right\} \quad . \quad (23)$$

As now μ only depends on λ, i. e. on the eccentricity (10) which is constant, the shells being homothetic, we see that μ is at any given moment for all parts of the nebula the same, hence: *the radial force R in the nebula is proportional to the density δ and the distance r from the axis of rotation.*

This simple result is of very great importance, as we shall see in the sequel.

§ 10. *The orbit of the Planets.*

The particles of the nebula had originally motions in all directions; but as we assumed the existence of a momentum of rotation (§ 6), the principle of the invariable plane will keep up this momentum (§ 8), while all motions at variance therewith will in time mutually destroy themselves. Therefore, *all particles describe circles around the axis of rotation.*

Such as the orbit of the single particles that formed a planet will also be the orbit of the latter; hence the eccentricity and inclination of all planetary orbits ought to be zero. This may also be seen from Plateau's Experiment, and agrees well with the smallness of both the eccentricity and the inclination (see §1, 9° and 11°, § 3, 4° and 5°). Still, neither of these two quantities is actually zero. Are, then, these small deviations from this value accounted for by some accessory conditions of the problem?

We think so, for there are *two* modifying circumstances, the *rupture of the ring*—which it is beyond our power as yet to take into consideration—and the perturbating influence of already separated masses. The latter we may estimate. Representing the eccentricity by e, the inclination of the orbit to the ecliptic by i, to the invariable plane by I, we have from observation [*Humboldt's Cosmos*]:*

* The numbers in the last column of the following table are not quite exact.—Eds. Jour. Sci.

	e.	*i.*	I.
Mercury, - - -	·2056	7° 0′	5° 19′
Venus, - - - -	·0068	3° 23′	1° 42′
Earth, - - -	·0168	0° 0′	1° 41′
Mars, - - - -	·0932	1° 51′	10′
Asteroids,[5] - - -	·160	7° 55′	6° 14′
Jupiter, - - - .	·0482	1° 19′	13′
Saturn, - - - .	·0561	2° 30′	48′
Uranus, - - - -	·0466	0° 46′	55′
Neptune, - - -	·0087	1° 47′	6′
Invar. plane, - - -	....	1° 41′	0° 0′

We see how clearly the principal members of the system *move in one plane*, and that this plane is the *invariable plane* of the system; the great planets deviate less than one degree, the principal of the interior planets, Earth and Venus, only $1\frac{2}{3}$ degrees—and even the inclination of the smallest planet, Mercury, amounts to but $5\frac{1}{3}$ degrees! So also in relation to the eccentricity, this being less than one-twentieth for the principal bodies.

As to the deviations, we see that Neptune, which if not the most distant planet, certainly is (or *was*) separated from the next by a very large distance, so that if either could not at all, or but slightly, be disturbed, has indeed the smallest inclination (only 6 minutes!) and about the smallest eccentricity (less than one-hundredth!). Jupiter, which, on account of its enormous mass, could not be much disturbed by other bodies, has an inclination of only 13 minutes, while Saturn and Uranus have—corresponding to their smaller mass—about four times as considerable an inclination (48 and 55 minutes). The eccentricities of these three orbits are about equal; perhaps that of Jupiter is near its maximum, or the eccentricity of Saturn and Uranus near their minimum.

The inclination of the Earth and Venus is greater than that of the exterior planets, for the mass of the former is small as compared to that of the latter; but as Venus and the Earth are the great planets among the interior, we see that the inclination and eccentricity of Mercury's orbit are much more considerable than either, and that Mars has less inclination and eccentricity than Mercury. Is it because Jupiter, the only planet that would exert considerable perturbation on its development, was so far distant?

The orbit of the asteroids is explained in § 5, V.[6] We gave publicity to these views in an address delivered before the physical section, at the meeting of the Scandinavian philosophers, July, 1860.

[5] Mean of *the first* 72 *Asteroids*, elements given in Table of Smithsonian Report, 1861, p. 218–219.

[6] We intended in this place to give a fuller account of our views concerning the development of the asteroids; but learning from a letter of Mr. Trowbridge that the continuation of his article will contain a solution of this problem, I abstain for the present from publishing my details.

The same principles will apply to the satellites; but we have too few data to make a comparison of this principle with observation profitable.

§ 11. *The periodic time of the Planets; Kepler's third law.*

Since every particle in the same shell revolves around the axis under the influence of a force R proportional to the distance r from the axis (§ 9), we know from mechanics that the periodic time T of such a particle is

$$T=\frac{2\pi}{\sqrt{\mu\delta}}, \quad \ldots\ldots \quad (24)$$

where, it will be remembered (23), μ is the same for the whole nebula, and δ constant for the same shell, so that *the time of revolution is the same for all particles of the same homothetic shell, but for the different shells inversely proportional to the square root of the density.* Thus every shell rotates as if it were solid; and if the whole nebula had the same density throughout it would rotate like one solid. But if the density be different in different parts, some shells will rotate faster than others (§ 12).

Eliminating δ by means of (13) we get

$$\left.\begin{aligned} T^2&=\mu'.\frac{a^3}{M}\\ \mu'&=\frac{8}{3}\,\frac{\lambda^3}{\sqrt{1+\lambda^2}}\,\frac{1}{(1+\lambda^2)\tan^{-1}\lambda-\lambda}\end{aligned}\right\} \quad (25)$$

We know that the ellipticity of the nebula is determined by the centrifugal force, and the latter by the state of condensation (§ 8); and even in case an ellipsoid becomes impossible, we can not but conclude that the figure continues to be determined in the same manner. But the condensation continues—the increase of the centrifugal force depending thereon will also continue and produce a series of rings in a certain succession, just as *one* ring was formed in the experiments of Plateau (§ 7). We see now how the continued increase of the condensation occasions a periodical change in the figure of the nebula. Granting the variation of the figure beyond the possible ellipsoid to be determined by the same circumstances as the ellipsoid itself, we may compare the corresponding stages of the nebula by referring to the same ellipticity e or the same μ' in (25); at any rate, we know that this can be done if we only compare the nebulæ, when within the limits of the possible ellipsoid. But then μ' will be the same for all rings, and as the mass of the planets is but very small as compared to that of the sun, M remains *almost* constant. Then (25) becomes

$$\frac{T^2}{a^3}=\text{constant}; \quad \ldots\ldots \quad (26)$$

or *the squares of the times of rotation of the different rings are as the cubes of their radii.*

If we remember that the possible ellipsoids reach to a proportion of 1 to about 3 between polar and equatorial diameters of the nebula, we can be sure that *this* covers the principal part of the metamorphosis; hence, (26) is rigorously proved for the greatest part of the condensation intervening between the formation of two successive rings; the nebula acquires its principal dimensions while changing in accordance with the ellipsoidic figure, and when abandoning this it quickly passes to the form of a slightly oblate spheroid and a ring. The interruption in our strictly mathematical demonstration cannot, therefore, seriously interfere with (26). But then this or *Kepler's third law is a consequence of the nebular hypothesis*, or *the observations embodied in this law sustain equally the nebular hypothesis and gravitation.*

Again, inductively, we may conclude from Kepler's third law that the interruption in our analytical deductions occasioned by our ignorance of the exact mechanical laws of the metamorphosis of the ellipsoid into the *globe ring* (we might in reference to Saturn find the expression *Kronion-form* convenient) is not of serious consequences.

Thus we may at least conclude from the third of Kepler's great laws that the development of the planets was periodical; for, this law being a fact, and (25) being rigorously true, we *must* have

$$\frac{\mu'}{M} = \text{constant}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

but, as remarked before, M remains essentially constant, hence μ or what is the same λ, i. e. the ellipticity ϵ of the nebula corresponding to the different planets, must have been the same at corresponding epochs, just as we assumed above.

But *if* the metamorphosis of the nebula has been periodic, and not simultaneous, we must ascertain *whether the successive intervals of time were equal or not.* We shall find that *they were equal*, just as it would be the most natural or the simplest to assume.

§ 12. *Spiral Nebulæ.*

In the preceding paragraph we considered the density of the nebula sensibly equal throughout, so that the nebula always rotated like a solid, all particles having sensibly the same period of revolution. This might be done, because the dimensions of such nebula—however immense in reality—are not sufficiently great to produce a very large change in δ (17) in the space allotted to each planet.

But there may be bodies of dimensions so vast as to render it utterly impossible to consider the density approximatively uni-

form throughout the nebulous mass. Then the nebula will *not* rotate like a solid, but the angular velocity ω of any particle will be

$$\omega = \frac{2\pi}{T}, \quad \ldots\ldots\ldots \quad (28)$$

or, by (24),

$$\omega = \sqrt{\mu\delta}. \quad \ldots\ldots\ldots \quad (29)$$

As μ (23) is constant for the whole nebula, we see that *the angular velocity is proportional to the square root of the density*, or, according to (17), greatest near the center of the nebula.

If θ be the angle of position of those particles which are originally (i. e. when $t = 0$) in one and the same straight line, we have at the time t,

$$\theta = \omega t, \quad \ldots\ldots\ldots \quad (30)$$

or by (29)

$$\theta^2 = \mu.\delta.t^2, \quad \ldots\ldots\ldots \quad (31)$$

Remembering that the density is a function of the distance (16) and also of the time on account of the progressing condensation, we see that (31) may be written,

$$\theta^2 = \mu t^2 f(a, t) \quad \ldots\ldots\ldots \quad (32)$$

At any given moment of time (t constant), all the particles that originally were situated in the same straight line given by

$$\theta = 0 \quad \ldots\ldots\ldots \quad (33)$$

will now form the curve

$$\theta^2 = \varphi(a),$$

i. e. *a spiral.* This contains the fundamental principles of a mechanical theory of the *spiral nebulæ.*

Substituting Laplace's law of the density (17) in (31) or (32), we obtain as the equation of the spiral

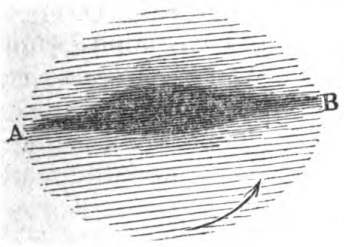
$$a = \frac{\alpha^2 - \theta^2}{C}, \quad \ldots\ldots\ldots \quad (34)$$

wherein $\alpha = \mu.\Delta.t^2$ depends upon the ellipticity (μ), the *density* Δ *at the center*, and the time t, whilst $C = c\mu t^2$ depends upon the same μ and t and the *rate of variation of the density.* We see that these spires are limited, for $0 < \theta < \alpha$; and that the sweep α of the spire increases with the *age* of the nebula, the *density* at its center, and the *ellipticity.*

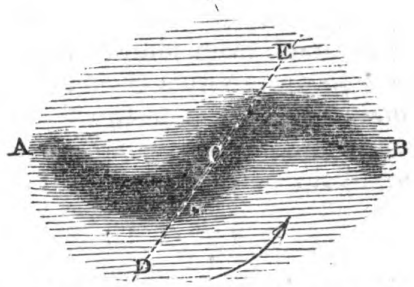
In order that such spiral structure may become apparent in a regular ellipsoidal nebula, the brightness must originally have been different in different meridians, though the density was constant in the same shell, i. e. the same in all meridians. Thus, if the brightness in the nebula was originally greatest in the opposite meridians AC and BC, (Fig. 1.) and it rotates in the direction of the arrow around the axis C, the *spiral nebula,* (fig. 2,) would result. As the age increases, the sweep, or the angle

BCE = α, would increase, whilst A and B remain nearly at the same distance from C: so that an annular nebula with a central core might in time result from a spiral nebula; even several concentric rings might be formed.

1.



2.



We cannot suppose any nebula to have different brightness in parts of the same density; and neither is it reasonable to assume such vast masses to be already shaped to a regular ellipsoid by the influence of the central forces (see 7).

It is much more reasonable to think that the nebulous masses at first were of *any shape*—such as might result from a predominating attraction of those portions where the heaviest elements were formed or collected in greater abundance. Then the formulæ deduced in the preceding paragraphs, though no longer representing the exact conditions of the nebula, still would continue to be approximate; the angular velocity would still be greatest near the central parts, as can also easily be shown directly, by considering the motion of each particle as subject to the attractions of all the others. Then the particles originally in a straight line would still in time form a spiral.

So we see that a nebula originally in the shape of a light rectilinear cloud with a condensation near the middle, like the part AB in fig. 1, would after some time exhibit a spiral like the dark part in fig. 2. The nebulæ, Herschel 1061, and H. 1337, as seen by Lord Rosse,[7] have exactly such a form. If, instead of having the nucleus in the middle, the original nebula had been denser near one extremity, like fig. 3, a simple spire like fig. 4 would

3.



4.



be the resulting spiral nebula, as we see it in H. 327, H. 1946,

[7] Prof. G. P. Bond, Director of Harvard College Observatory, kindly sent me copies of a number of Rosse's latest figures of spiral nebulæ—for which important service I here repeat my sincere thanks.

etc. A nucleus with four branches of different density and magnitude would give a spiral nebula like the beautiful object, Messier 99. If each of the two arms in figure 1 had been subdivided into two branches, H. 2084 would result.

These few remarks must be sufficient at this place. We have already deduced forms as fanciful as H. 1196, H. 131, H. 1744, and others, from simple rectilinear forms, and we hope before long to discuss this theory more at length. We here merely intended to show that the forms revealed to us by the great telescope of Lord Rosse appear to be simple mechanical consequences of the nebular theory, if applied to very large nebulæ.

§ 13. *The Law of the Planetary Distances.*

The law of the planetary distances has not as yet been discovered, though it has been most diligently sought for as the principal element of the "Harmony of the Spheres." The endeavors of Plato were in vain, and Kepler at last ascended to the truth that the present distances are not exactly the original ones. Titius, and after him Bode, came near it [§ 2, (1)]; but the *deviations* from this law remained unaccounted for, thus not giving the conformation most essential to any law.

To find the true law of the planetary distances has been our aim for nearly ten years; we hope the sequel will prove that we at length have found the solution of this problem in the following law:

The mutual distances of the planets correspond to equal intervals of time.

That this is a *fact* we will demonstrate; but *why* these intervals were equal we are not yet able fully to see—still we know that this is the simplest way in which the periodicity in the development of the nebula as found in § 10 can obtain.

Deferring a thorough discussion of the earlier attempts, (some of which are almost contemporaneous with our own solution,) to some future opportunity, we will now give the *inductive reasoning* which leads to our law above stated.

There are a few well known laws in the evolution of the nebula which embody the solution of the problem. We know that the planetary masses are insignificant as compared with the solar mass; hence we see that the orbits of the planets simply mark the equatorial band of the condensing nebula at those definite periods when the radius of the nebula had diminished to the distance of the planet. Thus we see that *the planetary distances must be functions of time.*

Or, if it be more plain, we may say that the original nebula, in contracting, left at certain intervals a few particles behind to mark the limit of the nebula at those instants. But while condensing, the uttermost particle of the nebula describes a spiral curve; and if we can find the relation between the distance a of

this particle and the time t, we need only to substitute the different intervals corresponding to the formation of the different planets in order to obtain their distances. But as the evolution is regularly periodical (§ 10), it is most probable that these intervals *are equal;* comparison with observation shows this to be the case.

But the motion of such a particle in so rare a nebula is regulated by the attraction of the whole nebula and the resistance of the ether. The first of these forces is inversely proportional to the square of the distance a, since the mass remains sensibly the same, and the particle is considered as on the equatorial *surface* of the nebula. In other words, the force of attraction on the particle is the same as the force of gravitation acting upon a planet. Resistance of the ether will necessarily follow the same law, whether a single particle or a planet be subject to it. But then our analysis[8] of the motion of a planet toward the sun is directly applicable to the motion of the superficial particle in its fall toward the center of the nebula. Formula (10) of that article shows the distance a (radius of the nebula) to be

$$a = \mathrm{A}.\varepsilon^{-2\nu t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

where A is the original distance (or radius of the nebula) and t the time of falling from A to a. Or, if a_t represent the distance of the planet that separated from the principal nebula at a time t *earlier* than the now nearest planet—i. e. the *age* of the planet as counted from Mercury,—the above (35) becomes

$$a_t = \beta.\gamma^t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

where β and γ are constants. But in the analysis leading to (35) the coëfficient ν of resistance

$$\nu = \frac{3}{8}\frac{\delta}{\Delta\varrho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

has been considered constant; here we cannot do so, for though the density δ of the ether and the radius ϱ of the particle may be considered constant, the density Δ of the particle varies very much, about inversely as the cube of the radius of the (homogeneous) nebula. If, therefore, ν' be the value of ν corresponding to the particle at the distance 30·0 of Neptune, ν'' the same at the distance 0·4 of Mercury, we have for a homogeneous nebula

$$\nu' : \nu'' = (\cdot 4)^3 : (30\cdot 0)^3 = 1 : 422000$$

nearly. If δ increases toward the center this proportion would be diminished; but still we see that ν decreases toward the interior. The formula (36) can therefore only express the principal part of the law; how (36) has to be amended in order to take

[8] On the Density, Rotation and relative Age of the Planets. This Journal, 1864, [2], xxxvii, 36.

the variation of ν into account has to be separately investigated. Before we attempt this we will compare (36) with observation.

Erecting at equal distances (§ 10) ordinates proportional to the actual planetary distances, and interpolating by connecting these points by a curve, we see that this curve has the appearance of a *logarithmic curve* (like those given on the plate appended to our former article, this Journal, 1864, vol. xxxvii); and if drawn with sufficient care, we find that the constancy of the subnormal, characteristic of the logarithmic curve, holds good in the present instance—thus proving that (36) really is applicable to the planetary distances. But it is not the exact law, for the axis in the diagram is evidently too far below the curve, or the distances are too great by a constant α, so that the diagram of the planetary distances will be expressed not by (36) but by

$$a_t = \alpha + \beta.\gamma^t, \quad \ldots\ldots\ldots \quad (38)$$

and it remains to be seen whether this additional constant α can be accounted for by the variation of ν (37). Before we investigate this, we will see how far (38) represents observation.

We see that it is almost the same as the law of Titius, but while in the latter t *is a mere index*, it is in (38) a variable, the great independent variable of mechanics, *time* or *age!* Besides, (38) deviates from Titius in the case of Mercury. Adapting the constants of Bode to (38), it becomes

$$a_t = 4 + (1{\cdot}5).2^t. \quad \ldots\ldots\ldots \quad (39)$$

Representing by a the actual distance, we have, for comparison with observation,

Planet.	age, t.	Distance calc. a_t.	obs. a.	Difference.
Mercury,	0	55	38·7	+ 16·3
Venus,	1	70	92·3	− 2·3
Earth,	2	100	100·0	0·0
Mars,	3	160	152·4	+ 7·6
Asteroids ①–⑦②,	4	280	262·3[8]	+ 17·7
Jupiter,	5	520	520·3	− ·3
Saturn,	6	1000	953·9	+ 46·1
Uranus,	7	1960	1918·2	+ 41·8
Neptune,	8	3880	3003·6	+876·4

We see that the *present distances* a agree with the *original* a_t for the principal planet of both groups, for the Earth and Jupiter. Mars, Saturn, and Uranus are about $\frac{1}{20}$th of their distance too near the sun, having approached the latter so much more on account of their mass being smaller. Mercury and Neptune have even approached still more, the former because of the smallness

[8] Calculated from the table in *Smithsonian Report* for 1861, p. 218-219. We found the following interesting fact: mean distance of (1) to (31) = 2·599; of (31) to (56) = 2·679; of (57) to (72) = 2·752, showing that in general *the more distant members of the group of asteroids have been later discovered.*

of its mass, the latter on account of its high age (see this Journal, vol. xxxvii, p. 41). Before the precise influence of resistance was known, these deviations were considered sufficient cause to reject the law of Titius-Bode; but now these very deviations have become essential supports of the truth of that law.

Another and better test of our law (38), and of the constants of Bodo (39), is obtained by directly solving (39) for the age t

$$t = \frac{\log (a_t - 40) - \log 15}{\log 2} \quad . \quad . \quad . \quad . \quad . \quad (40)$$

and seeing how far t is given by the series 0, 1, 2 ... We thus find

Planet.	Age.	too small.
Mercury,	imag.	
Venus,	1·1066	—·1066
Earth,	2·0000	·0000
Mars,	2·9056	+·094
Asteroids ①–⑦②,	3·890	+·11
Jupiter,	5·0001	·000
Saturn,	5·9264	+·073
Uranus,	6·9683	+·031
Neptune,	7·6230	+·377

From this table we see that the age of the planets above that of Mercury is as the series of natural numbers, *the deviations not only being but small*, but just such as influence of the mass would make them. This may be easily proved by the formula contained in the article on the age of the planets before referred to.

If the present age of Mercury be m, then the age of the interior planets will be to that of the exterior ones as $m + \frac{6}{4}$ is to $m + \frac{26}{4}$, or as $2m + 3$ to $2m + 13$. We found this ratio as 1 to 3 (this Journal, vol. xxxvii, p. 43); if true it would follow that $m = 1$, or the *total age of any planet would be* $t + 1$, *the unit being the age of Mercury.*

After having seen that (38), the modified form of (36), is applicable to the planetary distances, we will demonstrate that this modification is consistent with the signification of t, the time.

If the resistance R be proportional to the velocity v, or

$$R = \nu v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

we have the tangential force (this Journal, vol. xxxvii, p. 40)

$$\frac{1}{r} \frac{d\left(r^2 \frac{d\theta}{dt}\right)}{dt} = -R \cos \eta = -\nu r \frac{d\theta}{dt}, \quad . \quad . \quad (42)$$

where r is the radius vector, and θ the anomaly; but Kepler's second law gives

$$r^2 \frac{d\theta}{dt} = c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

so that (43) becomes

$$\frac{1}{r}\left[\frac{dc}{dt}+\nu c\right]=0, \quad \ldots\ldots \quad (44)$$

giving *for* ν *constant*,

$$c=\mathrm{C}.e^{-\nu t}, \quad \ldots\ldots \quad (45)$$

or, since by Kepler's third law, $c^2=a\mu$,

$$a=\frac{\mathrm{C}^2}{\mu}e^{-2\nu t}, \quad \ldots\ldots \quad (46)$$

all of which formulæ are demonstrated in our article referred to above.

Instead of solving the problem directly, we may indirectly try to find how ν must vary that (36) may become (38), i. e. to add a constant term to (46). In other words, C instead of being constant must be considered a function of t, i. e. (44) must be

$$\frac{1}{r}\left[\frac{de}{dt}+\nu c\right]=\varphi(t), \quad \ldots\ldots \quad (47)$$

so that the resistance now becomes, see (42),

$$\mathrm{R}=\nu v-\frac{\varphi(t)}{\cos\eta}=\nu v-\frac{ds}{d\theta}.\frac{\varphi(t)}{r}, \quad \ldots\ldots \quad (48)$$

instead of (41), where $\cos\eta=\frac{rd\theta}{ds}$, and ds is the element of the orbit. The function $\varphi(t)$ can now, by the method of the variation of the arbitrary constants, be so determined that (46) or (36) coincides with (38). Since r is a function of t, we may make

$$f(t)=r\,\varphi(t), \quad \ldots\ldots \quad (49)$$

hence (47) becomes

$$\frac{dc}{dt}+\nu c=f(t). \quad \ldots\ldots \quad (50)$$

Taking the complete differential of (45), i. e. also considering C variable, substituting in (50) and reducing by (45), we obtain for the determination of C,

$$e^{-\nu t}.\frac{d\mathrm{C}}{dt}=f(t). \quad \ldots\ldots \quad (51)$$

This gives, by making K an arbitrary constant,

$$\mathrm{C}=\mathrm{K}+\int e^{\nu t}.f(t).dt, \quad \ldots\ldots \quad (52)$$

which, substituted in (46), gives,

$$a=\frac{e^{-2\nu t}}{\mu}\left[\mathrm{K}+\int e^{\nu t}.f(t).dt\right]^2. \quad \ldots \quad (53)$$

This should be identical with (38), i. e. (remembering that t here is counted from the most distant, in (38) from the nearest planet and that γ in (36) is $e^{-2\nu}$ in (35))

$$a=\alpha+\beta.e^{-2\nu t}. \quad \ldots\ldots \quad (54)$$

Equating (53) and (54), and solving for $f(t)$, we find,

$$f(t)=\nu\frac{\alpha\sqrt{\mu}}{\sqrt{\alpha+\beta e^{-2\nu t}}}, \quad \ldots\ldots (55)$$

or, by (54), $$f(t)=\nu\alpha\sqrt{\frac{\mu}{a}}. \quad \ldots\ldots (56)$$

But Kepler's third law gives $\mu=a.v^2$ (this Journal, vol. xxxvii, p. 38, note); hence

$$f(t)=\nu\alpha v, \quad \ldots\ldots\ldots (57)$$

consequently, by (49), r and a being now the same again,

$$\varphi(t)=\nu\alpha\frac{v}{a}, \quad \ldots\ldots\ldots (58)$$

or (48), $\cos\eta$ being almost equal to one, the orbit being nearly circular, $$R=\nu v\left(1-\frac{v}{a}\right). \quad \ldots\ldots (59)$$

Thus we see that (36) becomes (38) if the resistance R, instead of being simply proportional to the velocity (41), is varying according to (59), which may be comprehended in (41) by taking the factor ν to decrease from $\nu\,(a=\infty)$ to $0\,(a=\alpha)$ according to

$$\nu'=\nu\left(1-\frac{\alpha}{a}\right). \quad \ldots\ldots\ldots (60)$$

This variation of the coefficient of resistance is conformable to (37), since Δ, according to (16) (then δ), increases as a decreases.

The law $$a_t=\alpha+\beta.e^{-2\nu t}=\alpha+\beta.\gamma^t$$

is, therefore, but an amplification of

$$a_t=\beta.e^{-2\nu t}=\beta.\gamma^t:$$

in the latter the coefficient of resistance *is constant*, in the former it *varies according to* (60). As now (60) is real, (54) or what is the same (38) is the real law of the planetary distances, t continuing to represent the age, and not, as in Bode's law, a mere index. *And as now finally* (38), *applied to the actual distances, gives values for t that are very nearly as the natural numbers, our law, announced above, holds true, that the planetary distances correspond to equal intervals of time;* or *the consecutive planets were abandoned at equal intervals of time.*

There remain yet two remarkable consequences to be drawn from this exponential law of the planetary distances. If in (38) *it is sufficiently great* (i. e. the corresponding planet far from the center) to make the first term insignificant as compared to the second, we have approximatively

$$a=\beta.\gamma^t,$$

hence $$a^{t+1}=\beta.\gamma^{t+1},$$

consequently $$\frac{a_{t+1}}{a_t} = \gamma, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

or *for the most distant planets their distances approach to a simple geometrical series whose quotient is the base* γ. But this law will again in part be interfered with on account of the action of resistance on the completed system, which, on account of the higher age, has drawn the most distant planets comparatively nearer to the sun than the less distant ones, so as to *diminish* the above quotient γ.

Again, if t is sufficiently small—or the planet sufficiently near the center—the exponential series contained in (38) is highly convergent, so that perhaps the approximation may be sufficient if only the term of the first order is taken, so that (38) becomes, A and B representing constants,

$$a_t = A + B.t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

hence $$a_{t+1} = A + B(t+1),$$

$$a_{t+2} = A + B.(t+2), \text{ etc.}$$

or, $$a_{t+1} - a_t = a_{t+2} - a_{t+1} = B, \quad . \quad . \quad (63)$$

i. e. *the innermost planets have a tendency to become equidistant.*

Both of these consequences are very plainly marked in the solar system, especially in the lunar, but also in the planetary orbs. For, as regards (61), we have for the distances of

Saturn'	to	Jupiter	as	1·85 to 1.
Uranus	"	Saturn	"	2·01 " 1.
Neptune	"	Uranus	"	1·57 " 1.

For Saturn–Jupiter this proportion is still less than $\gamma = 2$—also because Jupiter both on account of its age and mass has fallen less toward the sun than Saturn; but for Uranus–Saturn the ratio is almost equal to $\gamma = 2$, while for Neptune–Uranus it is less again, on account of the higher age of the first.

The second circumstance, expressed in (63), seems to be exemplified in the orbits of Mercury, Venus, Earth, the three planets that are nearest to the sun, or for which t is the smallest. Their distances are

	Distance.	Difference.
Mars, - - - -	152·4	52·4
Earth, - - - -	100·0	26·7
Venus, - - - -	72·3	33·6
Mercury, - - -	38·7	

We see how Mars, Earth and Venus follow Bode's law exactly, for one-half of 52·4 is 26·2, or very nearly 26·7—but the distance between Venus and Mercury is 33·6 instead of $\frac{1}{2}$ of 26·7 or 13·4. This difference might be considered as a consequence of (63); but we know that it is principally due to the small mass of Mercury.

[To be concluded.]

ART. XIX.—*Periodic action of Water;* by LOUIS NICKERSON.

IN reading, some weeks ago, the article by Prof. Loomis, on the vibrations of water flowing over a dam, I was somewhat surprised at the idea of deriving the peculiar motion from a foreign source, as a column of air; surprised, because, however much the air might effect, by reaction, after the action had commenced, the perturbations of a liquid, in whatever state of motion it may exist, have always been so connected with periodic action as to have given use to the name of its most common attribute "the wave," as the characteristic title of nearly all periodic action. Without an attempt to discuss the question with the distinguished gentleman engaged, I shall endeavor to point out the manner in which the vibrations may be considered simply as the result of a wave peculiarly circumstanced.

I was sitting one day upon the bank of a large river in the West. Before me was a strong ripple, supposed by the people around to have been caused by the lodgment of snags upon the bottom. The sound from it was much louder than the roar of the stream—then in a state of freshet, and itself uproarious. But there was a cadence in it, an easily distinguished division into regular periods, which induced an inclination to pause and watch. My position was just upon the middle of an arc, which a late "caving in" of the bank had indented, each point of the arc running past the average bank of the river toward the center. The one down the river being in the greatest projection gave to the arc the appearance of a crescent, quartering into the bank and with its back down-stream-ward. Now a portion of the main current of the river, striking this lower horn, rounded inward as though to make a whirl within the crescent, but, thrown off, shot over toward the center and up stream. Above, another partial current, cut off from the main stream by a shoal or otherwise, came down along shore, and passing the upper horn bore directly down upon the face of the first eddy. Now they attempt to bear each other back, as though striving for the mastery of the crescent. For an instant there is an equilibrium. Both currents at the place of meeting rise rampantly into waves; both seem to receive reinforcements. They might be supposed to be equally matched, but the upper current receives the most water. For another instant they stand poised and opposed, and then the upper rushes, broken, but conquering, down over the surface of the other. Carrying off, however, not only its superabundance, but dragging along a little more water, so that the lower current quickly regains its ascendency, driving the upper back to be again checked and again overpowered, as before.

Afterward, I watched this place for hours at a time, unfortunately without timing, but yet with so distinct and definite a feeling of the regularity of the periods, that it was easy to estimate in the mind the exact period when, equilibrium having been attained, the lower water would start suddenly back and the accumulated waves from above rush over it, always dragging a sufficient extra quantity of water from the upper current to give for a time the ascendency to the lower. The space thus fought over was three or four feet.

But mill-dams are certainly not in just this situation. The permanent weir opposes no such active resistance as the elastic and moving weir just described. Its characteristic is passive resistance. Must we therefore look to it for continuous action? I take it to be thus:

1st. That a certain quantity of water arrives at the pool, and is all passed over the weir in the end, but in periods.

2d. That the quantity passed between these points, is, at the lesser points of velocity, of greater transverse section. This is obvious.

3d. That the decrement of velocity, and corresponding increment of section, is greater at a point nearly under the point of greatest depression of the curve of amplitude.

4th. That outside of this stream the pool is made up of water in a state of slow motion, at rest, or even in some cases of reaction, whirling or flowing backward.

Now we may examine the curve of hydraulic amplitude and its changes. For this we take the formulæ of permanent motion; which, though not exact, is sufficiently characteristic, as derived from Weisbach,

$$a - a'_1 = \left(\frac{\sin \alpha - z \frac{P}{ab} \cdot \frac{v^2}{2g}}{1 - \frac{2}{a} \cdot \frac{v^2}{2g}} \right) l,$$

when

$\sin \alpha$ = slope of original stream
P = whetted perimeter,
al = transverse section,
v = velocity,
l = distance between a and a_1.
a = depth of dam or known point.
a_1 = required depth.
z = coefficient of resistance,
g = 32·2, or a gravitation.

The form of this curve is represented in the works of almost all hydraulic authors, and its equation shows it to be asymptotic to the original surface. It is easily seen above that when $\sin \alpha$ of the original surface becomes equal to $z \frac{P}{ab} \cdot \frac{v^2}{2g}$ and therefore equal to $\sin \alpha'$ of a transverse section of the pool; $a - a_1 = 0$,

or the pool is simply a continuation of the stream, and if $\frac{v^2}{2g}$ becomes equal to $\frac{a}{2}$, or the height due to velocity becomes equal to one-half the depth, both of the original stream, then $a-a_1=\infty$, a case which we shall examine more hereafter. We know that when the pool is first filled and the water is just on the point of flowing over the dam, the surface is horizontal, and we call it the hydrostatic amplitude. It is moreover true, that when the flowage commences and the hydraulic amplitude obtains, there is a stream of water passing through the pool, various in its velocities, and with a sheath of water, differently circumstanced, around it, which it in some way affects. We have also the admitted law, (Weisbach, vol. 1, art. 307; D'Aubinson, art. 54), that when any stream of liquid is in motion in any direction, its pressure in all other directions is equal to its hydrostatic pressure, less the pressure in the direction of its motion, and generally, that when a liquid in motion is made to pass through a liquid of less velocity, a part of the latter is dragged along by its greater lateral pressure, and passes off with the stream. In 1797 the engineer Venturi applied this principle successfully to the drainage of public lands. (Ewbank's Hydraulics, p. 478.)

In a pool fed at one point and yielding up the water at another, such as a mill-pond, this state of affairs practically obtains: that there is first a stream of water running some distance into the remou, and another passing out; in long dams only near the ends, perhaps, but in those of ordinary size throughout the whole length. This stream comes in contact with and passes through walls of partly quiescent water, not only that of the dead angles, but also of superimposition. For, says the engineer D'Aubinson, "moreover, the water of flowage seem only to be superimposed above the current, and not to participate wholly in its motion. The engineers who took the levels upon the Weser have observed, at a distance of 3884 feet from the dam, that the velocity of the surface was nearly insensible, whilst that at the bottom was quite strong."

We have now the fact that a stream is running through water much more nearly at rest, and that owing to the difference of pressure, some of the slower water must be dragged along in the course of the faster, in quantity and force varying as the difference of lateral pressure. We must remember that as in the end the weir can only pass over the same amount of water as it has received from the upper end, i. e., the water of the current, there must, then, be a periodic lull until the deficiency caused by this dragging action has been replaced.

It might readily appear that, as the velocity may become less from the interior of the stream to the outside, this might occur

and yet a continuous action be kept up. But a little thought will show that as normally the stream passes out exactly the quantity received through its own continuation outside the pool, and as the water which I have shown to be dragged out is an extra quantity, there must be a pause after its exit until the surface, which has been lowered a very small increment by its departure, attains its own proper regimen.

You will notice that this reasoning requires that there should be a rise and fall of the surface of the curve of amplitude. I have seen no such rise myself, nor been able to obtain a confirmation of its occurrence from others. But these causes of its existence appear to me too plain to be disputed. It must by necessity be extremely small,—too small perhaps for observation. For the formula $h = \frac{v^2}{2g}$ when applied to a 4 foot velocity, gives us $h = 0{\cdot}25$, and a velocity of 4·3 yields for h only $h = 0{\cdot}263$. By which we see that a rise which would only add a quantity $= 0{\cdot}013$ to the depth would increase the velocity full $\frac{3}{4}\frac{1}{0}$. Then for the ordinate of the parabola of theoretic fall, we should have for the *vis viva* of the horizontal component a quantity varying as the square of velocity, or, for velocities varying from 4 ft. to 4·3 ft., a mechanical effect varying as 16 to 18·5 against the resistance of the atmosphere. That the atmosphere both outside of the falling water and the column inclosed between it and the dam, would assist by its elastic reaction in the vibrated distance, there can be no manner of doubt. But a cursory thought strikes me as I write, that were the action of the air truly isochronous with these vibrations, then the vibrations would have a constant tendency to increase. Partly it assists, partly deadens.

Again let us recur to our formula. Although made by Weisbach only to measure the curve of amplitude, and for the ordinary case of remou, it still contains the very elements which we need, and its changes may at least mark corresponding changes in the law which we discuss. In

$$a_1 - a_0 = \left(\frac{\sin\alpha - z\frac{P}{ab}\cdot\frac{v^2}{2g}}{1 - \frac{2}{a}\cdot\frac{v^2}{2g}} \right) l$$

we see that $\sin\alpha$ is the measure of the slope of the original current, $z\frac{P}{ab}\cdot\frac{v^2}{2g}$ of the resistances of the whetted perimeter of the pool, whilst the denominator marks the changes which occur in the condition of the stream. To use it for our present purpose we must find the value of a_1 for some finite point on the axis of the stream, and then placing $l =$ to an infinitesimal distance

from that point, the difference $a_1 - a_0$ should show the fluctuation of height due to a periodic change in the discharge. Now when $\sin a = z\frac{P}{ab}.\frac{v^2}{2g}$, it is also by the law of the formula $=\sin a'$ of that transverse section of which the second member shows the resistance; therefore

$$\sin a = \sin a',$$

therefore the velocity of the pool = the velocity of the stream, and the surface line of the pool is a parallel line with the bed; for $a_1 - a_0 = 0$, therefore it is circumstanced as in the original stream. There is no backwater, no difference of pressure and no vibration.

When $$\sin a = 0$$

the surface becomes level, for there is no velocity, no flowage, therefore no resistance, $z\frac{P}{ab}.\frac{v^2}{2g} = 0$; again $a_1 - a_0 = 0$; and there is no action, periodic or otherwise.

So we see that there are two points at which the vibrations cease: namely, when the water is sufficiently high to flow over the dam without much remou, as with a stream undammed, and with its surface a line nearly corresponding with the surface of the original stream; and again when the water is so low as to make the difference between the hydrostatic and hydraulic pressures very small. Of course these limits are much circumscribed by the inertia of a large body of water which has constantly a tendency to absorb and soften these vibrations. The most violent palpitation should then occur when

$$\sin a = \left(z\frac{P}{ab}.\frac{v^2}{2g}\right) m,$$

m being a new quantity to be found by a knowledge of the stream.

Again, if we put $\frac{2}{a}.\frac{v^2}{2g} = 1$, or $\frac{v^2}{2g} = \frac{a}{2}$, or when the height due the velocity of the original stream becomes equal to one-half the depth of the same, we have

$$a_1 - a_0 = \infty.$$

Certainly nature admits no such differences as this. Yet, the fact beautifully follows from this, that when the height due the velocity of the original stream is equal to one half the depth of the same, the back water no longer retains the concave form, but, tending to rise infinitely, is checked by the action of gravity, falls backward upon the original stream, and tends to form a convex wave, with a nearly horizontal surface at a height above the bed of about one and one-half times the hydrostatic height. After this height becomes greater than half the depth, the wave

is actually formed, and the water of amplitude flows to and rebounds from the foot of the stream. Bidone discovered this law, and Belanger has applied to it a formula.

The general formula which relates to this action we may gather from what proceeds. If h be the height of the remou, just before greatest action, $h-h_2=$ height at the beginning of the lull that succeeds. The velocities are then

$$v = v_{,} - v_{,,}$$

And the times $= t = \sqrt{\frac{s_1 - s_2}{g}}$, s_1 and s_2 being spaces due to the two velocities combined with the time, t, of the vibration. Or the time of one vibration is equal to the time in which water falls through the height upon the dam, minus a space due to the velocity of different heights of remou, combined with the time observed. In other words, it is the time required for the quick water to draw a certain portion of nearly quiescent water into its own mean velocity—the quantity of quiescent water so drawn, and consequently the time, depending upon the regime of the remou. To this must be added the time of recuperation.

The action when $a-a_1=\infty$ would be the formation of a remou, similar to the one described as seen by the writer of this in the first part of this paper, just at the point where the stream runs into the pool, which would cause one set of vibrations there, and perhaps another set similar to those on ordinary dams at the weir.

For a practical solution of this question, to prevent the vibrations, I am only prepared to recapitulate the foregoing examination into the nature of the action.

1st, We may consider that the vibrations become practically small when the inertia of the pool becomes sufficient to absorb them; therefore they are small when the quantity of water running over the dam is small in relation to the pool, and $\sin \alpha=0$ or nearly.

2d, The same would be true if there were no backwater, or the stream retained its mean velocity unretarded, obtaining when the $\sin \alpha = \sin \alpha'$, as before, or when the surface becomes parallel with the bed; and again, should the pool be so filled as to make the bed become parallel with the surface.[1]

[1] After the horizontal line (or line of hydrostatic amplitude) which bounds the surface of a pool just on the point of running over its dam is found, and the water begins to flow over, the longitudinal outline of the surface changes from its straight and horizontal form, and becomes a curve, which joins the water of flowage at some distance from the dam, and proceeding backward, after the law of the equation given in this paper, becomes asymptotic to the surface of the original stream. It is called the hydraulic amplitude. This curve marks by its changes every alteration or irregularity in the flowage, either of quantity or velocity or resistance, and having been carefully subjected to mathematical laws, answered as a touchstone to the deductions of this paper. And the law developed, not being dependent upon the form or nature of the weir, is a law for the periodic action of all fluids in a state of motion past an obstacle.

ART. XX.—*Remarks on the Carboniferous and Cretaceous Rocks of Eastern Kansas and Nebraska, and their relations to those of the adjacent States, and other localities farther eastward; in connection with a review of a paper recently published on this subject by M. Jules Marcou,*[1] *in the Bulletin of the Geological Society of France:* by F. B. MEEK.

IT is doubtless known to most of the readers of this Journal, that other explorers have long differed from Mr. Marcou, in regard to several important points in the geology of the Western States and Territories. During the autumn of 1863, it seems that he made an excursion to the West for the purpose of examining some of the localities in Nebraska respecting which he and others could not agree. While making these examinations, he was accompanied by Professor Capellini, an able Italian geologist from Bologna; but, as Mr. Marcou distinctly states that the views set forth in his paper are entirely his own, and that Prof. Capellini may have arrived at very different conclusions, it is due to the latter gentleman, that the geologists and amateur collectors familiar with western geology, who may read this review, should be informed that he is in no way responsible for any of the opinions expressed by Mr. Marcou.

Geological observations made by rail-road or steamboat travel being necessarily disconnected, Mr. Marcou's remarks, in the paper under review, so far as based upon personal examinations, relate to few isolated points along the shores of that part of the Missouri river ascended by him, and lying between St. Joseph, Missouri, and Sioux City, Iowa. By a glance at a map of the West, it will be seen that the distance between these two points, by an air line, is about two hundred miles; and that this part of the river forms the eastern boundary line of the new Territory of Nebraska, consisting of a portion of the southeast corner of a vast area formerly known by the general name of Nebraska Territory.

The first points at which Mr. Marcou seems to have touched, after taking steamboat at St. Joseph, were near Savannah, and in the vicinity of Iowa-point, where he saw outcrops of rock some sixty feet in thickness, consisting of bluish-gray clays, with intercalated layers of gray limestone, all showing a slight inclination of 3° or 4° in a W.N.W. direction. He says that he made no collections here, but that he saw *Productus* and *Terebratula*, showing it to be Carboniferous. (To this formation it had been referred by all others.)[2]

[1] Une reconnoissance geologique du Nebraska; par M. Jules Marcou. Bulletin Geol. Soc. France; xxi, 132-147, January, 1864.

[2] It is worthy of note here that Dr. Owen collected at this locality species which he referred to *Nautilus tuberculatus*, *Productus Cora*, *Spirifer fasciger*, *Terebratula plano-sulcata*, and *Orthis Umbraculum*. (Report Iowa, Wiscon., and Minn., 135.) The

At another locality farther up the river, in the region of Nebraska City, he saw various exposures of rock, from which he constructed a section embracing some seventy feet of strata, consisting of reddish, gray, greenish and black clays, with a six inch seam of cannel-coal, and intercalated layers of light-colored dolomitic limestone, and toward the top a thin bed of sandstone containing numerous fragments of plants, some of which he thought like *Zamites* and *Walchia*;[3] the whole, according to him, showing a general inclination of only 5° to 7° to the N.N.W. These beds, he says, agree in color and other lithological characters with the Lower Trias of France, (*Dyas* of him,) to which horizon he refers them, and differ from the subjacent Carboniferous strata upon which they repose, according to him, unconformably, (sur lequel elles reposent en discordance de stratification). He does not, however, state that he anywhere saw the junction of these supposed two systems of strata, nor does he explain exactly the nature of the discordance of stratification. But we infer that he alludes to the fact of the Carboniferous beds showing at one place a slight inclination of 3° to 4° to the W.N.W.; and similarly slight inclination of the so-called "Dyassic" rocks, of 5° to 7° to the N.N.W., at another.

Geologists familiar with the numerous local undulations of strata in the west, where they often lie so nearly horizontal that their general inclination can only be distinguished from these local undulations by careful observations over considerable areas, will know how to appreciate such evidence as this. The improbability too (though within the range of possibilities) of a newer rock dipping at a higher angle than that upon which it reposes, and in a different direction, when the inferior rock is so nearly in its original horizontal position, will be apparent.

But from these rocks Mr. Marcou collected a number of fossils, which should clear up all doubts in regard to their age. These consist of the following forms, as identified by him, viz:—*Nautilus, Pleurotomaria, Murchisonia, Bellerophon, Panopœa, Edmondia, Avicula, Monotis, Ancella, Myalina, Bakevellia, Pecten, Lima, Orthis, Productus Prattenianus, Productus*, (undt.); *Chonetes mucronata, Spirifer* (*Martinia*) *Clannyanus, Spirifer* (undt.), *Terebratula*

shell he always referred to *Spirifer fasciger*, is now well known to be *S. cameratus* Morton, and that which he called *Productus Cora*, is the *P. æquicostatus* Shumard, two of the most common and characteristic species of our Western Coal-measures.

The species of so-called *Terebratula*, mentioned by Mr. Marcou, is most probably *Athyris* (or *Spirigera*) *subtilita* Hall, as that shell is known to occur there, and at nearly all other places in the same rock, while Mr. Marcou habitually calls it *T. subtilita*.

[3] It should be rememberod that these identifications are not given on the authority of Prof. Capellini, whose opinion on such a question would have been worthy of consideration. I am also gratified to see that, since this gentleman's return to Europe, he has published a work at Bologna, in which he says that he dissents from Mr. Marcou in regard to the age of the rocks at this locality, and thinks that the fossils he saw are more like Carboniferous forms.

near *T. subtilita*, some small Corals, or Bryozoa, an *Apiocrinus*, and a Crinoid near *Encrinus moniliformis*.

That Mr. Marcou may have collected or seen, at Nebraska City, specimens he honestly believes to belong to all the genera and species mentioned by him, and that he did find some of these forms there, the writer is fully prepared to understand from a careful personal study of the same beds and their characteristic fossils at numerous localities in Kansas and Nebraska, as well as from familiarity with collections from that and other localities in the immediate vicinity. For instance, he knows that *Chonetes mucronata* Meek and Hayden, *Productus Prattenianus* Norwood, *Spirigera subtilita* Hall, *Spirifer* (*Martinia*) *planoconvexus* Shumard, *Spirifer cameratus* Morton, *Myalina perattenuata*, *Pleurophorus occidentalis* and *Sedgwickia? concava* M. & H.,[4] together with species of *Aviculopecten*, and one of those forms belonging to the same group as the so-called *Monotis speluncaria* of authors, (genus *Eumicrotis* Meek,) occur there. Dr. Owen also found at this locality (called Fort Kearney in his Report, there being no town there at the time of his visit), in the highest bed of limestone, *Productus costatus*, *P. Flemingii*, and great numbers of *Fusulina cylindrica*. The shell referred by Mr. Marcou to *Spirifer Clannyanus* is beyond any reasonable doubt the same called *S. planoconvexus* by Dr. Shumard, since it is known to occur there and at numerous other localities in the same beds and far below; while it is scarcely distinguishable from *S. Clannyanus*. It is, however, quite as nearly allied to, if not really identical with, the well-known Carboniferous species *S. Urii*. Indeed, specimens of this shell sent to Mr. Davidson from the Coal-measures of Illinois (where it has received from Mr. McChesney the name *Ambocœlia gemmula*) were pronounced by him undistinguishable from British specimens of *S. Urii*. Good wood-cuts of the same shell, from Kansas, sent to Mr. Salter of London for comparison, within the past year, were also referred to *S. Urii*.

Now these species, that is, *Fusulina cylindrica*, *Chonetes mucronata*, *Productus Prattenianus*, *Productus costatus*, *P. Flemingii*, *Spirigera subtilita*, *Spirifer planoconvexus* (or *Urii*), and *Spirifer cameratus* are the most common and characteristic forms of the Coal-measures of Kansas and Nebraska, Northern Missouri and Western Iowa, in the very beds which, it will be seen, Mr. Marcou refers to the Subcarboniferous. They are also there found in the Coal-measures associated with species of *Nautilus*, *Bellerophon*, *Pleurotomaria*, *Murchisonia*, *Edmondia*, *Aviculopecten*, *Monotis*, (so-called), and, especially in Kansas, Nebraska, and northwestern Missouri, with a form so nearly like *Panopœa*

[4] In describing the latter three species, the writer and Dr. H. thought the bed from which they were obtained might be Permian; but on afterward ascertaining that these shells are there and elsewhere associated with numerous well marked Coal-measure forms, they were satisfied that it does not belong to the Permian.

as to readily deceive more skillful paleontologists than Mr. Marcou professes to be.[5]

The name *Ancella*, in Mr. Marcou's list of Nebraska City fossils, is doubtless a mis-print of *Aucella*, there being no such name as *Ancella* known to the writer in Paleontology or recent Zoology. As some authors refer such forms as the so-called *Monotis spelunearia* to *Aucella*, it is probably one of these, which are common in the Coal-measures and Permian rocks of Kansas and Nebraska, to which he alludes. That he found here associated with all the Carboniferous fossils known to occur at this locality, the Jurassic genera *Aucella* (as illustrated and defined by Count Keyserling) and *Apiocrinites*, paleontologists may be pardoned for hesitating to admit.

In regard to *Encrinus*, the writer would remark that he has now before him from the Coal-measure outcrop at Bellevue, Nebraska, (referred to the Subcarboniferous by Mr. Marcou, as will be seen farther on,) the cup of a Crinoid, which in form and the arrangement of its plates, up to the summit of the first radials, as well as in the articulating surfaces for the reception of the succeeding range of pieces, seems to agree exactly with the corresponding parts of the genus *Encrinus*. None of the other parts, excepting as detached pieces, have ever been seen by the writer. The cup of the same or a closely allied species, beyond all doubt the same genus, occurs in the Coal-measures of Illinois; and separate plates of the same are not uncommon through nearly all the Coal-measures of Kansas and Nebraska. That a Crinoid with such a cup may be expected to be found nearly related to *Encrinus*, it is quite reasonable to suppose; but even if it should be found in all respects undistinguishable from that genus, would it therefore be philosophical to refer these beds at Nebraska City, and the Coal-measures of Illinois (placed by Mr. Marcou in the Subcarboniferous) with their great numbers of Carboniferous fossils, in the Permian, or the Dyas as he prefers to call it? If not, then all the arguments based upon the presence of this Crinoid, at Nebraska City, fall to the ground.[6]

Mr. Marcou lays great stress upon the fact that the Crinoids found by him at Nebraska City differ entirely from the numerous American Carboniferous forms hitherto made known. He perhaps forgets that nearly all our Carboniferous species, yet described or illustrated, have come from the Subcarboniferous deposits far below the Coal-measures. Some species, however, are known from our Coal-measures, and the fragments of a number of

[5] The type here alluded to is a widely gaping, edentulous, very thin shell, truncated behind, and, when well preserved, covered with minute granules. In short, it, is the type of a new genus related to *Allorisma*, to which the writer has applied the name *Chœnomya*, in a work now in the press. The typical species, *Allorisma? Leavenworthensis* Meek and Hayden, occurs in the Coal-measures at Leavenworth City, Kansas, and others range much higher in the same series.

[6] See note at the end of this paper.

others have been seen, all of which differ widely from those known from the great Subcarboniferous limestones below the horizon of the Millstone grit.

The next locality examined by Mr. Marcou is at the village of Plattesmouth, some fifty miles farther up the Missouri by an air-line. Here he saw another exposure of rocks, some forty-five to fifty feet in thickness, composed of grayish and dark colored clays, in places streaked with red, together with a six-foot stratum of yellowish dolomitic limestone; all of which he says agree lithologically with the Lower Trias of France and Germany—Permian of authors, (= *Dyas* of him), to which horizon he refers them. As these beds, however, differ somewhat in color and composition from those seen at Nebraska City, he thinks they belong to another and lower division of the so-called Dyas, which, as its name implies, consists of two divisions in Europe, and consequently must be expcetde to present the same feature in this country.

Here too he collected a number of fossils, which, according to him, eminently represent a Lower New Red, or Dyassic, Fauna, though he at the same time admits that these fossils are very like Carboniferous types. But, to let the fossils speak for themselves, through Mr. Marcou, they were *Chonetes mucronata*, *Productus Calhounianus*, *Spirifer Clannyanus*, *Terebratula* [*Spirigera*] *subtilita*, *T. Mormonii*, *Spirifer* (undt.), *Fusulina cylindrica*, together with (from an upper bed) *Monotis*, *Avicula* and *Pecten*.

We may also add that Dr. Owen collected here from these beds specimens he referred to *Fusulina cylindrica*, *Productus semireticulatus*, *P. carbonarius*, *P. longispinus*, *Orthis Umbraculum*, *Spirifer fasciger* (?), *Chonetes semiovalis*, *Allorisma sulcata*, some Corals, and a small undetermined *Spirifer*. (Report Geol., Iowa, Wisc., and Minn., 133.)

Now *Chonetes mucronata* of Meek and Hayden was found in Kansas, ranging through a great thickness of Coal-measures, and the type specimens upon which this species was founded were collected at Fort Riley nearly, two hundred feet below the lowest well marked Permian beds. This was the highest position at which they met with this fossil, after careful examinations of hundreds of exposures; and at this place it was found directly associated with a shark tooth which Prof. Leidy at once recognized as the same he had described from the Coal-measures of the Alleghany Mountains, Pennsylvania, under the name *Petalodus Alleghaniensis*. (See Meek & Hayden's paper, *Proceed. Acad. Sci. Philad.*, Jan., 1859, p. 17.)

As already explained, the species referred by Mr. Marcou to *Spirifer Clannyanus* is the *S. plano-convexus* Shumard, which was originally described from this very locality. *Spirigera subtilita* all know to be a common characteristic Coal-measure species,

from Western Pennsylvania to the Rocky Mountains, and from Nebraska and New Mexico. Mr. Marcou figures it himself, under the name *Terebratula subtilita*, in his *Geology of North America*, even as a Mountain Limestone species, from Utah and New Mexico. His so-called *Terebratula Mormonii*, is a *Retzia*, dedicated by him to the Latter Day Saints, from the fact that he first found it at their Capital City. It is worthy of note, however, that he figures and describes it as a Mountain Limestone species in the work just mentioned. So it would seem this little shell, in migrating eastward, obtained a long lease of life, since it here turns up, according to the same authority, in the so-called Dyas. The identity of the fossil is not questioned; indeed the writer and Dr. Hayden long since identified it, ranging through a great thickness of Coal-measures in Kansas, and northwestern Missouri. The notable point is, that it should be in Utah a Mountain Limestone species, and here at Plattesmouth, part of an eminently characteristic Lower Triassic Fauna!

The *Productus Calhounianus* of Swallow, mentioned by Mr. Marcou, is the same referred by Dr. Owen to *P. semireticulatus*, from which, if separable at all, it is certainly with difficulty distinguished. Whether distinct or not, however, it is known, as stated by Prof. Swallow in describing it, to range far down through all those very Coal-measures in Kansas and Missouri which Mr. Marcou refers to the Mountain Limestone. The geological position of *Fusulina cylindrica*, in this country has already been explained. *Productus longispinus*, *P. carbonaria* and *Orthis Umbraculum*, or at any rate species so referred by Dr. Owen, and sometimes by others, are common to the Coal-measures and the Subcarboniferous beds in the West. The species always called *Spirifer fasciger?* by Dr. Owen, as explained in another place, is *S. cameratus* Morton;—the very specimen of it figured by Owen was from the Plattesmouth locality. It is known to be everywhere characteristic of the Coal-measures, from New Mexico to Nebraska, and from Western Pennsylvania[7] to the Rocky Mountains.

The group of shells to which the name *Monotis* is often applied in this country and England, and by some continental writers, (though generically distinct from the Triassic *Monotis salinarius*, the type of *Monotis* Bronn,) is usually regarded in Europe as a Permian type. It is well known, however, to range through a great thickness of upper Coal-measures in Kansas and Northwestern Missouri, referred by Mr. Marcou to the Subcarboniferous. The names *Avicula* and *Pecten* are used so loosely by paleontologists, that they may be said, as generally understood, to range from the Silurian to our existing seas.

Now, how any geologist, having even a limited knowledge of American Carboniferous rocks and fossils, could regard a group

[7] See Prof. Rogers' Report Pa., ii, 833, fig. 694.

of forms such as those mentioned above from Plattesmouth, as eminently a Lower Triassic (or Dyassic) fauna, seems inconceivable, excepting upon the supposition that he labors under some kind of a hard mental twist or bias on the subject of determining the age of rocks by their lithological characters.

After disposing of the so-called Lower Dyassic rocks at Plattesmouth, Mr. Marcou takes boat again, and ascends the Missouri some fifteen or more miles to Bellevue, north of the broad alluvial valley of Platte river. Here he saw a small exposure of rocks, some fifteen feet in height above the river, composed of whitish and yellowish limestone, and pale blue clays, altogether presenting different lithological characters from the outcrops seen below the Platte, and according to this favorite test of his, belonging to a very different epoch, or in other words to the Subcarboniferous.

Here he collected from the limestones, according to his identifications, the following fossils, viz:—*Productus Flemingii*, *P. semireticulatus*, *P. Cora*, *P. punctatus*, *P. scabriculus*, *P. pustulosus*, *P. pyxidiformis*, *Spirifer striatus*, var. *triplicatus* Hall, *S. Rocky-Montanus*, *S. lineatus*, *Terebratula subtilita*, *T. plano-sulcata*, *T. Royssii*, *T. Utah*, *Myalina*, *Nautilus*, and spines of *Archæocidaris*.

It may be as well to add just here, that Dr. Owen gives the following list of fossils collected by him at this locality, viz: *Fusulina cylindrica*, *Productus punctatus*, *P. Cora*, *P. costatus?*, *P. Flemingii*, *P. Humboldtii?*, *Spirifer fasciger?*, *Orthis Umbraculum*, *Terebratula plano-sulcata*, and a *Bellerophon* near *B. hiulcus*. (Rept. Iowa, Wisc., and Minn., p. 133).

From the same outcrop, the writer has now before him (collected by Dr. Hayden) *Productus costatus*, or a common form of the western Coal-measures generally referred to that species, *Productus Rogersii*, together with the Coal-measure form usually called *Productus punctatus*, *Spirigera subtilita*, *Rhynchonella Utah* (= *Terebratula* Marcou), *Terebratula bovidens* Morton, (= *T. millipunctata* Hall), *Spirifer Kentuckensis*, *S. cameratus*, an *Allorisma*, and the peculiar *Encrinus*-like Crinoid already mentioned.

As has been explained in another place, the shells from these rocks, referred by Owen and Mr. Marcou to *Productus semireticulatus* and *P. Cora*, are the *P. Calhounianus* Swallow, and *P. æquicostatus* Shumard; and, whether distinct or not from the species first named, they are very common in our Western Coal-measures. *P. punctatus* of their lists is *P. tubulospinus* of McChesney; which is scarcely distinguishable from the *punctatus*. At any rate, it is, as remarked by McChesney, very common in the Coal-measures "throughout the Western States." The same shell also occurs in the same position twenty-eight miles below Wheeling, in Ohio. *P. scabriculus* of Marcou's list is, beyond reasonable doubt, the widely distributed Coal-measure species, *P. Rog-*

ersii of Norwood and Pratten, as it is known to occur there, and is figured by Mr. Marcou under the name *P. scabriculus*, in his *N. Am. Geology*. *P. pustulosus* of his list, judging from the figure formerly given by him under that name, may be a variety of the *Rogersii*. *Spirifer striatus*, var. *triplicatus* Hall, (= *S. fasciger* of Owen's list), is *S. cameratus* Morton, everywhere common in the Coal-measures, and unknown from any lower position. *S. Rocky-Montanus* Marcou, is unknown to the writer from any locality east of the Black Hills. *S. lineatus* of Marcou's list is undoubtedly the same shell called *S. perplexa* by McChesney. (*Trans. Chicago Acad.*, 1). It is common in the upper Coal-measures of the West, being, as McChesney correctly states, found "in the Upper Coal-measures, in almost every part of the country where rocks of that age exist." It seems to differ from the Subcarboniferous species generally referred to *lineatus* mainly in being uniformly smaller. *Terebratula plano-sulcata* of Owen's and Marcou's lists is almost beyond doubt the *Athyris orbicularis* of McChesney; at any rate, that is the species figured by Mr. Marcou in his *North American Geology* under the name *Terebratula plano-sulcata* Phillips. It may or may not be identical with Phillips' species; but, as McChesney correctly states, it occurs in "the Coal-measures, particularly the upper portion, extensively distributed in the West." *Terebratula Royssii* is doubtless the *Spirigera* generally referred to that species in the West, but described by McChesney as *Athyris differentis*. Whether identical or not with the *S. Royssii* it is a common Upper Coal-measure form, as remarked by McChesney. *Terebratula Utah* of Marcou is a *Rhynchonella*, and, as long since shown by the writer and Dr. Hayden, is a common companion of the *Retzia Mormonii*, through all the Upper Coal-measures of Kansas and Northwestern Missouri. *Orthis Umbraculum*, more properly *Streptorhynchus Umbraculum*, or at least the shell figured under that name by Owen, is very common in the Coal-measures of Kansas, and ranges up to, if not into, the Permian.

Terebratula bovidens Morton (= *T. millepunctata* Hall) first described from the Coal-measures of Ohio, is a very common species in rocks of that age in the West, and unknown in any lower position. The same may also be said of *Spiriferina Kentuckensis* Shumard.

So we have here a group of fossils which any geologist, or mere amateur collector, acquainted with the forms characterizing our Carboniferous and Subcarboniferous rocks of the Mississippi valley, would at once, and without a moment's hesitation or doubt, refer to the Coal-measures. Some few of them are such as appear to be common to the Coal-measures and Subcarboniferous rocks, or are supposed to be, but all the others are wholly unknown below the horizon of the Millstone grit.

Again, it should be remembered that, of the foregoing lists, *Fusulina cylindrica*, the *Encrinus*-like Crinoid, *Spirigera subtilita*, *Productus semireticulatus*, (or *Calhounianus*), *Productus costatus*, *P. Flemingii*, (= *longispinus*), *Spirifer cameratus*, and *Orthis Umbraculum*, all likewise occur at the very localities, and in the very beds, referred by Mr. Marcou to the so-called Upper and Lower Dyas. It may also be added that, in the interior of Kansas, as well as at some places in Nebraska and Iowa, all the other Bellevue species occur in the very beds called Lower Dyas at Plattesmouth, and some of them, particularly in Kansas, in much higher positions.

In short, all the rocks seen by Mr. Marcou on the Missouri, from St. Joseph to the Cretaceous above Bellevue, belong to one unbroken series of Upper Coal-measures, as was first shown by Prof. Swallow; with possibly the exception of some of the highest out-crops near Nebraska City, where there is a downward undulation, that may have left portions of the Permian on the higher parts of the country. The few little isolated sections seen by him constitute but a mere fractional portion of this series; while his reference of these several outcrops to such widely different epochs, and his supposition that the beds he calls Mountain Limestone, form island-like masses, between those he refers to the Permian or so-called Dyas, were deposited unconformably, however honestly believed by him, may be all set down as purely imaginary. If he had gone out into the interior, where this series is much more extensively developed, and followed carefully up the smaller streams, he would have seen exactly the same beds he at one place calls Subcarboniferous, and at others Upper and Lower Dyas, with intermediate strata, all following each other in regular succession without the slightest physical or paleontological break. He might there also, by the same method of examining isolated sections, and applying the same lithological tests, and loose interpretation of fossil evidence, have found material enough to divide the so-called Dyas into twenty, or as many more, subdivisions as he pleased.

In 1859, the writer and Dr. Hayden, who were directly interested in the Permian discovery, and naturally desired, and confidently expected, to find somewhere a break between the Permian and Carboniferous rocks of that region, spent more than a month in traversing hundreds of miles of the districts in Kansas where these rocks are best developed. They did not hurry from point to point, but followed up the valleys of the streams on horseback, with a camping party, provided with a wagon and team for the transportation of supplies, specimens, &c. Yet, after carefully examining the various beds and seams, inch by inch, collecting all the fossils they could find, and carefully keeping separate those from the different strata and seams, they completely satisfied

themselves that there is no where in the whole series any break indicating a marked change of physical conditions. They found, it is true, alternations of clays, limestones, shales, sandstones, &c., but all resting conformably one upon another, and inseparably linked together by their organic remains. Starting from Leavenworth City on the Missouri, where the same Coal-measure rocks which Mr. Marcou will insist belong, in Iowa, Missouri and Illinois, to the Mountain Limestone, occur characterized by such fossils as *Fusulina cylindrica*, *Spirifer cameratus*, *S. plano-convexus*, (= *Clannyanus*, of Mr. Marcou's lists), *Productus costatus*, *P. semireticulatus*, *P. punctatus*, *P. æquicostatus*, (or at any rate the forms usually so-called in the West), *P. Rogersii*, *Spirigera subtilita*, *Terebratula bovidens*, *Retzia Mormonii*, *Rhynchonella Utah*, &c., (but even here also containing a species belonging to the same genus, and near the so-called *Monotis speluncaria*),—they continued their researches through the succeeding strata to the Permian in the interior. Through a great thickness of these rocks, they found the Coal-measure fossils persistent and abundant, but occasionally associated with a Permian type.[*] The higher they ascended in the series, the greater was the proportion of Permian types observed, while the Carboniferous types gradually disappeared, until at last, above a certain horizon, and near the upper part of the series, only Permian forms were met with, with the exception of the ubiquitous *Spirigera subtilita*, or a shell that could not be distinguished from it. (See this Journal, [2], xxvii, 424, 1859; *Proceed. Acad. Nat. Sci. Philad.*, Jan. 1859, p. 8; *Dr. Newberry's Rept. Geol.*, *Ives' Colorado Exped.*, p. 112, &c.)

One of the most remarkable opinions, however, set forth in the paper under review, respecting these rocks is, that all the Coal-measures of Northern Missouri, and of Iowa and Illinois, belong, not as supposed by all others, to the horizon of the true Coal-measures of Europe, but to the Mountain Limestone series. He also thinks that possibly the Coal-measures of Indiana, Ohio and Michigan, and a part of those of Pennsylvania, Virginia and Kentucky, may belong to this lower horizon.

That beds of coal occur in the lower part of the Millstone grit in Pennsylvania, Western Virginia, portions of Kentucky, Tennessee and Arkansas, precisely as in England (see a paper by Messrs. Hull & Green, *Quart. Jour. Geol. Soc.*, Lond., Mar., 1864, p. 246)—while in the western part of Arkansas a great thickness of shale, sandstone, &c., above the conglomerate, is believed

[*] A considerable thickness of these intermediate rocks were included by Prof. Swallow in the Permian. The writer and Dr. Hayden, although satisfied that this intermediate series was not separable by any visible break from the Carboniferous below, or the Permian above, thought that it might be convenient to designate it as Permo-Carboniferous.

to be barren of coal—are well known facts. But to maintain that the Coal-measures of Northern Missouri, Iowa and Illinois belong to this or any lower horizon, to say nothing of those of the other States mentioned, beneath which the Millstone grit is so well developed, is to state a proposition the fallacy of which is manifest to all who have studied these rocks with even a moderate degree of care. In the first place, the whole physical structure of our Carboniferous system, and its relations to the Devonian below, and the Permian above, show at once that the Coal-measures of the Mississippi valley occupy precisely the same horizon as those of Europe. Here, as there, we have first, above the Devonian, the Subcarboniferous or Mountain Limestone group; then the Millstone grit, and above the latter the regular Coal-measures, which in Kansas, shade gradually upward into the Permian. The existence of the extensively developed regular Coal-measures above the Millstone grit in Pennsylvania, Western Virginia, Ohio, Kentucky, Tennessee, Indiana, and Southern Illinois, are facts so well known as scarcely to need mentioning here. It is true that the Millstone grit, which is found from four to five hundred feet in thickness beneath the Coal-measures of Southern Illinois,[9] thins out in a north-westerly direction, so that, farther north in Illinois, in Iowa and Missouri, the Coal-measures are found reposing directly upon the Subcarboniferous rocks. If we take this as an evidence, however, that the Coal-measures there belong to the Mountain Limestone series, we might upon the same principles argue that at other places in these states the coal-bearing strata belong to the Devonian, or even to the lower part of the Lower Silurian; for it is well known that, at some localities there, all the intermediate rocks are wanting, and the Coal-measures are found in direct contact with these older rocks.

If we had no stratigraphical evidence, however, in regard to the parallelism of our Coal-measures with those of Europe, their flora alone would be sufficient to settle this question. Lesquereux, who was especially commissioned to study the fossil plants of the Coal-measures in connection with the State surveys of most of the Western States, containing Upper Carboniferous rocks, including Illinois, says, "if we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres* (certainly the best that has been attempted, either before or after him), all the European genera, even the undefined genus *Aphlebia* Sternb., have repre-

[9] Mr. H. Engelmann, an assistant in the Illinois State Geographical Survey, gives its thickness in the southern part of that State at five hundred feet, and gives a section showing it to rest upon the upper Archimedes Limestone group, everywhere regarded as the upper member of the Subcarboniferous or Mountain Limestone series. (*Trans. Acad. Sci., St. Louis*, Nov. 1862, p. 188.)

sentative species in the Coal-fields of America."[10] He also shows by tables in the same article, that out of a list of about 350 known species of our Coal-measure plants, 150, or approaching one-half, are identical with European Coal-measure species. Dr. J. S. Newberry, an equally good authority in this department of paleontology, had previously arrived at very nearly the same conclusion, from a careful study of the Carboniferous flora of Ohio and some of the neighboring states.[11]

In regard to the identity of the Coal-measures of Illinois, with the regular Coal-measures (overlying the Millstone grit) of Ohio, Pennsylvania, Kentucky, and other neighboring states, it is only necessary to again quote Lesquereux, who has given especial attention to tracing out the parallelism of the subordinate beds at distantly separated localities in the coal fields of the Middle and Western States. On this point he says, (this Jour., [2], xxx, 367,) "Such is, nevertheless, the uniformity of the distribution of the strata of our coal-basins, that a section made in Western Illinois or Western Kentucky, or in any part of the coal-fields in these States, will prove comparatively similar (that is with some differences in the thickness of the strata,) to any section made in the coal-fields of Pennsylvania or Ohio." Now

[10] This Journal, [2], xxx, 65.

[11] The fauna of our Coal-measures has not been compared with that of the equivalent rocks of Europe, in the same detail as its flora. Indeed, judging from the publications on the Mollusks, and other invertebrate remains of the European Coal-measures, our rocks of this age, particularly the upper members in the West, are far richer in animal remains than those of the Old World. Another fact that gives to our Coal-measure fauna a more Subcarboniferous aspect is, that during the deposition of these rocks in this country the sea seems to have much more frequently reoccupied the area of these formations than in the Old World. For, whatever theory we adopt in regard to the formation of coal, to meet all the requirements of the phenomena here presented, we must admit the frequent presence of the sea, since we find Corals, Brachiopods, Crinoids, marine types of Gasteropods, Acephals, &c., occurring through hundreds of feet of Coal-measure rocks in the West. Indeed, during more than twenty years' familiarity with the fossils of the Western Coal-measures, the writer has never met with any fresh or brackish-water types or terrestrial remains, other than plants. The presence of these marine remains is not the exception, but the rule, particularly in the upper and middle members of the western Coal-measures; they are found over wide areas, and through great thicknesses of strata, and in a profusion and state of preservation that precludes the possibility of accounting for their presence by the supposition that they were transported there by currents or earthquake waves. They occur not only in the limestones, shales, sandstones, &c., alternating with the numerous seams and beds of coal, but in some rare instances directly in the coal itself. The writer has a specimen of impure coal from Illinois, in which there is a very thin *Aviculopecten* entirely replaced by pyrites, which, when the block of coal was first broken open, presented almost the brilliancy of burnished gold. In Ohio also, numerous shark teeth and entire specimens of other marine fishes have been found by Dr. Newberry embedded directly in cannel-coal. (See this Journal, [2], xxiii, 212.) Dr. Newberry also found, at another locality in Ohio, specimens of *Solenya* (a marine shell), flattened between the laminæ of good coal. In other instances, beds of coal are known to shade upward into a black, more or less richly bituminous mass, containing great numbers of the shells of Brachiopoda, the delicate, unbroken spines of which can be seen piercing like needles the black matrix.

this conclusion, it should be remembered, is not based upon structure alone, or other lithological characters, but also upon a careful and thoroughly scientific investigation and comparison of the fossil plants characterizing each bed or subordinate stratum.

But it is not alone upon the evidence of structure, and their fossil flora, that the Illinois Coal-measures are known to belong to the same horizon as the regular Coal-measures of Indiana, Kentucky, Ohio, Pennsylvania, &c., for we have also the unmistakable evidence of its group of animal remains. It is true, as in all other formations, species sometimes occur in these rocks in one State that do not in another, but a miscellaneous collection of shells, Corals, Crinoids, Bryozoa, &c., from the Coal-measures of any part of Illinois, would be at once referred to that horizon by any person familiar with the forms characterizing rocks of the same age in any of the States above mentioned. Long lists of species might be cited to illustrate this point, but it is wholly unnecessary; we may remark, however, that *Pleurotomaria sphærulata*, *P. tabulata*, *Euomphalus catilloides*, and *Macrocheilus primigenius*, of Conrad, which are among the most characteristic fossils of the Coal-measures, not only in Illinois, but in Iowa, Missouri, and in part in Kansas, were first described by Mr. Conrad from the regular Coal-measures of Western Pennsylvania.

In relation to the Coal-measures of Illinois belonging to the same horizon as those of Missouri and Iowa, it is scarcely necessary to say anything, as all in this country who have given any attention to the subject know this to be the case, and even Mr. Marcou admits it. Indeed, we have the clearest evidence that the Illinois Coal-field was once continuous with that of Missouri and Iowa, from which it is only separated by the broad valley of denudation scooped out by the Mississippi river. Following the Missouri and Iowa Coal-field westward, we find that it passes uninterruptedly into Kansas and the southeast corner of Nebraska —the valley of the Missouri, owing to the northwestward inclination of the strata, not going deep enough to cut it into two distinct fields.

Throughout all this area, these rocks are characterized by essentially the same fauna. As in all other formations, some species are local in their geographical range, but the majority are not, and, as elsewhere stated, most of them range into the Coal-measures of Kentucky, Indiana, Ohio, and some even into Pennsylvania, while comparatively very few of them have even been suspected of being identical with forms occurring in any lower position.

As much of the evidence relied upon in this review in discussing the relations of the Western Carboniferous and Permian rocks, is derived from the presence or absence of certain types of Brachiopoda, it is but fair that we should not dismiss this part

of the subject without letting it be known that Mr. Marcou does not admit the validity of the testimony of this class of Mollusks. His remarks on this point, however, are so extraordinary, that, for fear of being suspected of having done him an injustice in the translation, they are here given in his own words, and in his own language. They are as follows:—"Je suis arrivé à la conviction qu' après les foraminiféres, les brachiopodes sont les plus mauvaises fossiles dont on puisse se servir comme fossiles caractéristiques des formations, et qu'en réalité ils ne sont meme pas du tout les *Leitmuschel.* J' ignore ou les zoologistes placent les brachiopodes, ou meme s'ils sont d' accord entre eux sur la place á leur assigner; mais ces sont certainement des etres tres-inferieurs et plus bas méme dans la série que les coraux, si j'en juge du moins d'après leur utilité pour la geologie pratique."

This is certainly very hard on the Brachiopoda; it was bad enough to place them in any sense of the word below the Corals but to deny them the right, next to the Foraminifera, to testify in regard to the age of the very rocks to which their shells have so largely contributed is still worse. Whatever zoologists may think of this view respecting the rank of the Brachiopoda, they will certainly accord to it the merit of originality. Geologists and paleontologists, however, are not likely to agree with him in regard to the value of this class of Mollusks as a means of distinguishing strata, as their almost united testimony goes directly to the contrary. Various quotations might be made to show this; but one from Mr. Thos. Davidson of London, the highest authority on the Brachiopoda, particularly the fossil forms, will be sufficient. After speaking of their almost universal distribution in marine strata, he says, "their value to the geologist is consequently very great; and, as they so commonly fall under his hammer, where other classes are often but sparingly represented, they must, therefore, be looked upon as excellent data for the age of deposits; for, although some few individual forms pass from one stage to another, the generality are limited to definite horizons."[12]

That in the hands of one who first makes up his mind in regard to the age of a formation from its color and other lithological characters, and then sets to work to interpret its fossils accordingly, without the ability or inclination to discriminate between the genera *Terebratula*, *Spirigera*, *Retzia*, and *Rhynchonella*, the Brachiopoda will be found of little use, and their testimony discordant, is quite natural to suppose. Under other circumstances, however, the result is always very different. Yet after all, the Brachiopoda have less reason to complain of Mr. Marcou's practical, than of his theoretical dealings with them, for

[12] British Foss. Brach. Genl. Introduction, p. 1.

it will be remembered that, so far as he relied at all upon paleontological evidence, he identifies the rocks at Bellevue, with a particular formation in New Mexico, and both with the Mountain Limestone of Europe, almost *solely* upon the testimony of certain *Brachiopoda.*

After completing his researches in the region of Bellevue, Mr. Marcou continued his progress up the Missouri to examine a sandstone formation extending along that stream for some distance below Sioux City, located at the mouth of Sioux River. In regard to the age of this formation, the readers of this Journal will remember that Mr. Marcou and some geologists in this country have differed widely. In 1853, the writer and Dr. Hayden examined it while returning from an expedition to the Bad Lands for Prof. Hall. They found but a few badly preserved fossils in it, but left the locality under the impression that it was probably Cretaceous, as it was immediately overlaid by rock undoubtedly of that age; which opinion was adopted in a paper published by Prof. Hall and the writer in *Mem. Am. Acad. Arts and Sci.*, 1856.

In his Geological Map, however, of N. America, published in 1855, (*Ann. des Mines,* [2], vii), Mr. Marcou had colored this rock here on the east side of the Missouri, *Mountain Limestone,* and along the west side, *New Red Sandstone.*

In papers published in 1856–7, Dr. Hayden and the writer still maintained that it was of Cretaceous age.

Again, in 1858, in reissuing his Geological map, along with his *Geology of North America,* Mr. Marcou left this rock colored as before on both sides of the Missouri, and on page 143 of the *Geology,* says, "the red formation of the vicinity of Sioux City, lying upon the Carboniferous rocks, is of the age of the New Red Sandstone."

About the same time, Dr. Hayden and the writer maintained that the numerous modern types of dicotyledonous leaves found in this rock could not belong to an older epoch than Cretaceous. But in a paper published in the *Archives des Sci. Bibliothèque Universelle,* June, 1858, Mr. Marcou unhesitatingly referred it, from our later description, to the *Jurassic.* Being perfectly satisfied that even this could not be the case, and in order to convince Mr. Marcou and others of the fact, the writer made sketches of some of the leaves found in this rock, and sent them to Prof. Oswald Heer, of Zurich, Switzerland, one of the most eminent botanical paleontologists of Europe, Dr. Newberry, one of our best authorities in this department of paleontology, being then in New Mexico. Before Prof. Heer's reply was received, Dr. Newberry returned, and the specimens of fossil leaves were submitted to him, and he fully agreed with us that the rock must be Cretaceous. The modern affinities of the leaves showed it could not be older, and its position beneath unques-

tionable Cretaceous strata, in a district where the undisturbed condition of all the rocks precluded the supposition that there might have been an overthrow, settled the question that it could not be newer.

Soon after Dr. Newberry's return, a letter was received from Professor Heer, who was evidently deceived by the unusually modern affinities of the leaves (and doubtless, though unconsciously to himself, in some degree by the theoretical views of Mr. Marcou, who was then in Zurich), stating that he could not regard these leaves of Cretaceous age, but that they appeared most nearly allied to Miocene types. Almost simultaneously, a printed pamphlet was issued by Mr. Marcou at Zurich, addressed to Meek and Hayden, in which he took the ground that they had included in this leaf-bearing formation all sorts of rocks, *excepting* Cretaceous, and endorsing Prof. Heer's suggestion that the beds containing these leaves are of Miocene age.

Notwithstanding the fact that Dr. Hayden and the writer had against them as weighty an authority as Prof. Heer, they continued to maintain that the rock is, nevertheless, Cretaceous.

After having expressed so many and such widely different opinions in regard to the age of this formation, we may readily understand that it must have been with no ordinary degree of interest that Mr. Marcou caught the first glimpse of these sandstone escarpments as he ascended the Missouri; especially as he had with him a skilful botanical paleontologist, to assist in unraveling the knotty question. After a thorough exploration, however, of the various out-crops in the vicinity of Sioux City, and along the Missouri below there, both of these gentlemen completely satisfied themselves that this formation *is* Cretaceous, and *only* Cretaceous, as had been from the first maintained by all in this country. This they both stated in conversation at Washington City, and Mr. Marcou frankly admits it in the paper under review. And thus ends one long mooted question in the geology of the West.

There are a few points, however, in regard to this formation, upon which the writer cannot agree with Mr. Marcou. In the first place, he (Mr. Marcou) considers it a fresh-water formation, because he found in it shells of a *Cyrena;* (for which he proposes the name *C. Nova-Mexicana*, from the supposition that it is identical with a species found by him in New Mexico). This is undoubtedly the *Cyrena arenaria* of Meek & Hayden,[13] which was long since described from the same locality and position. Its presence there, however, by no means proves this to be a fresh-water deposit, since the writer and Dr. Hayden found directly

[13] We first described it as a *Cyprina*, from imperfect specimens, but afterward referred it to the genus *Cyrena*.

associated with it casts of an *Axinæa* (= *Pectunculus*), figured and described by Hall and Meek in the *Memoirs Am. Acad. Arts and Sci.* Dr. Hayden and the writer have also described from there a *Mactra*, under the name *M. Siouxensis*, and a *Pharella*. As *Pectunculus* and *Mactra* are marine genera, and *Pharella* and *Cyrena* are brackish-water types, it is manifest that this rock was deposited in a bay or estuary, which must have been alternately brackish, and salt enough to sustain marine Mollusks. The nature of the sediment composing it, as well as the numerous leaves and even trunks of trees, at some places found in it, attest the fact of its being a shore deposit.

Again, while admitting this to be a Cretaceous rock, Mr. Marcou seems to think he is differing from Meek & Hayden, in not referring it to the oldest Cretaceous, and in placing it on a parallel with the New Jersey Cretaceous. In this, however, he is mistaken. We placed it at the base of our section of the *Cretaceous rocks of the Upper Missouri Country*, which position it undoubtedly holds. But we have not, in our more recent papers, especially referred it, even provisionally, to the horizon of the oldest Cretaceous of Europe. By looking at our paper published in the *Proceed. Acad. Nat. Sci. Philad.*, Dec., 1861, page 418, it will be seen that we there refer this rock to the position of the Gray Chalk of English geologists. It will also be seen there, and in our other papers, that we have, almost from the first, maintained that this formation is the exact equivalent of the leaf-bearing beds on Raritan River, New Jersey, forming the inferior member of the Cretaceous rocks of that State.

Mr. Marcou also has some remarks on the question of the parallelism of this sandstone and the overlying *Inoceramus* beds, with the upper part of his Pyramid Mountain section in New Mexico. We have not the space to enter upon the discussion of this question here, nor is it necessary. The able review of this whole subject in Dr. Newberry's report as Geologist of Ives's Colorado Expedition, and Dr. Shumard's several papers on the geology of Texas, place this question in so clear a light that probably no one but Mr. Marcou has any doubts on the subject.

Note on a new Crinoid, referred to on page 164.—Since the remarks on page 164 were in type, Mr. Worthen has sent me specimens of the Crinoid there alluded to, from the Coal-measures of Illinois, which show that, however much it may resemble *Encrinus*, it still presents differences of structure believed to be of generic importance. These differences consist in the presence of *a series of true subradial pieces;* while it differs from the structure (at least the normal) of *Encrinus*, in having but *two primary radials*, instead of *three*, in each ray. A European specimen

of *Encrinus* in the private collection of Smithson, deposited at the Smithsonian Institution, shows five very minute pieces within those generally considered the basal plates. If these minute pieces were developed so as to assume the character of true basal plates, then those generally regarded as such would become subradials, as in our Crinoid. As this, however, seems never to have been the case in true *Encrinus*, it is reasonable to infer that our Carboniferous form presents other differences entitling it to rank as the type of a distinct genus. The genus may be named and characterized as follows:

ERISOCRINUS, Meek & Worthen. Basal plates 5; subradials 5; radials 2×5; anal and interradial pieces none. Column and arms unknown. We know the following species:—

1. *E. typus* M. & W.—Body, below the summit of the first radial pieces, basin-shaped, rounded below, and subpentagonal in outline above; composed of thick, smooth, scarcely convex plates; breadth 0·60 inch, height 0·24 inch. Basal pieces small, about half hidden by the column; subradials three times as large as the basal pieces, equally hexagonal; first radial pieces much larger, of equal size and form, being all wider than long, pentagonal in outline, and broadly truncated above for the reception of the succeeding radials, which are nearly as large, equally pentagonal, and support upon their superior sloping sides either the first brachial pieces, or a series of secondary radials.—From the Coal-measures, near Springfield, Ill.

2. *E. Nebrascensis* M. & W.—Differs from the foregoing in having the cup below the summit of the first radials proportionally deeper, in consequence of the larger size of the subradial pieces, but may be only a variety of the same. Breadth, 0·69; height to top of first radials, 0·35 inch.—From the Coal-measures, Bellevue, Nebraska.

ART. XXI.—*Analysis of a Carbonate of Lime and Manganese, (Spartaite of Breithaupt,) from Sterling, Sussex County, New Jersey;* by S. W. TYLER, A.B. Communicated, with remarks, by C. U. SHEPARD.

MORE than a year since I supplied Mr. Tyler (a graduate of this College) with very fresh specimens of this mineral, to be analyzed during his residence at the Göttingen Laboratory. He has just forwarded to me, under date of Nov. 10th, the results he has obtained.

"Sp. gr. =2·815. Before the blowpipe, decrepitates and becomes black without fusing. With soda and microcosmic salt, gives the well-known reaction of manganese. Not acted on by acetic acid, but dissolves easily with effervescence in cold chlorhydric acid.

Analysis gives,

Mn O,	13·79	
Ca O,	43·65	as 1 : 4 : 5,
CO^2,	42·01	

which leads to $(\frac{1}{5}Mn\,O + \frac{4}{5}Ca\,O)\,CO^2$ as the formula of the species.

	Calculated.	Obtained.
Mn O,	13·79	13·79
4Ca O,	43·49	43·65
$5CO^2$,	42·72	42·01
	100·00	99·45"

This is the same mineral as that referred to by Rammelsberg, page 209 of his *Handbuch der Mineralchemie*, as analyzed by Jenzsch and Richter with the following results:

	By Jenzsch.	By Richter.
Carbonic acid,	40·77	44·04
Lime,	48·75	47·92
Magnesia,	0·92	1·21
Protox. iron	0·38	7·13
Protox. manganese,	6·83	
Oxyd of zinc,	0·38	....
Water,	0·32	....
	98·35	100·30

Rammelsberg gives the following as an approximative formula, based on the foregoing:

$$6\dot{C}a\ddot{C} + \left\{\begin{matrix}\dot{M}n \\ \dot{M}g\end{matrix}\right\}\ddot{C}.$$

Regarding the magnesia, iron and zinc as accidental, I have only to add, respecting the species, that in color it is generally a very pale reddish white, and where associated with dysluite and jeffersonite (hedenbergite) of a bluish gray tint, sometimes (from partial decomposition) yellowish or reddish brown. It cleaves with facility; and the rhombohedrons thus obtained afford, both by the common and the reflective goniometer, angles rather over than under 106°. The planes are striated parallel to their longer diagonals. With borax before the blowpipe the characteristic manganese reaction is speedily produced.

This mineral had for years been recognized in my cabinet as a double carbonate of lime and manganese, and called by me *calcimangite*, a denomination which I have only recently learned has been anticipated by Breithaupt's *Spartaite*. But as this last name is so nearly synonymous with Spartalite, (given in 1852 by Brooke and Miller, to the red oxyd of zinc), I would suggest the substitution of calcimangite for it, as an appellation that may serve to prevent confusion, the more especially as the two minerals occur together.

Amherst College, Dec. 14, 1864.

ART. XXII.—*Contributions to the Chemistry of Natural Waters;* by T. STERRY HUNT, A.M., F.R.S.

IT is proposed to divide this essay into three parts, in the first of which will be considered some general principles which must form the basis of a correct chemical history of natural waters. The second part will embrace a series of chemical analyses of mineral waters from the Paleozoic rocks of the Champlain and St. Lawrence basins, together with some river-waters; and the third part will consist chiefly of deductions and generalizations from these analyses.

I.

CONTENTS OF SECTIONS.—1, atmospheric waters; 2, 3, results of vegetable decay; 4–7, action on rocky sediments; 8, action on iron-oxyd; 9, solution of alumina; 10, reduction of sulphates; 11, kaolinization; 12, decay of silicates; 13, origin of carbonate of soda; 14, Bischof's view rejected; 15, 16, porosity of rocks, and their contained saline waters; 17, saliferous strata; 18, action of carbonate of soda on saline waters; 19, origin of sulphate of magnesia; 20, 21, Mitscherlich's view rejected; 22, 23, salts from evaporating sea-water, composition of ancient seas, origin of carbonate of lime; 25–27, origin of gypsum, carbonate of magnesia, and dolomite; 28, waters from oxydized sulphurets; 29, origin of free sulphuric and chlorhydric acids; 30, of sulphhydric and boric acids; 31, of carbonic acid gas; 32, of ammoniacal salts; 33–35, classification of mineral waters.

§ 1. The solvent powers of water are such that this liquid is never met with in nature in a perfectly pure state; even meteoric waters hold in solution, besides nitrogen, oxygen, carbonic acid, ammonia, and nitrous compounds, small quantities of solid matters which were previously suspended in the form of dust in the atmosphere. After falling to the earth, these same waters become still farther impregnated with foreign elements of very variable nature, according to the conditions of the surface on which they fall.

§ 2. Atmospheric waters, coming in contact with decaying vegetable matters at the earth's surface, take from them two classes of soluble ingredients, organic and inorganic. The waters of many streams and rivers are colored brown with dissolved organic matter, and yield, when evaporated to dryness, colored residues which carbonize by heat. This organic substance, in some cases at least, is azotized, and similar if not identical in composition and properties with the apocrenic acid of Berzelius. The decaying vegetation, at the same time that it yields a portion of its organic matter in a soluble form, parts with the mineral or cinereal elements which it had removed from the soil during life. The salts of potassium, calcium, and magnesium, the silica and phosphates which are so essential to the growing plant, are liberated during the process of decay, and hence we find these elements almost wanting in peat and

coal. See, on this point, the analyses by Vohl of peat, peat-moss, and the soluble matters set free during its decay. *Ann. der Chem. und Pharm.*, cix, 185, cited in *Rép. Chim. Appliquée*, i, 289. Also Liebig, analysis of bog-water; *Letters on Modern Agriculture*, p. 44; and, in the second part of this paper, the analysis of the waters of the Ottawa river.

§ 3. At the same time, an important change is effected in the gaseous contents of the atmospheric waters. The oxygen which they hold in solution is absorbed by the decaying organic matter, and replaced by carbonic acid, while any nitrates or nitrites which may be present are by the same means reduced to the state of ammonia (Kuhlmann). By thus losing oxygen, and taking up a readily oxydizable organic matter, these waters become reducing instead of oxydizing media in their farther progress.

§ 4. We have thus far considered the precipitated atmospheric waters as remaining at the earth's surface, but a great portion of them sooner or later in their course come upon permeable strata by which they are absorbed, and in their subterranean circulation undergo important changes. The effect of ordinary argillaceous strata destitute of neutral soluble salts may be first examined. Between such sedimentary strata and the waters charged with organic and mineral matters from decaying vegetation, there are important reactions. The composition of these waters is peculiar. They contain, relatively to the sodium, a large amount of potassium salts, besides notable quantities of silica and phosphates, in addition to the dissolved organic matters and the earthy carbonates, and in some cases ammoniacal salts and nitrates or nitrites. The sulphuric acid and chlorine are moreover not sufficient to neutralize the alkalies, which are perhaps in part combined with silica or with an organic acid.

§ 5. The experiments of Way, Voelcker, and others have shown that when such waters are brought into contact with argillaceous sediments, they part with their potash, ammonia, silica, and phosphoric acid and organic matter, which remain in combination with the soil; while, under ordinary conditions at least, neither soda, lime, magnesia, sulphuric acid nor chlorine are retained. This power of the soil appears from the experiments of Eichhorn to be in part due to the action of hydrated double aluminous silicates; and the process is one of double exchange, an equivalent of lime or soda being given up for the potash and ammonia retained. The phosphates are probably retained in combination with alumina or peroxyd of iron; and the silica and organic matters also enter into insoluble combinations. It follows from these reactions that the surface-waters charged with the products of vegetable decay, after having been brought in contact with argillaceous sediments, retain little else

than sulphates, chlorids, or carbonates of soda, lime and magnesia. In this way the mineral matters required for the growth of plants, and by them removed from the soil, are again restored to it; and from this reaction results the small proportion of potash salts in the waters of ordinary springs and wells as compared with river-waters. From the waters of rivers, lakes, and seas, aquatic plants again take up the dissolved potash, phosphates, and silica; and the subsequent decay of these plants in contact with the ooze of the bottom, or on the shores, again restores these elements to the earth. See a remarkable essay by Forchhammer, on the composition of fucoids, and their geological relations, *Jour. für Prakt. Chem.*, xxxvi, 388.

§ 6. The observations of Eichhorn upon the reaction between solutions of chlorids and pulverized chabazite, which, as a hydrated silicate of alumina and lime, may perhaps be taken as a representative of the hydrous double silicates in the soil, show that these substitutions of protoxyd bases are neither complete nor absolute. It would appear, on the contrary, that there takes place a partial exchange or a partition of bases according to their respective affinities. Thus the normal chabazite in presence of a solution of chlorid of sodium exchanges a large portion of its lime for soda; but if the resulting soda compound be placed in a solution of chlorid of calcium, an inverse substitution takes place, and a portion of lime enters again into the silicate, replacing an equivalent of soda; while, by the action of a solution of chlorid of potassium, both lime and soda are, to a large extent, replaced by potash. In like manner, chabazite in which by the action of a solution of sal-ammoniac, a part of the lime has been replaced by ammonia, will give up a portion of the ammonia, not only to solutions of chlorids of potassium and sodium, but even to chlorid of calcium. It results from these mutual decompositions, that there is a point where a chabazite containing both lime and soda, or lime and ammonia, would remain unchanged in mixed solutions of the corresponding chlorids, the affinities of the rival bases being balanced.[1] Inasmuch, however, as the proportions of ammonia and potash in natural waters are usually small, when compared with the amounts of lime and soda existing in the form of hydro-silicates in the soil, the result of these affinities is an almost complete elimination of the ammonia and potash from infiltrating waters.

§ 7. That the replacement of one base by another in this way is not complete is shown, moreover, by the experiments of Liebig, Dehérain and others, who have observed that a solution of gypsum removes from soils a certain amount of potash-salt, which was insoluble in pure water. In this way gypseous waters may also acquire portions of sulphate of soda, and perhaps of sulphate of magnesia, from silicates.

[1] This Journal, [2] xxviii, 72.

It is not certain that all the above reactions observed for chabazite are applicable without modification to the double hydro-aluminous silicates of sedimentary strata. Were such the case, important changes might, in certain conditions, be effected in the composition of saline waters. Thus, in presence of a great amount of a hydrous silicate of lime and alumina, solutions of chlorid of sodium might acquire a considerable amount of chlorid of calcium; but it is probable that these reactions, however important they may be in relation to the soil, and to surface-waters with their feeble saline impregnation, have at present but little influence on the composition of the stronger saline waters. It is, however, not impossible that the action of the ancient sea-waters, holding a large amount of chlorid of calcium, upon the hydrated and half-decomposed feldspars which constituted the clays of the period, may have given rise to those double silicates which formed the lime-soda feldspars so abundant in the Labrador series.

§ 8. The reactions just described assume an importance in the case of waters impregnated with soluble matters from vegetable decay, and in this event another and not less important class of phenomena intervenes, which are due to the deoxydizing power of the dissolved organic matter. By the action of this upon the insoluble peroxyd of iron set free from the decomposition of ferruginous minerals and disseminated in the sediments, protoxyd of iron is formed, which is soluble both in carbonic acid, and in the excess of the organic (acid) matter. By this means, not only are great quantities of iron dissolved, but masses of sediments are sometimes entirely deprived of iron-oxyd, and thus beds of white clay and sand are formed. The waters thus charged with proto-salts of iron absorb oxygen when exposed to the air, and then deposit the metal as hydrated peroxyd, which when the organic matter is in excess, carry down a greater or less proportion of it in combination. Such organic matters are rarely absent from limonite, and in some specimens of ochre amount to as much as fifteen per cent.[1] The conditions under which hydrous peroxyd of manganese is often found are very similar to those of hydrous peroxyd of iron, with which it is so frequently associated; and there is little doubt that oxyd of manganese may be dissolved by a process like that just pointed out. A portion of manganese has been observed in the soluble matters from decaying peat-moss; and it seems to be generally present in small quantities with iron in surface-waters.

§ 9. There is reason to believe that alumina is also, under certain conditions, dissolved by waters holding organic acids. The existence of pigotite, a native compound of alumina with an organic acid, and the occasional association of gibbsite with

[1] Geology of Canada, p. 512.

limonite point to such a reaction. That it is not more abundant in solution, is due to the fact that, unlike most other metallic oxyds, alumina, instead of being separated in a free state by the slow decomposition of its silicious compounds, remains in combination with silica. The formation of bauxite, a mixture of hydrate of alumina with variable proportions of hydrous peroxyd of iron, which forms extensive beds in the Tertiary sediments of the great Mediterranean basin, indicates a solution of alumina on a grand scale, and perhaps owes its origin to the decomposition of solutions of native alum by alkaline or earthy carbonates. Emery, a crystalline anhydrous form of alumina, has doubtless been formed in a similar manner. See this Journal, [2] xxxii, 287. The existence in many localities of an insoluble sub-sulphate of alumina, websterite, in layers and concretionary masses in Tertiary clays, evidently points to such a process. Compounds consisting chiefly of hydrated alumina are frequently found in fissures of the chalk in England. On the absence of free hydrated alumina from soils, see Muller, cited in this Journal, [2] xxxv, 292.

§ 10. The organic matter dissolved by the surface-waters serves to reduce to the condition of sulphurets the various soluble sulphates which it takes up at the same time or meets with in its course. These sulphurets, decomposed by carbonic acid, which is in part derived from the atmosphere, and in part from the oxydation of the carbon of the organic matter, give rise to alkaline and earthy carbonates on the one hand, and to sulphuretted hydrogen on the other. In this way, under the influence of a somewhat elevated temperature, are generated sulphurous waters, whether of subterranean springs, or of tropical sea-marshes and lagoons. The reaction between the sulphurets thus formed and the salts or oxyds of iron, copper and similar metals which may be present, gives rise to metallic sulphurets. The decomposition of sulphuretted hydrogen by the oxygen of the air, produces native sulphur, with which are generally found associated sulphates of lime and strontia. By virtue of these reactions, soluble sulphates of lime and magnesia may be completely eliminated from waters, the bases as insoluble carbonates, and the sulphur as sulphuretted hydrogen, free sulphur, or a metallic sulphuret. Moreover, as Forchhammer has pointed out in the paper already cited, sulphuret of potassium in the presence of ferruginous clays is also completely separated from solution, the sulphur as sulphuret of iron, and the alkali as a double aluminous silicate.

§ 11. We have thus far considered the composition of surface-waters as modified by the decay of vegetation, or by the reactions between the matters derived from this source and the permeated sediments. Not less important, however, than the elements thus

removed by substitution from sedimentary strata are those which are liberated by the slow decomposition of the minerals composing these sediments.

It has long been known that in the transformation of a feldspar into kaolin the double silicate of alumina and alkali takes up a portion of water, and is resolved into a hydrous silicate of alumina; while the alkali, together with a definite portion of silica, is separated in a soluble state. The feldspar, an anhydrous double salt formed at an elevated temperature, has a tendency under certain conditions to combine at a lower temperature with a portion of water, and break up into two simpler silicates. Daubrée has moreover shown that when kaolin is exposed to a heat of 400° C. in presence of a soluble silicate of potash the two silicates unite and regenerate feldspar. These reactions are completely analogous to those presented by very many other double salts, ethers, amids, and similar compounds. The preliminary conditions of this conversion of feldspar into kaolin and a soluble alkaline silicate, however, still require investigation. It is known that while some feldspathic rocks appear almost unalterable, others containing the same species of feldspar are found converted into kaolin to a depth of many feet from the surface. This chemical alteration, according to Fournet, is always preceded by a mechanical change of the feldspar, which first becomes opaque and friable, and is thus rendered permeable to water. He conceives this alteration to be molecular and to be connected with the passage of the silicate into a dimorphous or allotropic condition.[3]

§ 12. The researches of Ebelman in the alterations of various rocks and minerals have thrown considerable light on the relations of sediments and natural waters.[4] From the analyses of basaltic and similar rocks, which include silicates of lime, magnesia, iron and manganese in the forms of pyroxene, hornblende and olivine, and which undergo a slow and superficial decomposition under atmospheric influences, it appears that during the process of decay the greater part of the lime and magnesia is removed, together with a large proportion of silica. It was found moreover that in the case of a rock apparently composed of labradorite and pyroxene, the removal of the lime and magnesia from the decomposed portion was much more complete than that of the alkalies; showing thus the comparatively greater stability of the feldspathic element. The decomposition of the feldspar in these mixed rocks is, however, at length effected, and the final result approximates to a hydrous silicate of alumina, or clay. This slow decomposition of silicates of protoxyd-bases appears to be due to the action of carbonic acid, which, remov-

[3] Ann. de Chimie, [2], lv, 225. [4] Ebelman, Recueil des Travaux, ii, 1-79.

ing the lime and magnesia as carbonates, liberates the silica in a soluble form; while the iron and manganese, passing to a state of higher oxydation, remain behind, unless the action of organic matters intervenes to give them solubility.

§ 13. It is to be remarked that, apart from the peculiar and complete decomposition resulting in the production of kaolin, to which orthoclase, oligoclase, and some other feldspathides, as leucite, beryl, and perhaps also the scapolites and albite are occasionally subject, orthoclase is less liable to change than the soda-feldspars, albite, oligoclase and labradorite. Weathered surfaces of these become covered with a thin, soft, white and opaque crust, from decomposition, while the surfaces of orthoclase under similar conditions still preserve their hardness and translucency. The decomposition of feldspathides, and other aluminous double silicates, whether rapid and complete, or slow and partial, apparently yields the same results. A gradual process of this kind is constantly going on in the feldspathic matters which form a large proportion of the mechanical sediments of all formations; and in deeply buried strata is not improbably accelerated by the elevation of temperature. The soluble alkaline silicate resulting from this process is in most cases decomposed by carbonates of lime and magnesia in the sediments, giving rise to silicates of these bases, (which are for the greater part separated in an insoluble state,) and to carbonate of soda. Only in rare cases does potash appear in large proportion among the soluble salts thus liberated from sediments, partly because soda-feldspars are more subject to change, and partly from the fact that potash salts would be separated from the percolating waters in virtue of the reactions mentioned in § 5. Hence it happens that apart from the neutral soda salts of extraneous origin, waters permeating sediments containing alkaliferous silicates generally bring to the surface little more than soda combined with carbonic and sometimes with boric acid, and carbonates of lime and magnesia with small portions of silica.

§ 14. This explanation of the decomposition of alkaliferous silicates and of the origin of carbonate of soda is opposed to the view of Bischof, who conceives that carbonic acid is the chief agent in decomposing feldspathic minerals.[5] The solvent action of waters charged with carbonic acid is undoubted, as shown by various experimenters, especially by the Messrs. Rogers;[6] but this acid is not always present in the quantities required. The proportion of it in atmospheric waters is so inadequate that it becomes necessary to suppose some subterranean source of the gas, which is by no means a constant accompaniment of natron springs. A copious evolution of carbonic acid is observed in

[5] Bischof, Chem. Geol., ii, 181.

[6] This Journal, [2], v, 401.

the vicinity of the lake of Laach, where the alkaline waters studied by Bischof occur.[7] The same thing is met with in many other localities of such springs, among which may be mentioned the region around Saratoga, where saline waters containing carbonate of soda, and highly charged with carbonic acid, rise in abundance from the Lower Silurian strata; but farther northward, along the valleys of Lake Champlain and the St. Lawrence, similar alkaline saline waters, which abound in the continuation of the same geological formations, are not at all acidulous. From this the conclusion seems justifiable that the production of carbonate of soda is a process, in some cases at least, independent of the presence of free carbonic acid. In this connection, it is well to recall the solvent power of pure water on alkaliferous silicates, as shown more especially by Bunsen, and also by Damour, who found that distilled water at temperatures much below 212° takes up from silicates like palagonite and calcined mesotype, comparatively large amounts both of silica and alkalies. (Damour, *Ann. Chem. et Phys.*, [3], xix, 481.)

§ 15 Another and an important source of mineral impregnation to waters exists in the soluble salts enclosed in sedimentary strata, both in the solid state and in aqueous solution, and for the most part of marine origin. In order to form some conception of the amount of saline matters which may be contained in a dissolved state in the rocky strata of the earth, we have made numerous experiments to determine the porosity of various rocks; some few of the results of which may here be noticed. Fragments of the rocks were dried at a heat of 150° to 200° F., in a current of dry air until they ceased to lose weight. They were then soaked in distilled water, and kept under it for many hours beneath an exhausted receiver. When thus saturated they were wiped from adhering water, and weighed; first in air to determine the augmentation of weight from absorption, and secondly, in water to give, by the loss in weight, the volume of the specimens. These data furnish the means of determining the volume of water absorbed, which is given below for 100.00 parts of different rocks from the Paleozoic strata of the St. Lawrence basin.

Potsdam formation, (sandstone)			3	specimens	.... 2·26—2·71
"	"		3	"	.... 6·94—9 35
Calciferous	"	(cryst. dolomite)	4	"	.... 1·89—2·53
"	"	" "	2	"	.... 5·90—7·22
Chazy	"	(argill. limestone)	4	"	.... 6·45-13·55
Trenton	"	(gray crys. ")	4	"	.... 1 18—1·70
"	"	(black impalp. ")	2	"	.... 0·30—0.32
Utica	"	(black shale)	3	"	.... 0·75—2·10
Hudson River	"	(arenaceous shale)			....—7·94
Medina	"	(argill. sandstone)	2	specimens	.... 8·37-10·06
Guelph	"	(cryst. dolomite)	3	"	.... 9·34-10·60
Niagara	"	(impalpable ")	2	"	.... 9·69-10·92

[7] Bischof, Lehrbuch, i, 357-363.

The above data might be much more extended, but sufficient have been given to show the porosity of the principal Paleozoic rocks of the basin.[8]

§ 16. If we take from the Potsdam sandstone the mean of the first three trials, giving 2·5 per cent for the volume of water which it is capable of holding in its pores, we find that a thickness of 100 feet of it would contain in every square mile, in round numbers, 70,000,000 cubic feet of water; an amount which would supply a cubic foot (over seven gallons) a minute for more than thirteen years. The observed thickness of the Potsdam sandstone in the district of Montreal, varies from 200 to 700 feet, and the mean of 500 feet may be taken. To this are to be added 300 feet for the Calciferous formation, whose capacity for water may be taken, like the Potsdam sandstone, at 2·5 per cent. We have thus in each square mile of these formations, wherever they lie below the water-level, a volume of 490,000,000 cubic feet of water, equal to a supply of a cubic foot per minute for 106 years. The capacity of the 800 feet of Chazy and Trenton limestones which succeed these lower formations, may be fairly taken at one half that of those just named. But it is unnecessary to multiply such calculations—enough has been said to show that these sedimentary strata include in their pores great quantities of water, which was originally that of the ocean of the Paleozoic age. These strata throughout the great Silurian basin of the St. Lawrence, are now for the greater part beneath the sea-level; nor is there any good reason for supposing them to have ever been elevated much above their present horizon. Wells and borings sunk in various places in these rocks show them to be still filled with bitter saline waters; but in regions where these rocks are inclined and dislocated, surface-waters gradually replace these saline waters, which in a mixed and diluted state appear as mineral springs. These saline solutions, other things being equal, will be better preserved in limestones or argillaceous rocks than in the more porous and permeable sandstones.

§ 17. But besides the saline matters thus disseminated in a dissolved state in ordinary sedimentary rocks, there are great volumes of saliferous strata, properly so-called, charged with the results of the evaporation of ancient sea-basins. These strata enclose not only gypsum and rock-salt, but in some regions large quantities of the double chlorid of potassium and magnesium, carnallite; and in others sulphate of soda, sulphate of magnesia, and complex sulphates like blödite and polyhallite. Besides these crystalline salts, the mother-liquors containing the

[8] A great many similar determinations will be found in a Report on Building Stones to the British House of Commons in 1839, by Barry, Dela Beche and Smith. See also Delesse. Bull. Soc. Geol., [2], xix, 64.

more soluble and uncrystallizable compounds, may also be supposed to impregnate, in some cases, the sediments of these saliferous formations. The conditions under which these various salts are deposited from sea-water, and their relations to the composition of the ocean in earlier geological periods, are reserved for consideration in § 22. Infiltrating waters remove from these saliferous strata their soluble ingredients; which, together with the ancient sea-waters of other sedimentary rocks, give rise to the various neutral saline waters; while the mingling of these in various proportions with the alkaline waters whose origin has been described in § 13, produces intermediate classes of waters of much interest.

§ 18. I have elsewhere described the results of a series of experiments on the mutual action of the waters of these two classes.[9] When a dilute solution of bicarbonate of soda is gradually added to a solution which, like sea-water, contains besides chlorid of sodium, the chlorids and sulphates of calcium and magnesium, the greater part of the lime separates as carbonate, carrying down with it only from one to three-hundredths of carbonate of magnesia; a portion of lime however remaining in solution as bicarbonate. When the chlorid of calcium is wholly decomposed, the magnesian salt is attacked in its turn, and there finally results a solution in which the whole of the earthly chlorids are replaced by chlorid of sodium. A farther addition of the solution of carbonate of soda gives them the character of alkaline-saline waters; which moreover contain abundance of earthy carbonates.

The substitution of neutral carbonate for bicarbonate of soda in the above experiment does not affect the result, except in causing a somewhat larger proportion of magnesia to be thrown down with the carbonate of lime. The resulting liquid still retains large quantities of earthy carbonates in solution.[10]

§ 19. In the saline waters just considered, chlorids generally predominate, the sulphates being small in amount, and often altogether wanting. Some exceptions to this are however met with; for apart from waters impregnated with gypsum, whose origin is readily understood, there are others in which sulphate of soda or sulphate of magnesia enter largely. The soda-salt may sometimes be formed by the reaction between solution of gypsum and natriferous silicates referred to in § 7; or by the decomposition of gypsum by a solution of carbonate of soda; while in other cases its origin will probably be found in the natural deposits of sulphates, such as glauberite, thenardite, and glauber-salt, which occur in saliferous rocks; a similar origin is probable for many of those springs in which sulphate of mag-

[9] This Journal, [2], xxviii, 170.
[10] Geol. Survey of Canada, Report, 1853-56, p. 468.

nesia predominates. This salt also effloresces abundantly in a nearly pure form upon certain limestones, and is in some cases due to the action of sulphates from decomposing pyrites upon magnesian carbonate or silicate. In by far the greater number of cases, however, its appearance is unconnected with any such process; and is, according to Mitscherlich, due to a reaction between dolomite and dissolved gypsum.

§ 20. In support of this view, it was found by the chemist just named that when a solution of sulphate of lime was made to filter for some time through pulverized magnesian limestone, it was decomposed with the formation of carbonate of lime and sulphate of magnesia. This reaction I have been unable to verify. A solution of gypsum in distilled water was made to percolate slowly through a column of several inches of finely-powered dolomite; and after ten filtrations, occupying as many days, no perceptible amount of sulphate of magnesia had been formed. Solutions of gypsum were then digested for many months with pulverized dolomite, and also with crystalline carbonate of magnesia, but with similar negative results; nor did the substitution of a solution of chlorid of calcium lead to the formation of any soluble magnesian salt. Solutions of gypsum were then impregnated with carbonic acid, and allowed to remain in contact with pulverized dolomite and with magnesite as before, during six months of the warm season, when only inappreciable traces of magnesia were taken into solution. These experiments show that no decomposition of dissolved gypsum is effected by native carbonate of magnesia or by the double carbonate of lime and magnesia at ordinary temperatures.

§ 21. I find however that hydrated carbonate of magnesia readily completely decomposes a solution of gypsum when agitated with it, with formation of carbonate of lime and sulphate of magnesia, and the same result is produced with the native hydrate of magnesia when mingled with a solution of gypsum in presence of carbonic acid. Now there may be dolomites which contain an admixture of hydro-carbonate of magnesia, as there certainly are others which like predazzite, are penetrated with hydrate of magnesia. The reaction between solutions of gypsum and such magnesian limestones, (with the intervention in the case of predazzite, of atmospheric carbonic acid), would suffice to explain the results obtained by Mitscherlich, and the appearance in certain cases of sulphate of magnesia as an efflorescence on dolomites. In the experiments above described, the nearly pure crystalline dolomites from the Guelph and Niagara formations were made use of.

§ 22. When sea-water is exposed to spontaneous evaporation, the lime which it contains separates in the form of sulphate, gypsum being but sparingly soluble in a concentrated brine, and

the greater portion of the chlorid of sodium crystallizes out in a nearly pure state. The mother-liquor of specific gravity 1·24, having lost about four-fifths of its chlorid of sodium, still contains dissolved a large proportion of sulphate of magnesia. If the evaporation is continued at the ordinary temperature, till a density of 1·32 is attained, about one-half of the magnesian sulphate separates, mixed with common salt; and by reducing the temperature to 6° C., a large portion of pure sulphate of magnesia now crystallizes out. The further evaporation of the remaining liquor by the heat of summer causes the potassium salts to separate in the form of a hydrous double chlorid of potassium and magnesium, an artificial carnallite.[11]

By varying somewhat the conditions of temperature, the sulphate of magnesia and chlorid of sodium of the mother-liquor undergo mutual decomposition, with the production of sulphate of soda and chlorid of magnesium. Hydrated sulphate of soda crystallized out from such a mixed solution at 0° C., and by reducing the temperature to −18° C. the greater part of the sulphates may be separated in this form from the mother-liquor of 1·24, previously diluted with one-tenth of water; without which addition a mixture of hydrated chlorid of sodium would separate at the same time. If, on the other hand, the temperature of the mixed solution be raised above 50° C., the sulphate of soda crystallizes out in the anhydrous form, as thenardite. By the spontaneous evaporation during the heats of summer of the mother-liquors of density 1·35, a double sulphate of potassium and magnesium separates. These reactions are taken advantage of on a great scale in Balard's process, as modified by Merle,[12] for extracting salts from sea-water.

§ 23. The results of the evaporation of sea-water would however be widely different if an excess of lime-salts were present. In this case the whole of the sulphates present would be deposited in the form of gypsum at an early stage of the evaporation, and the mother-liquor after the separation of the greater part of the common salt, would contain little else than the chlorids of sodium, potassium, calcium, and magnesium.

§ 24. A consideration of the conditions of the ocean in earlier

[11] The hydrous double chlorid of potassium and magnesium (carnallite of H. Rose), occurs in large quantities in a stratum of clay overlying a great bed of rock-salt 100 feet thick, at Stassfurth in Prussia. It is associated with considerable quantities of sulphate of magnesia. According to Clemm this sulphate of magnesia, to which the name of *kieserite* has been given, and which occurs also in Anhalt, contains but one equivalent of water, (MgO,SO_3+HO). It is not more soluble than gypsum, and unlike the ordinary sulphate of magnesia, loses the whole of its acid at a red heat in a current of steam, the acid passing off undecomposed. This salt is found in such large quantities as to be of economic importance. (Bull. Soc. Chim. de Paris, 1864, p. 297.)

[12] See my paper in this Journal, [2], xxv, 361; also Report of Juries of the Exhibition of 1862, class II, p. 48.

geological periods will show that it must have contained a much larger quantity of lime-salts than at present. The alkaline carbonates, whose origin has been described in § 13, and which from the earliest times have been flowing into the sea, have gradually modified the composition of its waters, separating the lime as carbonate, and thus replacing the chlorid of calcium by chlorid of sodium, as I have long since pointed out.[13] This reaction has doubtless been the source of all the carbonate of lime in the earth's crust, if we except that derived from the decomposition of calcareous silicates. (§ 12). In this decomposition by carbonate of soda, as already described in § 18, it results from the incompatibility of chlorid of calcium with hydrous carbonate of magnesia, that the lime is first precipitated with a little adhering carbonate of magnesia, and it is only when the chlorid of calcium is all decomposed that the magnesian chlorid is transformed into carbonate of magnesia. This latter reaction can consequently take place only in limited basins, or in portions cut off from the oceanic circulation.

§ 25. It follows from what has been said that the lime-salt may be eliminated from sea-water either as sulphate or as carbonate. In the latter case no concentration is required; while in the former the conditions are two,—a sufficient proportion of sulphates to convert the whole of the lime into gypsum, and such a degree of concentration of the water as to render this insoluble. These conditions meet in the evaporation of modern sea-water; but the evaporated sea water of earlier periods, with its great predominance of lime-salts, would still contain large amounts of chlorid of calcium, the insolubility of gypsum in this case serving to eliminate all the sulphates from the mother-liquor. Evaporation alone would not suffice to remove the whole of the lime-salts from waters in which the calcium present was more than equivalent to the sulphuric acid; but the intervention of carbonate of soda would be required.

§ 26. In concentrated and evaporating waters freed from lime-salts by either of the reactions just mentioned, but still holding sulphate of magnesia, another process, which I have elsewhere described, may intervene.[14] The addition of a solution of bicarbonate of lime to such a solution gives rise, by double decomposition, to sulphate of lime and bicarbonate of magnesia. The former being much the less soluble salt, especially in a strongly saline liquid, is deposited as gypsum; and subsequently the magnesian carbonate is precipitated in a hydrous form. The effect of this reaction is to eliminate from the sea-water both the sulphuric acid and the magnesia, without the permanent addition to it of any foreign element.

[13] Canadian Journal for 1858, p. 202; this Journal, [2], xxv, 102, and *Comptes Rendus*, June 9, 1862, p. 1191.

[14] This Journal, [2], xxviii, 174.

§ 27. Gypsum may thus be separated from sea-water by two distinct processes,—the one a reaction between sulphate of magnesia and chlorid of calcium, and the other between the same sulphate and carbonate of lime. The latter, involving a separation of bicarbonate of magnesia, can as we have seen, only take place when the whole of the chlorid of calcium has been eliminated; and if we suppose the ancient ocean, unlike the present, to have contained more than an equivalent of lime for each equivalent of sulphuric acid, it is evident that a lake or basin of sea-water free from lime salts could only have been produced by the intervention of carbonate of soda. The action of this must have eliminated the whole of the lime as carbonate, or at least have so far reduced the amount of this base that the sulphates present would be sufficient to separate the remainder by evaporation, in the form of gypsum, and still leave in the mother-liquor a quantity of sulphate of magnesia for reaction with bicarbonate of lime.

The source of the magnesian carbonate, whose union, under certain conditions, with the carbonate of lime gives rise to the dolomite,[18] may thus be due either to the reaction just described between bicarbonate of lime and solutions holding sulphate of magnesia, or to the direct action of carbonate of soda upon waters containing magnesian salts; but in either case the previous elimination of the incompatible chlorid of calcium must be considered an indispensable preliminary to the production of the magnesian carbonate.

§ 28. To the three principal sources of mineral matters in mineral waters already enumerated, viz., decaying organic matters, decomposing silicates, and the soluble saline matters in rocks, a few other minor ones must be added. One of these is the oxydation of metallic sulphurets, chiefly iron-pyrites, giving rise to sulphate of iron, and more rarely to sulphates of copper, zinc, cobalt and nickel; and by secondary reactions to sulphates of alumina, lime, magnesia, and alkalies. This process of oxydation is necessarily superficial and local, but the soluble sulphates thus formed have probably played a not unimportant part. (§ 9.)

§ 29. Besides these last, which contain chiefly neutral and acid salts, there is another class of waters characterized by the presence of free sulphuric or chlorhydric acid, or both together. These acid waters sometimes occur as products of volcanic action, during which both chlorhydric acid and sulphur are often evolved in large quantities. This latter element generally comes to the surface as sulphuretted hydrogen, which by the

[18] This Journal, [2], xxviii, 180–186, and further, Geol. Survey of Canada, Report for 1859, 214–218.

oxydation of the hydrogen may deposit its sulphur in craters and fissures. In other cases, as shown by Dumas, the sulphur and hydrogen may be slowly and simultaneously oxydized at a low temperature, giving rise directly to sulphuric acid. Not less frequent, however, is probably the direct conversion, by combustion of the sulphuretted hydrogen into water and sulphurous acid, which afterward absorbing oxygen from the air is converted into sulphuric acid.

§ 30. The source of the hydrochloric acid and the sulphur of volcanos is probably the decomposition of chlorids and sulphates at high temperatures. It is known that for the decomposition of earthy chlorids, water and an elevated temperature are sufficient, and at a higher temperature, chlorid of sodium is readily decomposed in presence of siliceous and aluminous minerals, with the intervention of water. Another agency which probably comes into play in volcanic phenomena is that of organic matters, which, reducing the sulphates to sulphurets, enable the sulphur to be subsequently disengaged as sulphuretted hydrogen by the operation of water, either with or without the intervention of carbonic acid or of siliceous and argillaceous matters. Even in cases where this reducing action is excluded, the ignition of sulphates in contact with earthy matters must liberate the sulphuric acid as a mixture of sulphurous acid and oxygen; and these uniting in their distillation upward through the strata, may give rise to springs of sulphuric acid.[16] The reactions similar to those just noticed, involving borates like stassfurthite and hayesine, or boric silicates like tourmaline, etc., are to be ascribed the large amounts of boric acid which are sublimed in some volcanos, or volatilized with the watery vapor of the Tuscan *suffioni*.

§ 31. The action of subterranean heat upon buried strata containing sulphates and chlorids is then sufficient to explain the appearance of hydrochloric and sulphurous acids and sulphur, even without the intervention of organic matters, which are, however, seldom or never wanting; whether as coal, lignite, bitumen, and pyroschists, or in a more divided condition. The presence of hydrogen and of marsh-gas, as observed by Deville among volcanic products, is an evidence of this. The generation of marsh-gas, is, however, in most cases clearly unconnected with volcanic action or subterranean heat.

To the decomposition of carbonates in buried strata by siliceous matters, with the aid of heat, is to be ascribed the great amounts of carbonic acid gas which are in many places evolved from the earth, and impregnating the infiltrating waters, give rise to acidulous springs. The principal sources of this gas in Europe are in regions adjoining volcanos, either active or recently extinct;

[16] See the note to § 22, on kieserite.

but their occurrence in the Paleozoic strata of the United States, far remote from any evidence of volcanic phenomena other than slightly thermal springs, shows that an action too gentle or too deeply-seated to manifest itself in igneous eruptions, may evolve carbonic acid abundantly. The sulphuric-acid springs of Western New York and Canada, to be described further on, are not less remarkable illustrations of the same fact.

§ 32. The frequent presence of ammoniacal salts in volcanic exhalations is here worthy of notice, especially when considered in connection with the rarity of nitric and ammoniacal compounds in natural waters, except in some local conditions, as in the wells of cities, etc., where they are sometimes observed in comparatively large amounts. The explanation of this is evident: for although nitrates themselves are not directly removed from the water, they are by the reducing action of organic matters converted into ammonia, which is retained by the soil. In consequence of this affinity, the argillaceous strata, whether of the present period or of older formations, hold in a very fixed form a considerable quantity of nitrogen. This, from the slowness with which it is eliminated in the form of ammonia under the influence of alkaline solutions, probably exists as an ammoniacal silicate, (§ 6). The action of acids, however, as well as alkalies may be supposed to liberate it from its combination, and thus generate the ammoniacal salts which are such frequent accompaniments of volcanic phenomena. The numerous experiments of Delesse show that ammonia, or at least nitrogen capable of being evolved by heat and alkalies in the form of ammonia, is present in the limestones, marls, argillites, and sandstones of former geological periods, in quantities scarcely inferior to those in similar deposits of modern times, amounting for most of the ancient sedimentary strata to from one to five thousands of nitrogen;[17] from which it will be seen that the amount of this element thus retained in the rocky strata of the earth's crust is very great.[18]

§ 33. If we attempt a chemical classification of natural waters in accordance with the principles laid down in the preceding sections they may be considered under the following heads:

A. Atmospheric waters.
B. Waters impregnated with the soluble products of vegetable decay.
C. Waters impregnated with the salts from decomposing feldspathic rocks, and holding a portion of carbonate of soda as a characteristic ingredient.
D. Waters holding neutral salts of sodium, calcium or magnesium from strata where they existed as solid salts, or as impregnating brines.

[17] Ann. des Mines, [5], xviii, 151-523.

[18] For an exposition of the views put forward in the four preceding sections, see my paper in the Canadian Journal for 1858, p. 206.

E. Waters holding chiefly sulphates from decomposing pyrites; copperas and alum waters.

F. Waters holding free sulphuric or chlorhydric acid.

§ 34. The name of mineral waters is popularly applied only to such as contain sufficient foreign matters to give them a decided taste; and hence the waters of the divisions A and B, and many of the feebler ones of C and D, are excluded. Those of E and F have peculiar local sources; but those of C and D are often associated in adjacent geological formations, and their commingling in various proportions gives rise to mineral waters intermediate in composition. In accordance of these considerations, a classification of mineral waters for technical purposes was adopted by me in *The Geology of Canada*, p. 531, including only those of C, D, and F, which were arranged in six classes.

I. Saline waters containing chlorid of sodium, often with large portions of chlorids of calcium and magnesium, with or without sulphates. The carbonates of lime and magnesia are either wanting, or present only in small quantities. These waters are generally bitter to the taste, and may be designated as brines or bitterns.

II. Saline waters which differ from the last in containing besides the chlorids just mentioned, considerable quantities of carbonates of lime and magnesia. These waters generally contain much smaller proportions of earthy chlorids than the first class, and are hence less bitter to the taste.

III. Saline waters which contain, besides chlorid of sodium and the carbonates of lime and magnesia, a portion of carbonate of soda.

IV. Waters which differ from the last in containing but a small proportion of chlorid of sodium, and in which the carbonate of soda predominates. The waters of this class generally contain much less solid matter than the three previous classes, and have not a very marked taste until evaporated to a small volume, when they will be found, like the last, to be strongly alkaline.

Of these four classes, I corresponds to the division D, and IV to C, while II and III are regarded as resulting from the admixture of these in varying proportions. Sulphates are sometimes present in these waters, but never predominate; in their absence, salts of barium and strontium are often met with. The chlorids are generally, if not always, associated with bromids and iodids. Small quantities of potassium salts are also present, while borates, phosphates, silicates, and small portions of iron, manganese and alumina are generally present. These various waters are occasionally sulphurous, and those of the last three classes may be impregnated with carbonic acid.

V. The fifth class includes acid waters remarkable for containing a large proportion of free sulphuric acid, with sulphates of lime, magnesia, portions of iron, and alumina. These waters, which are characterized by their sour and styptic taste, generally contain some sulphuretted hydrogen.

VI. The sixth class includes some neutral saline waters, in which the sulphates of lime, magnesia, and the alkalies predominate, chlorids being present only in small quantities. These waters, like the last, are often impregnated with sulphuretted hydrogen.

The above classification, although adopted originally for the convenient description of the mineral waters of Canada, will, it is thought, be found to embrace all known classes of natural waters, with the exception of those included under E, and some waters from volcanic sources holding muriatic acid. These may constitute two additional classes. In the first three of the classes above described, chlorids predominate; in the fourth, carbonates; and in the fifth and sixth, sulphates. The waters of the first, second, and sixth classes are neutral; those of the third and fourth, alkaline; and those of the fifth acid.

The results of the chemical analysis of various waters of these classes it is proposed to give in the second part of this paper.

Art. XXIII.—*Abstract of a Memoir on Shooting Stars;* by H. A. Newton.

(Read before the National Academy of Sciences, August, 6th, 1864.)[1]

1. The periodical shooting stars, particularly those of August and November, have hitherto very naturally attracted more attention than the sporadic ones, those seen on every clear night throughout the year. Yet these latter are objects of no small interest. There are methods of observing, and of computation, by which much can be learned about them, and observations already made are of use in determining something of their numbers, and of their place in the Solar System. I propose to combine these existing materials, and see to what they lead us. If it shall appear that I use rude processes and inexact data, and reject terms of considerable importance, I must plead that it is a step forward to do anything in this direction, and express the hope that better data will soon warrant the use of more refined processes. It will be necessary to assume certain propositions which are not probably strictly true, and others which may not be universally conceded. Such assumptions are however very distinctly set forth, that future observations may correct them if erroneous, and verify them if true. The details of many of the arguments are necessarily omitted in this abstract.

The table of altitudes of the paths of shooting stars published in the last volume of this Journal, p. 135, is the basis of the computations. Singly, these altitudes are liable to large probable

[1] From the Proceedings of the Academy: published by permission.

errors; yet, taken together and used with caution, it is believed that they may be safely employed in a first approximation.

2. *Distribution vertically of the middle points of the luminous portions of the meteor-paths.*—From the table named, by rejecting the manifestly unreliable cases, and giving weight to the others, the following numbers are obtained to express the relative numbers of the shooting stars at different altitudes above the earth's surface. By altitude is meant the altitude of the middle point of the visible part of the path expressed in kilometers.

Between	0	and	30	kil.	39	sh. stars,	Between	150	and 180 kil.	57 sh. stars.
"	30	"	60		114	"	"	180	" 210	20 "
"	60	"	90		243	"	"	210	" 240	20 "
"	90	"	120		277	"	"	240	" 270	8 "
"	120	"	150		106	"	Above	270 kil.		12 "

There is evidently a definite upper limit to the region of meteor-paths. Again, the best observations give few if any very low altitudes. The altitudes less than 30 kilometers, and those greater than 180, are therefore disregarded.

I assume that these observed paths are fair examples as to altitude of all visible paths, and hence that the frequency of the middle points of all visible paths at different altitudes above the earth's surface is proportional to, and may be expressed by, these numbers, viz:

From	30	kilometers	to	60	by	114
"	60	"	"	90	"	243
"	90	"	"	120	"	277
"	120	"	"	150	"	106
"	150	"	"	180	"	57

Representing these numbers by ϱ, and the mean altitude of the paths by d, we have approximately,

$$d = \frac{\Sigma \varrho x}{\Sigma \varrho},$$

where in the finite summation indicated by Σ the successive values of ϱ are to be taken, and x is to be successively $\frac{1}{2}(30+60)$, $\frac{1}{2}(60+90)$, $\frac{1}{2}(90+120)$, &c. The value of d, that is, the mean altitude of meteor paths above the earth's surface, is thus found to be 95·55 kilometers, or not quite 60 miles.

3. *Distribution of the paths of Shooting Stars over the apparent heavens in azimuth.*—Theoretical considerations seem to require that the meteors should be nearly uniformly distributed in azimuth. Observations confirm this. Collating the observations of Mr. Herrick and others, as published in this Journal, and preserving those only in which the heavens were divided into four parts, we have of 6,598 observed paths, 800 in N., 733 in N.E., 965 in E., 852 in S.E., 847 in S., 833 in S.W., 889 in W., and 679 in N.W. These numbers imply a small predominance toward the southeast. Since the average zenith distance of the

middle points of paths is about 48°·3, the above numbers give as a center of gravity, or center of distribution, a point about 2° from the zenith in the direction of S. 28° E. We therefore conclude that the relative frequency of occurrence of meteor-paths in different parts of the visible heavens is in the main *a function of zenith distance only.*

4. *Distribution of meteor-paths over the sky in altitude.*—The law distribution of the paths in apparent altitude may be obtained directly by observations properly devised for that object. But such observations would have to be continued for a considerable time, and would involve great labor. I have therefore sought for a method of obtaining the approximate law of distribution from observations already made for other purposes.

If the zenith distances of a large number of paths seen by one observer were measured or computed, we should find them affected by his habits of watching. Thus, one who looked habitually to the zenith would see only those near that point, while one looking low down would see few near the zenith. But, combining the observations of a large number of persons, we might hope that many of these individual habitudes would counteract each other, and that the aggregate results would be affected only by common errors. I have therefore taken for this purpose various sets of observations made by about forty different persons. For a part the distances of the middle points of several paths from the zenith for the instant of appearance were computed. For the remainder the place of the zenith was in each case computed, and the zenith distance carefully measured on a very good sixteen-inch globe. The number of paths thus computed or measured was 1,393. Of these, 30 were within 10° of the zenith, 60 were between 10° and 20° from the zenith, 142 between 20° and 30°, &c., as in the second column of the following table:—

Table illustrating the distribution of meteor-paths over the sky.

Zenith distance.	No. of meteors.	Area of sky.	$\frac{\text{No.}}{\text{Area.}}$	$\sec^3\theta$.	$\frac{\text{No.}}{\text{Area } \sec^3\theta}$.
° °					
0–10	30	·01519	1975	1·012	1951
10–20	60	·04512	1330	1·110	1198
20–30	142	·07366	1928	1·343	1436
30–40	197	·09999	1970	1·819	1083
40–50	274	·12325	2223	2·828	786
50–60	304	.14279	2129	5·299	402
60–70	245	·15798	1551	13·25	117
70–80	110	·16837	653	57·68	11
80–90	31	·17365	178	1510	0

If the area of the visible hemisphere is unity, the numbers in the third column give the areas of the corresponding zones. The number of paths divided by the areas gives quotients pro-

portional to the numbers of stars at different altitudes to any unit of surface. These form the fourth column. They increase slowly to about 45°, and then rapidly diminish to the horizon.

The number of paths along an oblique line is greater than that along a perpendicular line. They should be very nearly as the cubes of the lengths of the lines, or, disregarding the curvature of the earth, as $\sec^3\theta : 1$. The cubes of the secants of 5°, 15°, 25°, &c., are given in the fifth column, these angles being taken as the mean zenith distances of the zones. In the sixth column are the quotients of the numbers in the fourth column divided by those in the fifth.

Under any reasonable law the numbers in the last column should diminish from the zenith. The deviations from this uniform diminution that appear in the table are due in part to the small number of paths measured. The first number represents a small area, and should perhaps be combined with the second. Upon the whole, there seems to be a deficiency near the zenith, which may be due to the tendency of observers to look habitually towards points from 30° to 60° high.

From inspection of this column, and from considerations detailed in the memoir, 1,800 is thought to be a reasonable number to assume as that to which the series approaches. The product $1{,}800 \times \sec^3 5° \times \cdot01519 = 27{\cdot}67$, would then give the number of shooting stars out of 1,393 that should be seen within 10° of the zenith. This is equivalent to one in 50·35. That is, *about one in fifty of all shooting stars seen at a place should have the middle points of their apparent paths within* 10° *of the zenith.*

5. *Number of shooting stars that come into the atmosphere each day.*—Shooting stars are seen in all countries, and any differences of number for different countries, thus far detected, may be easily explained by the personal equations of observers, or by differences in the clearness of the atmosphere. It will be assumed that for a given *considerable* period of time the meteors are equally abundant over all parts of the earth's surface.

Their frequency at different altitudes from the surface of the earth varies. If we suppose ϱ_1 to be the number of middle points of visible paths that fall in a given period of time into any cubic unit of the space of the region of meteor-paths, we should have ϱ_1 a function of the altitude above the earth's surface. Let x represent the altitude, and R the earth's radius. Suppose now an inverted cone, whose vertex is at the eye of the observer, whose axis is a vertical line, and whose semi-vertical angle is 10°. In general, shooting stars which have the middle points of luminous parts of their trajectories in this cone will have the middle of their apparent paths within 10° of the zenith. The number in this cone in the given time will be expressed very nearly by the formula,

$$\int_a^b \pi\varrho_1 \tan^2 10° x^2 dx,$$

where a and b are the values of x for the lower and upper surfaces of the region of meteors.

On the other hand, the total number of shooting stars within the given period over the whole earth will be equal to

$$\int_a^b 4\pi\varrho_1 (R+x)^2 dx.$$

The whole number visible at one place is 50·35 times the number seen within 10° of the zenith, and therefore 50·35 times the number within the above described cone. Hence if m is the number in a given period visible at one place, and N the number that would be visible (except for daylight, clouds, moon, &c.,) through the whole earth in the same period, we should have,

$$N = \frac{m}{50·35} \cdot \frac{\int_a^b 4\pi\varrho_1 (R+x)^2 dx}{\int_a^b \pi\varrho_1 \tan^2 10° x^2 dx}.$$

Now ϱ_1 is a function of x, and, assuming as heretofore that the observed altitudes are fair examples of the real altitudes, we shall have $\varrho_1 = k\varrho$, where k is a constant depending on the period of time assumed, the unit of space assumed, and the abundance of meteors. As we have assumed an equable distribution over the earth's surface, this constant may be removed from the integral sign. Again, without great error finite summation may take the place of integration, in which case the equation easily reduces to the form,

$$N = 2·555\, m \left\{ \frac{\Sigma_a^b \varrho x^2 + 2R\Sigma_a^b \varrho x + R^2 \Sigma_a^b \varrho}{\Sigma_a^b \varrho x^2} \right\}.$$

Taking x successively 45, 75, 105, 135, and 165 kilometers, and ϱ also 114, 243, 277, 106, and 57, and R equal to 6370, we find

$$N = 10{,}460\, m,$$

that is, *the number over the whole earth is to be considered as* 10,460 *times the number visible at one place.*

To obtain this result, it was assumed that the shooting stars were uniformly distributed over the earth's surface, and that the conditions of visibility are uniform. If, however, we regard the actual instead of the theoretical case, we find that the numbers seen vary through the hours of the night. The rapid diminution toward the horizon, already shown, indicates the great influence of mists, &c., in the air, in absorbing the light of these bodies. But for this cause, many more could be seen within ten

degrees of the horizon than in the whole of the rest of the heavens; whereas of 1393 only 31 were actually seen in this part of the sky. These mists must of course vary in different climates. Hence the numbers visible in different places may reasonably be expected to differ. Let, however, a locality have an atmosphere of mean purity, let it in other respects be one of medium character with respect to the number of visible meteor-paths, and let m be the mean hourly number of shooting stars seen in a clear sky at that place: then we may consider that the whole number that under clear skies could be seen over the earth in one hour would be 10,460 m.

The value of m is of course to be determined by observation. The hourly number varies through the night. The value of m may be found from observations extending through the night, or from those made near midnight.

Mr. E. Bouvard, in the year between Oct., 1840, and Oct., 1841, observed at Paris on every clear moonless night. (*Comptes Rendus*, xiii, 1029). He always watched between 11 and 1 o'clock. During 74 hours and 22 minutes he saw 572 shooting stars. Allowing one-fourth of a minute for recording each meteor (which is the time estimated by him), and we have an average of 8 meteors per hour.

By what factor we must multiply the number seen by one observer to obtain the whole number visible at the place, we have no observations that I know of to determine. It is probable that this multiplier is as large as four, and that thirty is therefore not too large for the mean value of m.

This would give the average number of meteors that traverse the atmosphere daily, and that are large enough to be visible by the naked eye, if the sun, moon and clouds would permit, equal to $30 \times 24 \times 10{,}460$, or *more than seven and a half millions.*

6. It will now be assumed that the phenomenon called a shooting or falling star is caused by a small body (probably a solid) which originally was moving in its own orbit in the solar system, or in space; that this body coming into the atmosphere of the earth elicits light by the loss of velocity, and is usually itself dissipated before reaching the earth's surface. The term *meteoroid* will be used to designate such a body before it enters the earth's atmosphere.

7. *Number of meteoroids in the space which the earth traverses.*— Suppose many small bodies to be distributed through an indefinite space, so that there shall be n bodies in a cubic unit. Suppose that these bodies have all an uniform velocity of v units per second in the same direction. Suppose a larger sphere whose radius is R, and which is without attraction, to be at rest in this space. The sphere intercepts in each second as many small bodies as are contained in a right cylinder, whose length is v, and whose radius is R, that is $\pi R^2 nv$ bodies.

If the sphere attracts the small bodies, it may be shown that instead of $\pi R^2 nv$ we should have $\pi R^2 nv\left(1+\frac{v_0^2}{v^2}\right)$, when v_0 is the velocity of a particle falling by the attraction of the sphere from rest at infinity, to the surface of the sphere. If the sphere has an uniform motion the same formula applies by making v the velocity relative to the sphere.

This result may be extended to several systems of small bodies. Let there be distributed through the indefinite space, in each unit of it, n' bodies of one system, n'' bodies of a second system, n''' bodies of a third system, and so forth, and let the bodies of the first system move in one direction with a velocity v' relative to the sphere, let the bodies of the second system move in another direction with a velocity v'' relative to the sphere, and so forth, then will the number of bodies which the sphere intercepts in each second be equal to

$$\pi R^2 n'v'\left(1+\frac{v_0^2}{v'^2}\right)+\pi R^2 n''v''\left(1+\frac{v_0^2}{v''^2}\right)+\pi R^2 n'''v'''\left(1+\frac{v_0^2}{v'''^2}\right)+\text{ etc.}$$

Call this N′ and we may write,

$$N'=\pi R^2\left(\Sigma n'v'+v_0^2\Sigma\frac{n'}{v'}\right),$$

where the summation indicated by Σ extends to all the systems of bodies.

If v is the mean value of v', v'', &c., for all the bodies and n is the sum of $n'+n''+n'''+$&c., then

$$\Sigma n'v'=nv.$$

The remaining term is the sum of fractions whose denominators vary. We may, however, write,

$$v_0^2\Sigma\frac{n'}{v'}=\frac{nv_0^2}{v}(1+\theta),$$

where θ is a number and is evidently positive. If the values of v', v'', v''', &c., do not vary widely, θ will be small. Making these substitutions, we have

$$N'=\frac{\pi nR^2}{v}(v^2+v_0^2+\theta v_0^2).$$

This formula expresses the number of meteoroids which the earth intercepts by considering the earth with its atmosphere as the supposed sphere, R its radius measured from the upper part of the region of meteor-paths, v the average velocity of the meteoroids when they come into the region of the earth's sensible attraction, v_0 the velocity of a body falling by the earth's attraction from infinity to a distance R from the centre of the earth, N′ the average number of meteoroids coming into the

earth's atmosphere in a second, and n the average number in a cubic unit of the space the earth is traversing in a given period.

If m be the average number visible at one place in a unit of time we have found above that $N=10{,}460\,m$. The volume of a sphere whose radius is R is $\frac{4}{3}\pi R^3$. Let M be the number of meteoroids in a space equal to such a sphere, then $M=\frac{4}{3}\pi n R^3$, and

$$10460\,m=\tfrac{3}{4}\frac{M}{vR}(v^2+v_0^2+\theta v_0^2),$$

or

$$M=\tfrac{4}{3}\cdot\frac{10460\,m\,Rv}{v^2+v_0^2+\theta v_0^2},$$

where m denotes the average number (or fraction of a number) seen in one place per second. If the hourly number is, as before assumed, equal to 30, then $m=\frac{1}{120}$, and $M=116{\cdot}2\,\frac{Rv}{v^2+v_0^2(1+\theta)}$.

8. *The mean length of apparent paths.*—I have computed or measured the lengths of 1016 paths, observed by a large number of persons. The mean length is found to be 12°·6.

9. *Numbers of Telescopic shooting stars.*—Shooting stars are of all degrees of brilliancy, and there are many very faint ones. Almost every hour that a person watches, he sees, or thinks he sees, flights that are yet so faint as to leave him in doubt whether they are shooting stars, or only illusions. We may therefore reasonably conclude that large numbers of shooting-stars are entirely invisible to the naked eye, which yet might be seen by the telescope.

This conclusion is verified by observation. In 1854, Messrs. Pape and Winnecke observed[1] together at Göttingen for 32 hours, on nights between the 24th of July and the 3d of August. Pape saw with the naked eye 312 shooting stars, and Winnecke saw 45 in the same time with a comet seeker. The diameter of the field of view is not given, but in observations at the same time diameters of 53′ and 36′, with powers of 30 and 60, were used.

If the apparent length of a meteor-path is l and the breadth of the field of view of a telescope is b, and if the axis of the telescope is directed towards any part of the area whose length is l, and whose breadth is b, the meteor-path would cross the field of view. If all paths were of the same length, b, and were equally distributed over the heavens, a telescope would then command a portion of the whole heavens expressed by the fraction $lb \div$ *surface of sky*. Meteor-paths diminish somewhat in length with apparent brightness. On the other hand a path may perhaps be longer when viewed by aid of the telescope than when seen by the naked eye. Hence for the approximate mean value of l may be taken 12°·6, the mean value of the length of the apparent meteor-paths visible to the naked eye. Let b be 53′

[1] Astron. Nachrichten, xxxix, 113.

and the ratio of those actually seen through one telescope to those which are bright enough to be visible in it is $53 \times 12°{\cdot}6 : 360 \times 60 \times 180 \div \pi$, or, 1:1853. I have selected the larger diameter of the telescope that the ratio may not be too large. For the same reason I prefer to reject in the divisor that part of the surface of the heavens within 15° of the horizon. This makes the ratio $1 \div 1371$.

We have seen that according to Bouvard's observations one person should see an average of eight shooting stars per hour. Hence if $\frac{45}{312}$ is taken as the ratio of the number seen in a comet seeker to those seen by one person with the naked eye, there should be in each hour $\frac{8 \times 45 \times 1371}{312}$, or 1582 shooting stars hourly that might be visible through a comet seeker if the whole heavens could be watched. The ratio between those visible at one place and those visible somewhere over the whole earth is $1 \div 10{,}460$ for common meteors. If the same ratio applies to telescopic shooting stars (doubtless not a very reasonable supposition), we have for the whole number of meteoroids coming daily into the air, at least, $1582 \times 24 \times 10460$, or more than 400,000,000. There is, moreover, no reason to doubt that a farther increase of optical power would reveal still larger numbers of these small bodies.

10. *Mean distance of the meteor-paths from an observer.*—Although the data in our possession are inadequate to furnish the mean distance of the meteor-paths from an observer, yet limits to this mean distance can be obtained. Not so large a portion of the meteor-paths along an oblique line can be seen as along a vertical line. Now from the distribution of the paths in absolute altitude, and their distribution over the apparent heavens, can be computed the mean distance on either of two suppositions:

1st, That those paths along the oblique line that become invisible are always the most distant ones;

2d, That the paths which become invisible are distributed along the oblique line in proportion to the numbers along the line.

It is evident that the first supposition affords a mean distance less than the truth, and that the second affords one that is too great. These two limits are found to be about 232 and 140 kilometers. It must, however, be admitted that in this computation the unavoidable errors arising from using finite summation for integration, and from other sources, are considerable.

11. *Mean foreshortening of the meteor-paths by perspective.*—To determine the effect of perspective in shortening the apparent paths we have need to use the following geometrical proposition:

If a sphere whose radius is a be supposed to have an indefinite number of diameters, and the extremities of these diameters are

would be not much greater than that of a body falling to the earth from an infinite distance—that is, not much greater than 11 kilometers per second. So small a mean velocity is entirely inconsistent with direct observations, as well as the conclusions given above.

We might, it is true, suppose the ring to have a considerable breadth, in which case the meteors would have a larger mean relative velocity. But if the breadth be such as to furnish a velocity at all consistent with observation, we have no longer a ring lying between the orbits of Mars and Venus, but a disk extending much beyond these planets.

15. *A large portion of the meteoroids must, when they meet the earth, have absolute velocities greater than the earth's velocity in its orbit; or else, the sporadic meteors have a series of radiants at some distance from the ecliptic, and hence come from a series of rings considerably inclined to the earth's orbit.*—For shooting stars cannot have relative motions upward from the earth's surface when they enter the atmosphere. If then the absolute velocities of the meteoroids were all less than those of the earth, their relative motions (neglecting the earth's attractions,) would all be from points of the heavens less than 90° from that point toward which the earth is moving. To an observer early in the evening but a small part of this hemisphere is above the horizon, while in the morning almost all of the hemisphere is visible. Hence, either the number of those seen in the morning should be *very much greater* than that of those seen in the evening, or else there should be a radiant in that part of the sky which is above the horizon in the earlier hours of the night. If the earth's attraction be considered the disproportion between the morning and evening hours would be slightly diminished. But even with this allowance the increase through the night would be greater than the observed increase, unless one of the two suppositions above given is true.

16. *Distribution of the orbits of the meteoroids in the solar system.*—There are at least three suppositions respecting the distribution of the orbits of the meteoroids in the solar system which are naturally suggested. Either of them may be considered as plausible, and one does not exclude another.

1st. They may form a number of rings, like the August group, cutting or passing near the earth's orbit at many points along its circuit. The sporadic shooting stars may be outliers of such rings.

2d, They may form a disk in or near the plane of the orbits of the planets.

3d. They may be distributed at random, like the orbits of the comets.

According to the first of these suppositions there should be a succession of radiants corresponding to the several rings. Dr.

Heis and Mr. R. P. Greg believe that they have detected such a series. Continued observation directed to this end will probably decide whether the meteoroids belong entirely, or mostly, to rings.

According to the second supposition the apparent paths of the sporadic meteors should, if produced in the direction of the motion, all cut the ecliptic below the horizon. For the absolute motion of a meteoroid and the earth's motion being both in, or near, the ecliptic, the resultant relative motion should be from some point in the ecliptic. That point should be above the horizon, since the body must move downward to enter the atmosphere. And as the apparent path produced backward cuts this point, it will, if produced forward, cut the ecliptic below the horizon.

We have thus a simple means of determining whether the sporadic meteors come exclusively, or even largely, from a disk or a lenticular shaped group about the sun, like that which the zodiacal light is often supposed to indicate.

If the third supposition is true, the mean velocity of the meteors is a function of the numbers of shooting stars in the different hours of the night. For, if the velocity is very small, few would be seen in the evening, while if it is very large, there should be nearly as many in the evening as in the morning.

Consider a small spherical space in the upper region of the earth's atmosphere as receiving the meteors.

By supposition the absolute motions of the meteoroids are directed in equal numbers from all parts of the celestial sphere. If their velocities are all equal and represented by v', the earth's velocity being v, if N_0 represents the numbers of those whose absolute motions in a given period are from the visible celestial hemisphere, n the whole number that should come from all parts of the celestial sphere, and θ the distance from the zenith to that point of the heavens to which the earth is moving, then we find between these quantities the equation,[3]

$$N_0 = \frac{n}{2}\left(1+\frac{v}{v'}\cos\alpha\right).$$

Let l be the latitude of an observer, and h the hour-angle counting from the time when the point toward which the earth is moving is on the meridian, then when that point is on the equator we have $\cos\alpha = \cos l \cos h$. This will correspond to the mean of the year. Computing now the values of $N_0 \div n$, on the several suppositions $v' = v\sqrt{2}$, $v' = v$, and $v' = \frac{4}{5}v$, for the latitudes of New Haven, and of Paris we have the following table. When $v' = v$ we have orbits whose major axes equal those of the earth. When $v' = v\sqrt{2}$ we have parabolic orbits.

[3] After reading this memoir, I found that Mr. A. S. Herschel had obtained the same formula.

Table showing the hypothetical distribution of the shooting stars through the hours of the night.

Hour of the night.	New Haven.			Paris.		
	$v'=v\sqrt{2}$	$v'=v$	$v'=8v$	$v'=v\sqrt{2}$	$v'=v$	$v'=8v$
6	·765	·875	·970	·732	·829	·911
7	·756	·862	·954	·725	·818	·897
8	·730	·825	·907	·701	·785	·856
9	·687	·765	·832	·664	·732	·791
10	·633	·688	·735	·616	·665	·706
11	·569	·597	·622	·560	·585	·606
12	·500	·500	·500	·500	·500	·500
1	·431	·403	·378	·440	·415	·394
2	·367	·312	·265	·384	·335	·294
3	·313	·235	·168	·336	·268	·209
4	·270	·175	·093	·299	·215	·144
5	·244	·138	·046	·275	·182	·103
6	·235	·125	·030	·268	·171	·089

The velocities have been considered as uniform. But it is evident that if the shooting stars have various velocities, the hourly distribution should correspond approximately to that for the mean velocity.

The number entering a spherical space has been considered. In fact we have to consider the number entering the region of the atmosphere commanded by observers at one place. The two are not identical. For if so, we ought, on the nights of 9–12th of August to have the conformable meteors equally distributed through those hours of the night in which the radiant is above the horizon. But there is in fact an increase of numbers till dawn. Hence the difference between the numbers seen in morning hours, and those seen in the evening hours, should be greater than is given by the preceding table.

On the other hand, the effect of the earth's attraction is to render the numbers a little more uniform through the night. Again, there should be less difference between the morning and evening during the half year from July to December, than in the other half year, for the point to which the earth is then moving is north of the equator.

Mr. Herrick estimated that there were about three times as many shooting stars in the morning as in the evening. Mr. Coulvier-Gravier gives the following[4] as the mean hourly numbers of shooting stars at Paris:—

5^h—6^h ..... 7·2	10^h—11^h ..... 8·0	3^h—4^h ..... 15·6
6 —7 ..... 6·5	11 —12 ..... 9·5	4 —5 ..... 13·8
7 —8 ..... 7·0	12 — 1 ..... 10·7	5 —6 ..... 13·7
8 —9 ..... 6·3	1 — 2 ..... 13·1	6 —7 ..... 13·0
9 —10 ..... 7·9	2 — 3 ..... 16·8	

How much reliance is to be placed on these numbers of the Paris observer we know not.

[4] Recherches sur les météores, p. 220.

This estimate, and these numbers, taken strictly imply a mean velocity greater than that of a parabolic orbit. The character of the data does not allow the argument to be pressed. Yet we must regard as almost certain (on the hypothesis of an equable distribution of the directions of absolute motions), that the mean velocity of the meteoroids exceeds considerably that of the earth; that the orbits are not approximately circular, but resemble more the orbits of the comets.

17. *Number of meteoroids in the space which the earth is traversing.*—We have found that, of the space through which the earth is moving, a volume equal to that of the earth (atmosphere included), contains a mean number of meteoroids expressed by the equation

$$M = 116{\cdot}2\,\frac{Rv}{v^2 + v_0^2(1+\theta)}.$$

In this equation v is the mean relative velocity of the meteoroids. If their absolute velocities were equal, and the points from which they come uniformly distributed over the heavens, we should evidently have

$$v = \frac{1}{2}\int_0^{\pi} (v^2 + v'^2 - 2vv'\cos\omega)^{\frac{1}{2}} \sin\omega d\omega = v + \frac{v'^2}{3v}.$$

For v' we may use as an approximation the mean absolute velocity. If $v = v'$, then $v = \frac{4}{3}v$. If $v' = v\sqrt{2}$, then $v = \frac{5}{3}v$. As the velocity seems to be greater than that of the earth, and more nearly equal to that of bodies moving in parabolic orbits, the latter value is preferred for v. On this supposition, and by neglecting θ, we find M greater than 14,000. If allowance be made for the space occupied by the earth's atmosphere we find that, in the mean, *in each volume of the size of the earth, of the space which the earth is traversing in its orbit about the sun, there are as many as* 13,000 *small bodies, each body such as would furnish a shooting star visible under favorable circumstances to the naked eye.* If telescopic meteors be counted, this number should be increased at least forty-fold.

There seems to be little reason for supposing that the space near the earth's orbit is very much more thickly strewn with these bodies than other parts of the solar system. That they are grouped according to some law, is altogether probable. But a velocity different from that of the earth implies, of necessity, that they are not grouped closely about the earth's orbit.

These bodies cannot be regarded as the fragments of former worlds. They are rather the materials from which the worlds are forming. If astronomy furnishes any measure of their total mass we may therefrom obtain some idea, rude though it be, of the mean mass of the individuals.

ART. XXIV.—*Action of Binoxyd of Lead and Sulphuric Acid on Hippuric Acid;* by Dr. JULIUS MAIER, Chemical Assistant, School of Mines, Columbia College.

Hipparaffin, $\mathrm{C_8H_7N\Theta}$.

HIPPURIC acid, at a moderate temperature, is treated with binoxyd of lead and sulphuric acid, care being taken to avoid an excess of either. A strong evolution of carbonic acid ensues, and the mass congeals to a thick paste. This is placed on a filter, and washed with a dilute soda solution till the wash-water shows no further reaction of sulphuric acid. This mass is treated with alcohol, which dissolves the hipparaffin, and evaporated in a water-bath to one-half its volume. On cooling, the entire liquid congeals to a crystalline mass. The separated hipparaffin is collected on a filter, and washed with boiling water, until all acid reaction has disappeared, and is then redissolved in hot alcohol, and allowed to crystallize. Hipparaffin forms a network of white silk-like needles, which melt at 210° C., subliming previously at 100° C. They are entirely insoluble in water, that reagent not even causing them to become moist; they are readily soluble in alcohol and ether. Hipparaffin owes its name to the fact of its not being perceptibly decomposed by acids or alkalies. It is not soluble in solutions of potassa, and develops ammonia only after continued fusing with caustic potassa; it is easily dissolved by a mixture of sulphuric and nitric acids, and is not separated from this solution by water. Fuming nitric acid dissolves it with liberation of gas. This solution, after neutralization with carbonate of soda and evaporation, yields benzoic acid on addition of chlorhydric acid. Upon being heated in a stream of chlorhydric acid gas, a colorless oil passes over, which congeals to a crystalline mass. Schwarz,[1] who first described hipparaffin gave it the incorrect formula $\mathrm{C_{16}H_8NO_2}$. I found its composition to be $\mathrm{C_8H_7N\Theta}$, the formation of which is explained as follows:

Hippuric acid.	Binoxyd lead.	Sulphuric acid.	Hipparaffin.	Carbonic acid.	Sulphate of lead.	Water.

$$\mathrm{C_9H_9N\Theta_3+Pb_2\Theta_2+S_2H_2\Theta_4=C_8H_7N\Theta+CH_2\Theta_3+S_2Pb_2\Theta_4+H_2\Theta}$$

Hipparin, $\mathrm{C_8H_9N\Theta_2}$.

When an excess of sulphuric acid is used in the preparation of hipparaffin, an oily liquid is separated which rises to the surface of the alcoholic solution, and on cooling congeals to a crystalline mass, on the addition of water. This mass dissolves in hot water, and crystallizes in long, white, glassy needles, grouped in the shape of fans. These needles melt at 49° C., and become

[1] Schwarz Annal., lxxv, 201.

solid at 20° C. They are soluble in hot water, alcohol and ether. Their formation may be explained as follows:

Hippuric acid. Binoxyd of lead. Sulphuric acid. Hipparin. Carbonic acid. Sulphate of lead.

$$C_9H_9NO_3+Pb_2O_2+S_2H_2O_4=C_8H_9NO_2+CH_2O_3+S_2Pb_2O_4$$

Hippuric and Benzoic acids are entirely decomposed by heating them with an excess of superoxyd of lead and sulphuric acid, all carbon being driven off in the form of carbonic acid.

The following are the products of the decomposition of hippuric acid by binoxyd of lead and sulphuric acid:

1. $C_9H_9NO_3+Pb_2O_2+S_2H_2O_4=C_8H_9NO_2+CH_2O_3+S_2Pb_2O_4$
2. $C_9H_9NO_3+Pb_2O_2+S_2H_2O_4=C_8H_7NO+CH_2O_3+S_2Pb_2O_4+H_2O$
3. $C_9H_9NO_3+Pb_6O_6+S_6H_6O_{12}=C_7H_7NO+C_2H_4O_6+S_6Pb_6O_{12}+H_4O_2$

Hippuric acid. Binoxyd lead. Sulphuric acid. Benzamid. Carbonic acid. Sulphate of lead. Water.

Action of Bromine and Iodine on Hippuric Acid.

Bromohippuric acid, $C_9H_8BrNO_3$.

An alcoholic solution of hippuric acid, at a boiling heat, is treated with bromine; the solution is allowed to boil a few minutes and water added, and then evaporated on a water-bath to one-half its volume. The yellowish brown color of the solution gradually disappears, and, on cooling, a white crystalline deposit is formed, which consists of fine needles plainly visible under the microscope. These needles are difficultly soluble in water, alcohol or ether, when cold, but readily soluble in all of these media upon boiling. They are colorless; when moistened and allowed to stand for a short time at an ordinary temperature, they undergo decomposition and turn brown, owing to the separation of bromine. The aqueous solution has a strongly acid reaction, and is not precipitated by lime, baryta, or the salts of copper. Upon being heated with caustic lime, the crystals emit a distinct odor of benzonitril, and a violet colored oily liquid was formed, previous to which benzoic acid having a red color passed over. The crystals dried in vacuo and burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2735 grms. gave 0·4160 grms. carbonic acid and 0·083 grms. water.
0·2760 grms. burned with caustic lime, gave 0·1995 grms. bromid of silver.
0·6855 grms. burned with soda-lime, gave 0·0405 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	41·86	41·50	....	....
8. Hydrogen,	8	3·10	3·40	....	....
1. Bromine,	80	31·00	....	30·91	....
1. Nitrogen,	14	5·43	....	....	4·86
3. Oxygen,	48	18·61	....	....	....
		100·00			

The salts of potash and soda could not be obtained in crystals. The salt of lime, $C_9H_7CaBrNO_3$, crystallizes in fine white needles, is difficultly soluble in cold water, readily so in hot water; dried between 90° and 100° C. it was not decomposed. The analysis gave the following result: 0·3835 grms. of the dried crystals gave 0·0665 grms. carbonate of lime, the formula as calculated requiring 0·0692 grams.

Iodohippuric acid, $C_9H_8INO_3$.

This acid was obtained in the same manner as the bromine compound, employing iodine instead of bromine. To the alcoholic solution water was added, and it was filtered from the separated iodine; the filtrate was allowed to stand over night. The solution which was at first brown became colorless, and fine needles were deposited. These are white, odorless, difficultly soluble in water, alcohol and ether, in the cold, but readily so in all of them upon boiling. They are decomposed at 90° C., turning yellow, owing to the separation of iodine. All the salts with the exception of that of silver are soluble in water.

The crystals, when burned with oxyd of copper, and finally in a stream of oxygen, gave the following results:

0·2430 grms. gave 0·3080 grms. carbonic acid and 0·0605 grms. water.
0·2080 grms. gave 0·1590 grms. iodid of silver, when burned with caustic lime.
0·4760 grms. burned with soda-lime gave 0·0240 grms. ammonia.

		Calculated.	Found.		
			I.	II.	III.
9. Carbon,	108	35·41	34·57	....	....
8. Hydrogen,	8	2·62	2·76	....	....
1. Iodine,	127	41·64	....	41·30	....
1. Nitrogen,	14	4·59	....	....	4·16
3. Oxygen,	48	15·74	....	....	....
		100·00			

New York, Dec. 1st, 1864.

ART. XXV.—*On the Preparation of Oxalate of Ethyl;* by M. CAREY LEA, Philadelphia.

THE processes described in the text books for the preparation of oxalate of ethyl are liable to objection. If we heat binoxalate of potash with sulphuric acid and alcohol, we introduce into our distillate the decomposition-products of sulphuric acid in addition to those of oxalic acid and alcohol. It has also been recommended[1] to heat oxalic acid in a retort until it begins to

[1] Gmelin Handbook, Cav. ed., ix, 179.

emit white fumes, and then allow alcohol to drop in through the tubulure. As the temperature at which the acid begins to sublime is considerably below that at which it melts, this process is apt to result in breaking the retort.

I was therefore led to seek for a better process, and have used the following repeatedly, with very satisfactory results.

A tube of an inch or more in diameter, and of a proper length, is drawn out at one extremity, and at the same time bent to an angle, somewhat as in the ordinary form of a combustion tube, except that the bent part is made less slender, and shorter ($\frac{1}{4}$ inch diameter at extremity). The tube is then completely filled with dehydrated oxalic acid, except for the last two inches at the open end. This is closed with a cork penetrated by a tube. For this purpose the cork is not bored parallel with its axis, but in an inclined direction, so that the smaller tube when inserted two or three inches may touch the side of the larger. This last is then supported in a horizontal position over a gas combustion furnace, so that the bent end may be directed upwards, and the cork turned so that the small tube may touch the upper side of the interior of the large one. The tube should be supported about six inches above the wire gauze of the furnace. The bent end of the tube is now connected with a Liebig's condenser, and the small tube with a flask containing alcohol of 42 B. The furnace is then lighted, and as soon as the acid melts, a stream of alcohol vapor is sent over it by heating the flask. The gas-furnace is maintained at a heat such that a very gentle ebullition shall be kept up in the tube. At the end of an hour to an hour and a quarter the operation is terminated, and the oxalic acid in the tube is converted into oxalic ether. The distillate also contains a little oxalic ether which cannot be separated by water, but which may be used for preparing oxamid. Or the distillate may be gently evaporated to one-tenth its bulk, and then may be mixed with a little water.

This process gives an abundant product, and requires no attention after the proper temperature is once adjusted. In filling the tube with the oxalic acid, it is not necessary to leave an air passage; the material settles in fusing sufficiently without. The oxalate of ethyl, which is a little brownish, and contains the usual impurities of the crude product, may then be purified in the ordinary manner.

ART. XXVI.—*Method of applying the binocular principle, to the eye-piece of a Microscope or Telescope;* by ROBERT B. TOLLES.

To apply the binocular principle to the eye-piece of a microscope or telescope, it is only necessary to make use of the erecting form of eye-piece, and to place the dividing prism at the point where the pencils composing the whole bundle of rays proceeding from the object cross in the eye-piece, which is the point where, in any erecting eye-piece, the diaphragm proper is correctly placed.

If the theory of the erecting eye-piece of common form were generally understood, no demonstration that binocularity can be given to such an eye-piece would be necessary.

Suffice it to say that, since any one pencil of light proceeding from any point of the object through the *whole area* of the object-glass *does* at this point equally fill the whole area of the diaphragm (that being of proper aperture) substantially in the same manner, therefore the division for binocular vision, if made here by the appropriate prism, must be a very nearly equal division of every particular pencil, and give a similar and satisfactory image of the entire field in each eye-tube. This is a sufficient expression of the whole theory of the binocular eye-piece.

It is, however, important in order to avoid pseudoscopic effects, to adopt the proper form of dividing prism; and this form is precisely that best suited to that kind of binocular microscope in which the dividing prism is placed immediately above the objective. The natural presumption has been—contrary to this—that prisms of rectangular form would give the proper effects in the eye-piece, because of the pseudoscopic effects produced by their use in the microscope of binocular body. But this is an error, inasmuch as the pencils proceeding to form the second image in the erecting eye-piece reach the small dividing prism under conditions suitable for correct vision of the object *were the eye placed there*, and, accordingly, the same false appearances obtain with the eye-piece of rectangular prisms, *having oculars above*, as if such division were made immediately above the objective; the effect being, that the order in which the rays proceeding from the sides of the object or image viewed reach the eyes, as to right and left, is reversed from that which exists in natural vision; the left eye receiving a preponderating portion from the right side, and the right from the left side of the object.

It is to be noted, however, that the eye-piece with rectangular prisms, arranged after the first method of Prof. Riddell, does not uniformly produce conversion of relief, or that inversion of per-

spective which obtained in that first experimental arrangement for a binocular microscope. Such a *binocular eye-piece* used in the microscope upon transparent objects only occasionally gives the view in depth thus inverted. With low powers, and considerable thickness of the transparent object, the view is usually pseudoscopic. With medium and higher powers, it is otherwise; and the effect is much controlled in this respect by the direction of the light upon the object.

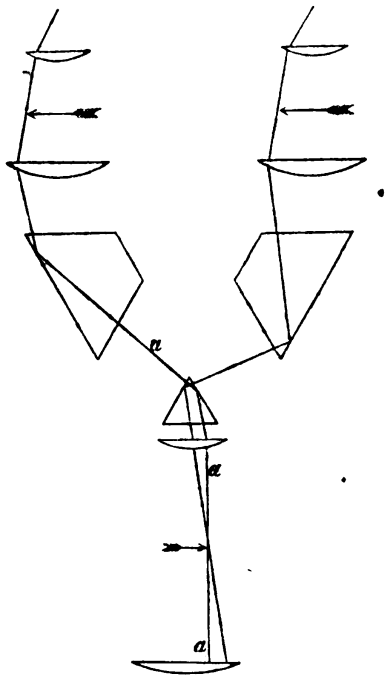
When the binocular eye-piece formed with rectangular prisms is used in the telescope to view a landscape, the perspective is not *throughout* inverted, but portions of the field appear interposed between the eye and nearer objects in a singular and somewhat startling manner.

By arranging the compound rectangular dividing prism so that the optical pencil is divided in the *plane of vision*, instead of vertically, the pseudoscopic effect is almost entirely obviated.

In constructing the binocular eye-piece, the prisms and arrangement of Nachet have been found to answer every condition and requisite of binocular vision. The dividing prism being placed, as before stated, at the point of crossing of pencils in the erecting eye-piece, each pencil of light will enter the small dividing prism and impinge upon its reflecting surfaces in a manner similar to that illustrated in the Nachet binocular microscope. The binocular eye-piece has greatly the advantage over the other arrangement. For when the prisms are placed *in the binocular body immediately above the objective*, their position, in order to secure a *proper division* of each transmitted pencil, should change with every change of objective used—which can be easily provided for in the case of low powers but is rather impracticable with the higher numbers, it being very difficult to bring the prisms sufficiently near to the posterior combination of the objective. On the contrary, when the binocular arrangement is embodied *in the eye-piece*, the prism being once fixed in proper position, as before described, is correctly placed for every power of objective, and the eye-piece, thus binocular in form, is as applicable through the whole range of powers as if it were monocular. Applied to high powers, only one condition would be distinguishingly critical in the case of the eye-piece,—that of the centricity of the central prism. The form of *erecting eye-piece* found most advantageous in this binocular adaptation is a duplication of the ordinary Huyghenian negative eye-piece, wherein the small dividing prism is very nearly at the eye-hole point of such a negative eye-piece as is ordinarily applied in the monocular microscope. At a proper distance above this is placed another negative eye-piece, in which is formed a second image of the object viewed.

This form of erecting eye-piece gives less extension above the

body of the microscope than the positive form, and for that reason is preferred.



The annexed diagram illustrates the division of one lateral pencil proceeding from one point of the first image formed in the eye-piece, and its general course to emergence at the two eye surfaces of the eye-piece. When the eye-piece is constructed of the form outlined in the annexed diagram, (which is about half size in the superior portion), the field is produced very satisfactorily, and of tolerable expansion; and does not necessitate more than 4·50 inches extension beyond the microscope body. The draw-tube can be as well withdrawn, and the eye-piece occupy its place, thus diminishing somewhat the total extent of the instrument. With proper modifications of the system of lenses placed before the prisms in the eye-piece, the whole binocular arrangement can be brought still nearer the objective, and retain also all the characteristics of the binocular eye-piece as contradistinguished from the binocular microscope known and in use.

The objection that loss of light must occur on account of the additional front system of lenses pertaining to an erecting eye-piece, (the lower system in the diagram,) of course militates against this arrangement; but there are, on the other hand, incidental advantages in the use of the erecting form. And moreover, if desirable, the "eye-lens" of the lower system can be successfully united with the dividing prism, thus eliminating two surfaces; and it is practicable to make the field-lens of each upper system solid with the opposite prism. The binocular erecting microscope would thus have no more refracting surfaces than the instrument of negative form and binocular body. As the object is, for the sake of efficiency with a high power objective, to give as large area to the transmitted pencil as possible at the point where it undergoes division in the small prism, therefore the power of the front system should be kept down, and amplification, as far as necessary in the eye-piece, be produced after the division has taken place. The accompanying cut is in this respect not in strict proportion—the lower system of lenses being represented not more than half the distance apart that the

most favorable arrangement for use with high powers (and equally with low) would call for, and the standard length of microscope body admit of. But the aperture of the lenses might remain about the same.

Having obtained by this means a pencil (or beam rather) transmitted through the eye-piece of the greatest possible dimension or area, at the point of binocular division, greater amplification in the eye-piece, as to its total power, might be advantageously effected by means of lenticular immergent and emergent surfaces of the upper prisms; the lower face of each prism to be *convex*, the upper emergent surfaces concave, giving achromatized refraction in each case. By this means, a larger field, together with a minimum length of tubes above the prisms, would be secured.

By thus appropriating every surface of all the prisms not a reflecting surface, for the purpose of lenticular refraction, the greatest aggregate advantage appears to be secured.

Applied to the telescope, the binocular eye-piece should of course have, and practically has, the same characteristics as when used in the microscope. In this case, however, the front lens of the eye-piece can be dispensed with. Its utility, as thus applied to those telescopes too large to be conveniently in the double form, is too obvious and striking to need remark. The view thus obtained is truly stereoscopic.

Canastota, N. Y., Dec. 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the wave lengths of Fraunhofer's lines.*—ANGSTRÖM has communicated to the Royal Swedish Academy of Sciences a memoir on the wave-lengths of the principal fixed lines in the solar spectrum, which possesses great value not merely as a revision of Fraunhofer's determinations, but also as containing data not hitherto to be obtained. The measurements which form the basis of the work were executed by means of an optical theodolite by Pistor and Martins, reading by means of two microscopes to 2″·1 of arc. The eye-piece was also provided with a micrometer, the head of the screw being divided into 100 equal parts, each division of the scale corresponding to 1″·308 for parallel rays. The ruled glass was by Nobert, and contained in a space of 9·0155 Paris lines, 4501 divisions, drawn with a diamond. The error of the division according to Nobert's measures, was less than 0·00002 of a Paris line. With this ruled glass Fraunhofer's lines could be seen in the third and fourth spectra, the lines being themselves more numerous and distinct than in the spectrum produced by a flint glass prism. The observations were calculated according to the well known formula

$$e \sin \theta = m\lambda,$$

in which e is the distance between two successive lines $= 0{\cdot}000166954$ of a Paris inch; λ the wave length sought, θ the observed angles of diffraction, and m the number of the spectrum. The influence of variations of temperature and pressure of the atmosphere was found to be insensible; that of the temperature of the ruled glass, which varied from 13° C. to 18° C., was also found too small to require the application of a correction. The same statement applies to a variation in the angle θ, arising from the absolute motion of the instrument in the direction of the path of the incident ray. As means of several determinations the following results were obtained for the lines measured by Fraunhofer, the unit of measure being $\frac{1}{1{\cdot}000000000}$ of a Paris inch.

B	2539·73	b	1916·50	H	1467·18
C	2426·29	F	1797·27	H_1	1453·98
D	2178·59	G	1592·34	—	.....
E	1948·24				

The difference in wave lengths for the two D lines was 2·226; between the two E lines only 0·395. (The numbers given above doubtless refer to the least refrangible lines in the cases of D and E.) The author gives a careful discussion of the results of the two series of measurements made by Fraunhofer, and arrives at the conclusion that the measurements made with Nobert's ruled glass deserve the greater degree of confidence. The following table gives the results of Angström's determinations of the wave lengths of other lines in the spectrum, the unit being the same as above.

A	2812	
B	2539·7	
C	2426·26	
a	2312·2	a strong atmospheric line.
	2287·3	A group of strong lines produced by iron and calcium.
	79·6	
	76·8	
	69·4	
	67·7	
	62·1	
	55·1	
D	2179·70	sodium lines.
	2177·48	
1.	2076·1	Iron.
2.	71·3	Iron.
3.	69·7	
4.	2068·3	Iron.
5.	65·4	
6.	60·1	"
7.	16·9	"
8.	13·6	"
	13·1	
9.	07·3	"
10.	05·3	"
11.	1998·4	Iron.
	97·9	
12.	85·8	" strong.
	85·3	" faint.
	84·2	"
	83·5	"
13.	1974·2	" double.
14.	69·6	"
15.	68·1	"
16.	65·3	"
17.	1953·2	"
E	1948·44	Iron and calcium.
	48·04	
	46·8	" " double.
	34·6	Iron.
	36·4	"
	19·6	" double.
b	1916·50	Magnesium.
b_1	12·39	"
b_{11}	1911·10	Iron and magnesium.
	10·49	Iron.
	03·4	"
c	1832·70	"
	19·1	"
	18·4	"

	08·3	Iron, double.
	01·1	"
F	1797·27	Hydrogen.
f	1632·2	Iron.
	28·5	"
	20·4	"
	04·3	Hydrogen.
	1598·8	Iron.
G	1592·34	Iron.
	79·1	"
	74·7	"
	71·2	Iron.
g	62·4	Calcium, double.
	32·0	Iron, double.
h	15·9	No metallic line.
	05·3	Iron, strong.
	02·0	"
	1495·2	"
	80·4	Strong, no corresponding metallic line.
H	67·2	Calcium.
H_1	54·0	"

In the concluding section of his memoir the author points out a method of determining the motion of the solar system by observing the interference bands produced by a ruled surface of glass. The principle of this method was laid before the Swedish Academy, Oct. 6, 1860, and was proposed by Babinet two years ago in the French Institute. It depends simply upon the fact that if we assume that the rays which pass through the openings in the ruled surface are propagated independently of the motion of the instrument, the same must be true of the interference bands, and consequently, in observing with the telescope, an aberration must ensue proportional to the relation between the motion of the telescope perpendicular to its axis and the velocity of the light in the direction of the axis. The results obtained in testing this method appear to confirm the correctness of the theory, but are not sufficiently numerous to be decisive. The author proposes to resume the subject under more favorable circumstances of weather.—*Pogg. Ann.*, cxxiii, 489. W. G.

[*Note.*—The annexed table of wave-lengths, taken in connection with that of Angström, will present to the reader, it is believed, all, or almost all, the exact knowledge which we have at present of the subject. The unit is here the $\frac{1}{10,000,000}$ of a millimeter.

	Wave-lengths.				Wave-lengths.		
1.	A	= 7680	Mascart.	18.	α' Hg	5759	Plücker.
2.	B	6897	Fraunhofer.	19.	γCO_2	5599	"
3.	α L	6763	Müller.	20.	$\gamma SnCl_2$	5584	"
4.	β N	6610	"	21.	β Hg	5461	"
5.	C	6559	Fraunhofer.	22.	α Cl	5451	"
6.	α H	6533	Plücker.	23.	δ I	5337	"
6*b*.	αPCl_3	6493	"	24.	$\delta SnCl_2$	5333	"
7.	$\alpha SnCl_2$	6445	"	25.	β O	5328	"
8.	$\alpha SiCl_2$	6329	"	26.	E	5265	Fraunhofer.
9.	α O	6150	"	27.	β Cl	5216	Plücker.
10.	11 N	6089	"	28.	δCO_2	5190	"
11.	βPCl_3	6024	"	28*b*.	γ O	5185	"
12.	$\beta SiCl_2$	5978	"	29.	α Br	= 5169	"
13.	β I	5947	"	30.	ε I	5167	"
14.	D	5888	Fraunhofer.	31.	$\gamma SiCl_2$	5050	"
15.	$\beta SnCl_2$	5794	Plücker.	32.	F	4856	Fraunhofer.
16.	α Hg	5782	"	33.	β H	4843	Plücker.
17.	17 N	5762	"	34.	β Br	4793	"

35.	γCl	4792	Plücker.	46.	γHg	4359	Plücker.
36.	γBr	4766	"	47.	γH	4339	"
37.	δBr	4691	"	48.	G	4296	Fraunhofer.
38.	ζI	4661	"	49.	iI	4215	Plücker.
38*b*.	δS_2	4631	Müller.	50.	H	3963	Fraunhofer.
39.	ηI	4629	Plücker.	51.	L	3791	Esselbach.
40.	γPCl_3	4591	"	52.	M	3657	"
41.	$\varepsilon SnCl_2$	4524	"	53.	N	3498	"
42.	ζCO_2	4501	"	54.	O	3360	"
43.	θI	4446	"	55.	P	3290	"
44.	ηCO_2	4382	"	56.	Q	3232	"
45.	δO	4367	"	57.	R	3091	"

W. G.

2. *On the phenomenon of interference in the prismatic and diffraction spectra.*—STEFAN has communicated to the Viennese Academy an interesting paper on the interference bands discovered by Talbot and employed by Esselbach,[1] and subsequently by Bernard,[2] for the measurement of the wave-lengths of the ultra-violet rays. Talbot's method of obtaining these bands consists in placing a thin plate of mica before the eye in such a way as to cover one-half of the pupil, leaving the other half free. On looking at a prism so placed that the spectrum is distinctly seen, dark bands will be seen parallel to the edge of the prism, provided that the plate of mica covers the half of the pupil which is upon the same side with the edge of the prism producing the spectrum. Stefan remarks in the first place, that the mica may be placed anywhere between the eye and the prism, and may even be cemented to the prism, provided that it cover the side next the edge. No bands are seen when the rays pass either through the covered or uncovered part of the prism above, but only when the eye receives rays which have passed through both these portions. The plate of mica may be cemented to the farther surface of the prism, or placed at any point between the prism and the slit, provided that the part of the bundle of rays which traverses the plate also passes near the edge of the prism. The same bands may be seen in the diffraction spectra produced by a plate of ruled glass: when the mica covers the left half of the pupil the bands are seen in the spectra lying to the right, and *vice versâ*. When two plates of mica are cemented to the same ruled glass, one to the right and the other to the left, with a narrow space between them which occupies the center of the pupil, bands are seen on both sides in all the spectra. These bands are similar when the plates are of equal thickness; they disappear when the plates are brought together, unless one plate is thicker than the other, when the bands are seen toward the side where the plate is thinnest. The plate of mica must not be too thick in these experiments, as otherwise the number and fineness of the lines become so great that they can no longer be distinguished by the eye. For a prism of 45° the author uses a plate of mica 0·1 millimeter in thickness; with a prism of 60° glass plates up to 0·15 mm. may be used, and a plate of this thickness gives about 120 lines between B and H. The lines produced by thicker plates are best seen by looking at the spectrum with a telescope and placing the inter-

[1] Pogg. Ann., xcviii, 513. [2] This Journal, Sept., 1864.

ference plate, as Brewster and Airy have done, between the eye and the eye-piece of the telescope, only, when the astronomical or inverting eye-piece is used, the plate of glass or mica must be placed upon the side which is away from the edge of the prism. Glass plates of the kind used as covering glasses for microscopic preparations are more even than plates of mica and give much sharper lines. The interference plate may be placed anywhere between the eye and the slit of the collimator. By attaching it to the objective on the side toward the refracting edge of the prism the lines are seen at once over the whole spectrum. It is important, in the case of thick plates, that the surfaces should be parallel. A glass plate about three millimeters in thickness with a flint glass prism of 60°, gave about 2500 very sharp and distinct bands between B and H. The bands near B are produced by the interference of rays whose difference of path amounts to over 2700 wave-lengths, while the interval between those near H amounts to more than 5000 wave-lengths. By inclining the interference plate its optical thickness is of course changed; an increase of the inclination causes the lines to run from the violet to the red end of the spectrum, and *vice versâ*. The lines have their maximum deviations when the plate is perpendicular to the direction of the ray, and this affords an easy method of adjustment. As the plate may be placed anywhere between the objective and the prism or between the prism and the collimator, we have the means of determining the influence of physical changes produced in the plate by heat or pressure. The same statements apply in general to the phenomena as seen by the diffraction spectrum. Finally it is possible to produce the interference bands by placing the plate in front of the slit so as to cover one-half of the opening. In this case it is a matter of indifference upon which side of the slit the plate is placed.—*Pogg. Ann.*, cxxii; from *Sitzungs Ber. der kais. Akad. der Wiss.*, xlix. W. G.

3. *On thermo-electric batteries of remarkable power.*—Bunsen has found that kupferkies possesses a far higher place in the thermo-electric series than bismuth, and that pyrolusite is also superior to the metal though inferior to kupferkies. When kupferkies is combined with an alloy of two parts of antimony and one of tin, or, in order to employ high temperatures, with copper, a couple is produced which, under equal circumstances, gives far more powerful currents than any combination hitherto known. In four experiments the ratio between the electro-motive forces in a kupferkies-copper element and in Daniell's battery was found to be as 1 : 14·7, 1 : 11·9, 1 : 10·9, and 1 : 9·7, while the essential resistances to conduction were 0·74, 0·79, 0·72, and 0·72. During the experiments the temperature of one end of the element, which was plunged into water, was about 60° C., while that of the other end was above that of melting tin. The battery gives a ten times more powerful action than a bismuth-antimony element of the same resistance heated to 0° and 100°. Ten pairs of the kupferkies-copper elements fully replace a Daniell's pair of fourteen square centimeters of active copper surface. Kupferkies is easily fused at a high temperature and may be cast in a mould, but its place in the thermo-electric series is then far below that of bismuth. The native mineral, which is easily worked, must be employed. Pyrolusite combined with platinum gives a battery, the electro-motive force of

which may be easily brought to $\frac{1}{10}$ of that of Daniell's battery without decomposing the mineral. In one experiment Bunsen found the electromotive force of pyrolusite and platinum not less than $\frac{1}{9}\frac{1}{8}$ of that of Daniell's battery, but the resistance to conduction was 18·4 times as great as that of the Daniell's cell used for comparison.—*Pogg. Ann.*, cxxii, 505.

W. G.

4. *On the thallic alcohols.*—Under this term LAMY has described the compounds which are formed by replacing an equivalent of hydrogen in wood-spirit, common alcohol, &c., by an equivalent of thallium, and which are represented respectively by the formulas $\left.\begin{matrix}C_2H_3\\Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_4H_5\\Tl\end{matrix}\right\}O_2$, $\left.\begin{matrix}C_{10}H_{11}\\Tl\end{matrix}\right\}O_2$, &c. Methyl-thallic alcohol is solid and crystalline. The ethyl compound is a heavy colorless oily liquid; its density at 0° is 3·550; its indices of refraction for B and H are respectively 1·661 and 1·759, the difference between these numbers which measures the dispersive power is 0·098, while the corresponding difference for bisulphid of carbon is 0·079. The amyl-thallic alcohol is also a colorless oil, but its refractive and dispersive powers are much less than those of the corresponding ethyl compound, which is at once the heaviest, the most refractive and the most dispersive of organic compounds. The thallic alcohols are decomposed by boiling; they give by distillation pure hydrogen, alcohol, the corresponding acid and a large residue of metallic thallium. All three compounds burn in the air with a green, more or less brilliant flame, and all are soluble in the corresponding alcohol or in ether. Chloroform also dissolves thallic alcohols, but the solution is soon decomposed with deposition of protochlorid of thallium. Water decomposes these compounds, producing a hydrate of the oxyd of thallium and regenerating the alcohol proper. Acids also decompose them, but carbonic acid forms a solid and definite combination. The analogy between this class of compounds and the corresponding bodies containing potassium and sodium is striking, and confirms the correctness of the view which associates thallium with the alkaline metals and separates it from lead.—*Comptes Rendus*, lix, 780.

W. G.

5. *The Correlation and the Conservation of Forces.*[1]—Under this title Dr. Youmans has presented a collection of memoirs by Grove, Helmholtz, Mayer, Faraday, Liebig, and Carpenter, together with a well written introduction and brief biographical notices of the various writers whose works he has collected. The work is a very welcome addition to our scientific literature, and will be particularly acceptable to those who wish to obtain a popular, but at the same time precise and clear view of what Faraday justly calls the highest law in physical science—the principle of the conservation of force. To scientific men the book will also be most useful for convenience of reference. Sufficient attention has not been paid to the publication of collected monographs or memoirs upon special subjects. Dr. Youmans's work exhibits the value of such collections in a

[1] The Correlation and Conservation of Forces. A Series of Expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig, and Dr. Carpenter. With an Introduction, and Brief Biographical Notices of the Chief Promoters of the New Views. By Edward L. Youmans, M.D. 12mo, pp. xlii, 438. N. Y.: D. Appleton & Co.

very striking manner, and we earnestly hope that his excellent example may be followed in other branches of science. Meantime we will permit ourselves to beg that the faithful laborer in the cause of science and the liberal publisher to whom we owe this volume will still further add to the obligations under which they have laid men of science by giving a complete collection of the mathematical papers which have appeared upon the subject of the mechanical equivalent of heat, and which are now scattered through many volumes of scientific journals and transactions of learned societies, so as to be accessible to few and inconvenient for all.

6. *A new developing solution.*—Some experiments recently undertaken in company with Mr. A. B. Crockett for the purpose of ascertaining the quality of different developing solutions, have led us to the conclusion that a mixture of double sulphate of potash and iron with double sulphate of ammonia and iron, produces better results than any hitherto used. The negatives made by this combination are remarkable for clearness of the shadows, and beauty of half tones, without an excess of intensity in the high lights. The solution acts with remarkable regularity and uniformity, and has little or no tendency to produce fogginess, or a thickening of the fine lines, even when kept upon the plate for a long time.

In practice the following formula was found most convenient. Although not theoretically exact, it produces excellent results.

Pure water, - - - - - - -	32 oz.
Neutral sulphate of potash, - - - - -	½ "
Proto-sulphate of iron, - - - - - -	¾ "
Double sulphate of ammonia and iron, - -	1 "

To this solution add two drops of ammonia, two ounces of acetic acid, and alcohol sufficient to make it flow evenly over the plate. A. E. V.

II. GEOLOGY.

1. *Note on the Geological age of the New Jersey Highlands as held by Prof. H. D. Rogers;* by J. P. Lesley.—The last sentence of an article in the January number of the American Journal of Science, (at p. 97,) extracted from the Proceedings of the Natural History Society of Montreal, seems to me to do great injustice to Professor H. D. Rogers, and other geologists, whose views of the structure and relationships of the rocks of the Highlands have long been published, and are in this article both ignored and misrepresented. The sentence to which I refer reads as follows:—

"This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region [the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs] as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestone of Orange county and of New Jersey underlie unconformably the Lower Silurian strata."

Now Prof. Rogers, to my certain knowledge, not only never expressed such an opinion, at any time, but has always maintained just the reverse; and proved, thirty years ago, what he published on page 43, (Chap. 2,) of

his New Jersey Report, in 1840, viz: that the "sedimentary rocks [of the Lower Silurian system] repose in immediate contact with the gneiss, presenting from the attitude of their beds, abundant evidence that they were precipitated upon it [that is, upon the system of the Highlands in New Jersey] while it was yet only in part elevated above the waters." The Canadian geologists will find in these words a clear statement of precisely the view which they themselves have learned to entertain by their week's excursion last summer.

Whatever errors Rogers may have made—and we all make errors—no one can charge him justly with so tremendous a blunder as that of regarding the Highland range as a metamorphic repetition of the Silurian sediments. He mistook indeed the crystalline limestones engaged among the Highlands for metamorphosed synclinal outliers of No. II, as at Franklin (see page 62, New Jersey Report); but this is an exceptional case, as any one can see by studying the true outlying Silurian synclinals represented as sunk among the gneiss hills on Rogers's along sections in the New Jersey Report; or by referring to the many sections (made very nearly at the same time) of the synclinal Silurian valleys among the Durham hills of Pennsylvania. Professor Cook has seen horizontal Potsdam or Calciferous beds overlying these upturned Franklin limestones; (see this Journal, [2], xxxii, 208); and there are exposures of similar crystalline limestones, west of New Jersey, where the evidence is rather in favor of, than against, their subsilurian age. But Prof. Rogers's synthesis of Appalachian geology was on too grand a scale to permit him to overlook *as a whole* and *in the main* the mutual relationship of the Silurian and Azoic Systems. The words which I have quoted above are immediately followed by others showing how clear and large a view he took of the subject, a view which the Canadian geologists are only illustrating:—

"As the same strata [the Silurian], moreover, hold a similar relation to our primary rocks throughout their entire range, from Vermont to Alabama, separated from them by no other group of strata yet discovered, claiming an earlier origin, I have deemed it expedient, for the sake of classification, to confer upon them the title of the *Older Secondary* strata of the United States .... with the synonyme of the *Appalachian System of Strata.*" (p. 44.)

On page 12, Rogers designates the Highland rocks as the "*Gneiss system,*" and, on page 13, carefully distinguishes them from the Staten Island, Trenton, Philadelphia rocks, on the south, as he subsequently distinguishes them from the Potsdam, Trenton, and Hudson river Silurian on the North. On page 11, he gives his column of Formations, and places the "*Primary Rocks*" of the Highlands, underneath No. 6, of the "*Lower Secondary Rocks;*" "a white quartzose sandstone, somewhat coarse and friable: occurs only in a few localities"; and pages 12 to 43 are devoted to a Chapter on these rocks, under the heading "*Primary Rocks of the State: Geology of the Highlands.*"

In fine, no geologist, even of small capacity, could study the New Jersey Highlands, the Durham Hills of Eastern Pennsylvania, and their continuations in the South Mountain and Blue Ridge, and even conceive the suspicion, much less publish it as a conviction, that their strata were metamorphic representatives of the Silurians. We know very well

that to say that Rogers thought or said such a bêtisme is to do him gross injustice.

East of the Hudson, there seems to have been made by Rogers, Hall, Logan and many others, all, in fact, except Emmons, a gross mistake of this kind, owing to peculiar circumstances, principally the presence of a great fault of unknown size and location, secondly, by the obliteration of the usual fossils from the top-slates ("Hudson River") of the Lower Silurian System, and thirdly, by a serious change in the Lower Silurian group itself, viz: its expansion in thickness toward the northeast, which made it at first to be confounded with the true Highland gneiss-range running behind it. But in Southern New York, New Jersey, Pennsylvania, and farther south, no such mistake was possible, and at least was never made by Rogers. In fact he had enjoyed such advantages for studying the phenomena of this very belt that he was just the last man in the world who could have made it.

I think the above reclamation is due, in his absence, to Professor Rogers, whose untiring energy and devotion to Appalachian geology did so much to lay fundamental views, which our Canadian friends will find it beyond their power to do more than illustrate. I wish they would try their hand at bearing the veritable *crux criticorum* of our geology, the age and relationships of the Philadelphia-Baltimore belt. There is also something to do about *our* Potsdam sandstone. For as to the patches of so-called Potsdam on the North face of the Highlands at Reading—the long ridge of Potsdam crossing the Schuylkill, 15 miles above Philadelphia—the cliffs of so-called Potsdam at Chicques rock, on the Susquehanna, two miles above Columbia—the supposed Potsdam at the anticlinal of the Nittany Valley, crossing the Juniata near the foot of the Alleghanies—I think it is very doubtful, in spite of the *Scolithus linearis* which they contain, whether these correspond *exactly* to the New York bottom layer of the Silurian. The structure of the Chester county valley, of white marble, with a Potsdam barrier on the north and a mica slate and serpentine country on the south, is a great puzzle. If Sir William Logan and Prof. Hall will take *these* in hand they will run small risk of doing injustice to anybody.

At the same time, it is very satisfactory to have a well trained Canadian eye, familiar with Laurentian lithology, after taking a good look at our gneissic mountains, pronounce them certainly Laurentian and not Huronian, and to receive from the same high authority the announcement that the Sillery sandstones and other prominent members of the Quebec group disappear, coming this way, at about the latitude of the Hudson Gap. For we have been looking for their representatives in Pennsylvania, in vain; although Emmons is understood as identifying his Taconic system with the rocks of the southeast side of the Great Valley.

Philadelphia, Jan. 13, 1865.

2. *Skulls of the Reindeer Period from a Belgian bone-cave, indicating a superior, as well as an inferior, race of primitive men in Europe.*—Prof. Van Beneden, the distinguished zoologist of Belgium, in recent explorations in caverns, has found, as he writes to John Lubbock, Esq., of Chiselhirst, Kent, crania of two distinct pre-historic races, of the Reindeer period, and one of them, that least well preserved, "est franchement brachy-

cephale et prognathe, mais avec une belle boite cranienne." The cavern is in the Carboniferous limestone, 30 or 40 meters above the level of the Lesse. A large number of human remains were found.—*Reader, Jan.* 7.

3. *Casts of fossils.*—Prof. H. A. WARD, of Rochester, New York, is contributing greatly to the diffusion of a knowledge of geological science through the country, by furnishing casts, made under his direction, of fossils of various kinds, from those of the Megatherium, Dinotherium, Glyptodon, and others of gigantic dimensions, to those of Ammonites, Trilobites, eggs of Æpiornis, Gold nuggets, etc. The casts are remarkably well made, firmly secured against breaking, and of high finish. All institutions in which geology is taught, would find it greatly to their advantage to supply themselves with some of the Rochester casts.

4. *Geological Survey of Canada*, Sir W. E. LOGAN, *Director. Figures and Descriptions of Canadian Organic Remains*, Decade II: *Graptolites of the Quebec Group*, by JAMES HALL. 152 pages, 8vo, with 21 plates and many wood-cuts. Montreal, 1865: *Dawson Brothers;* London, New York, and Paris: *Bailliere.*—The first, third and fourth Decades of the figures and descriptions of Canadian Organic Remains, published as a part of the Reports of the Geological Survey of Canada, were issued in the years 1858, 1859, as announced in this Journal in vol. xxvi, at p. 299, and vol. xxviii, at p. 148. The second Decade, just now published, contains an elaborate and extended memoir on the Canadian Graptolites by James Hall. The wonderful variety of these fossils afforded by the Quebec group has enabled Prof. Hall to throw great light on the growth, development and structure of Graptolites. The numerous plates contain full and excellent illustrations of the species. The number of species recognized in the Quebec group is over *fifty*, and none of these are known higher in the series. In the Trenton and Hudson River groups the whole number known is about *thirty*, and in the Upper Silurian there are but *two* (and these occur in the Clinton group), excepting the species of the genus *Dictyonema*, here referred by Hall, of which there are *three* species in the Quebec group. *one* in the Trenton, *three* in the Niagara, *one* in the upper Helderberg and *two* in the Hamilton.

5. *Alger's Cabinet of Minerals.*—The excellent Cabinet of minerals made by the late Francis Alger, of Boston, has been purchased for Allegheny College, Meadville, Pennsylvania.

III. BOTANY AND ZOOLOGY.

1. *Harvard University Herbarium.*—This establishment is noticed in the Annual Report of the President of the University to the Board of Overseers, made in January last, as follows:—

"Dr. Asa Gray has presented to the University his invaluable Herbarium and his Botanical Library; which have been safely transferred to the fire-proof building furnished, at a cost of over twelve thousand dollars, by the generosity of Nathaniel Thayer, Esq., of Boston. A fund has also been raised by subscription, for the support and increase of the collection. . . . The gift of Dr. Gray cannot be estimated in money, but it embraces the results of many years' labor faithfully given by that distinguished botanist, aided by the generosity of his collaborators and correspondents in various parts of the world."

The collections were formally presented by the following letter:—

"*Botanic Garden, Cambridge, November* 30, 1864.

"To the Rev. Dr. HILL, President of Harvard University,

"*My Dear Sir*:—I have the pleasure to inform you that the Herbarium and Botanical Library, which a year ago I offered to present to the University, are now safely deposited in the building erected for their reception by Mr. Thayer. I have regarded them as belonging to the University from the beginning of the present year: but I wish more formally to make them over to the President and Fellows, as the foundation of the Harvard University Herbarium.

"The Herbarium is estimated to contain at least 200,000 specimens, and is constantly increasing. From the very large number of typical specimens it comprises, its safe preservation is very important.

"The Library, from the rough catalogue which has been made out, contains about 2200 botanical works—perhaps 1600 volumes, and nearly as many separate memoirs, tracts, &c.

"The current expenses of the establishment for the first half of the year now drawing to a close have been defrayed by Dr. Jacob Bigelow, who placed in my hands a special donation of two hundred dollars for this purpose.

"I had stated that the income of a capital sum of $10,000 would be required to defray the current expenses of the Herbarium, i. e. for the purchase of certain collections and books not obtainable by exchange, for freights and charges, paper, alcohol, fuel, &c. I am informed that this sum, which Mr. George B. Emerson undertook to raise by subscription, is substantially secured. It is desirable, but probably not at this time practicable, that this endowment should be so far extended as to provide for the services of a Curator, so that I could myself devote valuable time to the prosecution of important botanical works for which I am prepared, and to which I am pledged.

"I have the honor to be, with great respect, very truly yours,

ASA GRAY."

We understand that extensive collections of botanical specimens, to be added to the herbarium, have recently accrued. Among them are—

A full suite of Mr. Charles Wright's collections, (about 2500 specimens,) made in Cuba during the past four years, and just now arranged and distributed among botanists.

A very interesting set of plants recently collected, chiefly by Professor Brewer, in the Geological Survey of California under Professor Whitney.

The numerous and important duplicate *Carices*, (and other *Cyperaceæ*,) of the late Dr. Boott, presented by Mrs. Boott; the proper herbarium, set of *Carices* having been bequeathed to the herbarium of the Royal Gardens at Kew.

A large collection of plants of Mauritius and Madagascar, and a continuation of the distribution of the British East Indian herbaria of Griffith, Helfer, &c., presented by the directors of the Royal Botanical Gardens and Herbaria at Kew.

A similar distribution (in continuation) of plants of the Dutch East Indies and Japan, from the Royal Netherlands Herbarium, Leyden, now under the charge of Professor Miquel.

A selection from the Mexican collections of the late Professor Liebmann (Oaks, Ferns, *Cyperaceæ*, &c.); from the Royal Danish Botanic Garden, Copenhagen.

An extensive set of authentically determined plants of Persia, Siberia, and Northern China, from Professor Bunge of Dorpat; and of Algerian plants, &c., from Dr. Cosson.

A set of Mandon's plants of the Andes of Bolivia; acquired by purchase.

A fine general collection of *Algæ*, from Professor Agardh of Lund, authentically named, according to his new *Species Algarum*.

But the most notable accession is the munificent gift which has just been made by John A. Lowell, Esq., of all the botanical books of his own library which the new establishment did not already possess, being chiefly very large, choice, and costly works, such as the Flora Danica, Sibthorp's Flora Græca, Bateman's Orchidaceæ of Guatemala, the Botanical Register, Botanical Cabinet, Botanical Repository, Richenbach's Icones, the large edition of Duhamel, the great works of Jacquin, and others of the same character,—in all 335 volumes, the pecuniary value of which must be reckoned at several thousand dollars.

The building, to which these treasures are consigned, is of brick, upon a granite foundation, with freestone facings, is 32 feet in front and 56 feet deep, a story and a half high, the walls all hollow and ventilated, for greater security from dampness. On the side next the residence of the Professor (with whose private study it is connected by a cold conservatory, 18 feet in length), the wall is entire, except at the private entrance, which is guarded by a double set of wrought iron doors. The room for the herbarium (for greater security against fire) is also separated from the small working-rooms and library and entry in front, by a hollow brick partition, through which passage is had only by heavy iron doors. In the half-story over the front rooms are two other commodious working or store rooms. The principal room, for the herbarium, is about 30½ by 35 feet, and 19 feet high to the vaulted ceiling. It is lighted by a domed skylight, and by a large double window in the north-western end. At the height of about 8 feet, an iron gallery, four feet deep, surrounds the apartment, interrupted only by the large window. The space between the floor and the gallery is completely filled by the herbarium-cabinets, except upon one side of the entrance, where a sort of furnace or stove, of soapstone, chiefly in the front room, comes through the partition wall, and supplies warm air by registers. The walls above the gallery are reserved for a second and similar tier of cabinets, to be constructed when needed. The cabinets, casings, and all the woodwork except the floors (which are of hard pine, bedded in mortar) are of chestnut wood. The building was constructed under the superintendence of the architect, Mr. Edward D. Harris, in a thorough and durable manner, and it is hoped will form a safe and permanent place of deposit for the collections which have been and may hereafter be consigned to it.

2. *Story about a Cedar of Lebanon.*—In the *Edinburgh Review* for October last, turning over the pages of an article on Coniferous Trees, we read, with more of amusement than instruction, a detailed account of the famous Cedar of Lebanon, in the Jardin des Plantes, which, the Review-

er informs us, is "well-known from an anecdote connected with its arrival there."

"It appears that about one hundred and thirty years ago, viz. in 1737, M. Bernard de Jussieu, the celebrated botanist, *when travelling in the Holy Land*, had brought away with him, from among the cedars of Mount Lebanon, a little seedling. Being unprovided with better means of conveyance, *he made a flower pot of his hat, in which he planted it.* He got it safely on board the vessel in which he sailed for France; but tempestuous weather and contrary winds drove the ship out of her course, and prolonged the voyage so much that the water began to fail," .... And the narrative goes on, at great length, to say how the passengers were reduced to the allowance of half a glassful of water a day,—how Jussieu, "all through the lengthened voyage, under the bright sun of the Mediterranean, shared his half glassful of water with his little plant,"—how "his own strength began to sink under the prolonged privation, but he never flinched, and arrived at Marseilles with his own health damaged, but with that of his little plant uninjured,"—how it is said that he came near losing his treasure through the incredulity of the custom house officers, who suspected a contrivance for smuggling jewels or prohibited articles under the roots of the seedling;—how his entreaties and eloquent appeals at length prevailed, and he was allowed to carry his seedling undisturbed to the *Jardin des Plantes*, where it was planted, and became a great and famous tree;—how it grew and flourished until it reached one hundred years of age and 80 feet in height; and, finally, how "in its hundredth year (1837) *it was cut down to make room for a railway, and now the hissing steam-engine passes over the place where it stood.*" (The italics are our own.)

Of course, it is almost unnecessary to tell our readers that Bernard Jussieu never visited the Holy Land, and was not likely, if he had, to come home bare-headed, using his hat the while for a pot; that the fact, or at least the accepted tradition, is merely this, that he brought the seedling Cedar from *England* to Paris in his hat. The story of the voyage from the Levant to Marseilles appears to be an adaptation of one about the three Coffee-plants, which *Antoine* de Jussieu, in the year 1720, sent from the *Jardin des Plantes* to the vessel commanded by Capt. Declieux, who was charged by the French Government with the duty of transporting them to Martinique. The voyage being unusually long, the water is said to have given out, two of the precious plants died, and the remaining one is said to have been kept alive by the devotion of the Captain, who bestowed upon it his own scanty ration of water, and so preserved the ancestor of all the coffee-plantations of the Antilles. For this devotion, we presume, his name is commemorated in the genus *Declieuxia*, of the Coffee Family.

What are the other ingredients of this *pot-pourri* we are unable to conjecture. But the naturalists of the *Jardin des Plantes* may be somewhat astonished to learn that a railway traverses their peaceful grounds, and that a hissing steam-engine runs over the steep little hill upon which flourished, and as they fondly imagine still flourishes, Bernard de Jussieu's Cedar of Lebanon.

We learn from this same article, farther on, that "poor Douglas," the botanical explorer of Oregon and California, "perished at last in a pit-

fall set for *bears or buffalos in a North American* forest; and that the *average height* of full-grown trees of the Mountain Redwood, (*Sequoia gigantea*, which they still call *Wellingtonia* in Scotland) is full three hundred feet!

3. *Calluna vulgaris in Newfoundland.*—Mr. Murray, late of the Geological Survey of Canada, and now engaged in a survey of Newfoundland, has brought to Montreal specimens of this plant, which were collected by Judge Robinson, on the east coast of Newfoundland, near Ferryland (lat. 47°, long. 52° 50′), and which are stated to be from a small patch of the plant not more than three yards square.

4. *Preservation of Starfishes with natural colors;* by A. E. VERRILL.—Starfishes may be dried, so as to retain their natural colors almost unimpaired, by immersing them in alcohol of moderate strength for about a minute, or just long enough to destroy the life and produce contraction of the tissues, and afterward drying them rapidly by artificial heat. The drying is best effected by placing them upon an open cloth stretched tightly upon a frame and supported a few feet above a stove. Care should be taken not to raise the heat too high, as the green shades change to red at a temperature near that of boiling water. By this process I have succeeded in preserving the delicate shades of red, purple and orange of the species found on the coast of New England, including *Solaster papposus*, *S. endeca*, *Cribella*, *Asteracanthion pallida*, *A. littoralis*, and various other species, specimens of which are in the Museum of Yale College.

The same process is equally applicable to Echini and Crustacea.

5. *A new American Silkworm.*—After numerous experiments, Mr. L. Trouvelot, of Medford, Mass., has succeeded in rearing successfully, and in great numbers, *Attacus Polyphemus* Linn., and in preparing from its cocoon an excellent quality of silk, possessing great lustre and strength, and pronounced superior to Japanese and all other silks, except the best Chinese, by competent judges.

The silk is unwound by a simple process perfected by Mr. Trouvelot, each cocoon yielding about 1500 yards. This insect is very hardy, being found throughout the Northern States and Canada; and, as it feeds upon the leaves of oak, maple, willow, and other common forest trees, may be reared easily in any part of the country.

Mr. Trouvelot has gradually increased his stock from year to year, by raising young from the eggs of the few individuals first captured, until he has at present seven wagon-loads of cocoons, the entire progeny of which he proposes to raise during the coming season.

The thanks of the country are due to the ingenious and persevering author of this successful attempt to introduce a new and interesting field for industry and enterprise, which cannot fail to be a source of profit to those who intelligently engage in it, and of increased wealth and prosperity to the people, should it be developed to the extent that now seems possible.

The first public notice of his experiments with this insect was given by Mr. Trouvelot at a meeting of the Institute of Technology, at Boston, about a year ago, when he exhibited specimens of silk manufactured from it, both natural-colored and dyed. A. E. V.

IV. ASTRONOMY AND METEOROLOGY.

1. *Shooting Stars of Nov.* 11–14, 1864.—Since the last No. of the Journal of Science was issued the Committee on Meteors of the Connecticut Academy of Arts and Sciences have received, by letter from Prof. B. Silliman, dated in California, the following observations obtained by him, while in that part of the country:—

First; Observations at Virginia, in Nevada, made by Mr. RICHARD H. STRETCH:—*Nov. 12th.* One observer. Three hours watch, from $1^h\ 15^m$ A. M. to $4^h\ 15^m$ A. M. Eighteen shooting stars were observed. Twelve were conformable to the well-known radiant in Leo, although four of the number exhibit large deviations. But the deviations are about equal, on either side of the radiant. Of the number that were not conformable, five had their courses directed from an area, $3\frac{1}{2}°$ in diameter, in the same general quarter of the heavens, but far to the north,—in fact, centered about 1° N.W. of β in *U. Major.* Only one—and that of small magnitude—exhibited no tendency from the common radiant. All were yellow, except one that exploded with a beautiful green light. Nearly all appeared as brilliant points of light,—and six of the number were as conspicuous as stars of the first magnitude; also four of the latter were remarkable either for scintillations, or explosion, or broad trains attending them. These four were of the twelve first above mentioned.

Nov. 13th.—The same observer. His watch was begun at $1^h\ 45^m$ A. M. In one hour and eight minutes twenty-two falling stars were seen through the moonlight. Afterward twenty-four more were observed, during a time not stated, but ending, not improbably, about 4^h A. M.—as on the morning preceding. Excepting No. 22, at $2^h\ 53^m$, all appeared as points of yellow light. Mr. Stretch has illustrated the phases of this No. 22, by three sketches. It exploded like a rocket, its tail dropped, and grew shaggy and broad, it turned from yellow to red, shaping itself, at first, like the horn of an ox, and finally like a corona; or, more nearly like a blunt crescent occupying three-fourths of a circle, with the left hand branch exceeding the other one-half in length, but of much less curvature. The duration was three-quarters of a minute.

It is remarkable that the entire group was conformable to a common radiant; although eight of the forty-six—and only eight—show a large deviation—a part to the east, but more to the west. This radiant, however, was in A. R. 156° and N. Dec. 24°,—or nearly seven degrees of arc eastward by northward of its ordinary situation. This position, and the same of the 12th, are derived from the very neat charts which accompany the observations of Mr. Stretch, and which have every meteor's flight traced clearly and numbered upon them.

Second; Observations at Shasta, Nevada, made by Mr. G. K. GODFREY. *Nov. 14th.*—Time, from midnight until 2^h A. M., certainly, and probably until later. At midnight Mr. Godfrey had his attention attracted, "while riding into Shasta, by the falling of innumerable stars." Of these he counted "some fifty," but the time of counting is not stated. Occasionally a star "shot off to the east with great velocity, leaving a brilliant trail behind. There were some which "cast a bright scintilla-

tion of a violet color," another group "were of a deep crimson, and much larger, apparently, than the fixed stars, and described a small arc in the sky, and were confined to a small space in the heavens." These last expressions agree (color excepted) with the striking appearances, in 1833, of a limited space around the radiant; but Mr. Godfrey has not stated, even in general terms, the position of the "small space" which he has described. All that can be certainly inferred from his account is the simple fact that shooting stars appeared, at Shasta, on the morning of Nov. 14th last, in such numbers, and of such brilliancy, as to fix Mr. Godfrey's attention upon them for, at least, two hours without any previous notice or expectation of such an occurrence. A. C. T.

New Haven, Jan. 5.

2. *Meteor and Meteorites of Orgueil.*—On the evening of the 14th of May, 1864, a very bright fireball was seen in France throughout the whole region from Paris to the Pyrenees. Loud detonations were heard in the neighborhood of Montauban, and a large number of stones came down near the villages of Orgueil and Nohic. The passage of the meteor was witnessed by a large number of intelligent observers since it occurred early in the evening. Numerous accounts of its appearance have been published in the *Comptes Rendus.*

This fall of meteorites is of peculiar interest. Whilst we have over a hundred large fireballs and detonating meteors whose paths through the atmosphere have been computed, with more or less precision, there are only four or five of them from which stones have been known to come. Of these four or five only one, the Weston meteor, has been so well observed that we can speak with confidence of its path.

The published accounts show that the Orgueil meteor was first seen at an altitude greater than 55 miles, that it exploded at an altitude of about 20 miles, and that it was descending in a line inclined at the least 20° or 25° to the horizon. The velocity must have been not less than 15 or 20 miles per second. This example affords the strongest proof that the stone-producing meteors and the detonating meteors are phenomena not essentially unlike. H. A. N.

3. *Chemical and mineralogical characters of the meteorite of Orgueil.*—Daubrée, Cloëz, Pisani and Des Cloizeaux have communicated to the French Academy papers on the physical and chemical characters of the Orgueil meteorite. In outward appearance it resembles an earthy lignite. The dark mass contains minute grains of a bronze yellow substance having a metallic lustre and a high density, which permits its easy separation from the main portion of the meteorite by levigation; observed under the microscope, these particles are seen to be distinct hexagonal tables; they are strongly attracted by the magnet and have all the physical and chemical properties of magnetic pyrites. A very marked characteristic of the meteorite is, that when placed in water it falls to powder, and a portion of it is in such an extreme state of mechanical division that it remains a long time in suspension in the water, and passes through the closest filter paper. The density of the meteorite taken in pure benzine gave Cloëz 2·567. An analysis by this chemist shows it to contain a considerable amount of magnetic oxyd of iron, besides silicates, protosulphid of iron, traces of nickel and chromium, 5·92 per cent of carbon

(probably of the graphitic form), 9·06 p. c. of water and 5·30 p. c. of matter soluble in water, consisting of chlorids of ammonium, sodium and potassium, and sulphates of magnesia, soda, etc. It is decomposed by chlorhydric acid with evolution of sulphuretted hydrogen giving a greenish yellow solution and leaving a black residue amounting to 7·6 p. c.; this residue when heated with excess of air burns and leaves a gray substance amounting to 2·2 p. c.

Pisani confirms the observations of Cloëz as to the peculiar comportment of this stone when treated with water, and calls attention to its porosity, and to the fact that this accounts for the facility with which the sulphids have become oxydized to sulphates and hyposulphites, and for the avidity which a dried specimen of it has for water. An experiment showed that a specimen dried at 110° C. absorbed 7 p. c. of water in a few hours when simply exposed to the air. Pisani found 3·35 p. c. of matters soluble in water in operating on about 18 grams of the undried substance. This contained hyposulphurous acid 0·48, sulphuric acid 1·40, chlorine 0·08, magnesia 0·30, lime 0·16, potash 0·60, soda, ammonia, etc., and loss 0·77. Alcohol took up 0·37 p. c. which proved to consist chiefly of sulphur. Pisani's tabulated results give for the composition of the whole meteorite:

$\ddot{S}i$	$\dot{M}g$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	$\dot{N}a$	$\dot{K}$	$\ddot{A}l$
26·08	17·00	6·96	0·36	1·85	2·26	0·19	0·90,

together with chromic iron 0·49, magnetic iron 12·03, nickeliferous sulphid of iron 16·97, water and supposed organic substances 14·91 = 100. This gives for the oxygen ratio of the oxyds and silica 9·98 : 13·90 = 3 : 4.

Pisani proved the presence of magnetic iron by dissolving the mineral in hot nitric acid which decomposed the silicate and the sulphids, and left a black magnetic residue. The nickel was proved to exist as sulphid by treating the substance with sulphid of ammonium, which dissolved out sulphid of nickel. The stone contains 55·60 p. c. of silicates, and taking into account the water determined by Cloëz, the oxygen ratio of the silicate is that of serpentine. If the alumina is due to anorthite it will give 2·42 p. c. of this feldspar contained in the meteorite. Subsequent observations by Des Cloizeaux, Pisani, Daubrée and Cloëz prove that this remarkable meteorite contains minute rhombohedral crystals of a double carbonate of magnesia and iron, and Cloëz obtained a little more than half of one per cent of carbonic acid from a portion of the meteorite operated upon.—*Comptes Rendus*, May 30, July 18, and Nov. 14, 1864.

G. J. B.

4. *Shooting Stars of Jan. 2d.*—On the morning of Jan. 2d, 1865, shooting stars were sufficiently numerous to attract the attention, at New Haven, of those who were not aware that unusual numbers were looked for on that morning.

5. *Discovery of another Asteroid, Alcmene*, ⑫.—Another small planet was discovered by Luther, at Bilk, on the 27th of November, 1864. It has been named Alcmene. Oppolzer of Vienna gives the following elements of its orbit, computed from observations of Nov. 27th, Dec. 3d, and Dec. 8th.

Epoch Dec. 0·0, Berlin m. t.	$i = 3° 3' 14''$
$M = 308° 40' 1''$	$\varphi = 11\ 25\ 41$
$\pi = 136\ 47\ 50$	$\mu = 781''·152$
$\Omega = 28\ 49\ 28$	$\log a = 0·438181.$

6. *Comet IV*, 1864.—On the 11th of December, 1864, Mr. Bäker at Nauen, near Berlin, discovered a telescopic comet.

7. *Spectrum of a shooting star.*—Herschel has recently observed the spectrum of a shooting star. It appeared near Capella, and was almost as brilliant as that star. He followed it for more than a second in its rather slow motion, and ascertained that its spectrum was as continuous a spectrum as that of Capella, and a little more extended, and, therefore, that it consisted of a solid or liquid substance and not of a gas or incandescent vapor, as Mr. Huggins has suggested with regard to some nebulæ.

V. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *Patent regenerative Gas-furnaces, of* C. W. & F. Siemens.—Professor Faraday, in his lecture at the Royal Institution, on the 20th June, 1862, describes these furnaces in the following terms:—

"The gaseous fuel is obtained by the mutual action of coal, air, and water at a moderate red heat. A brick chamber, perhaps 6 ft. by 12 ft., and about 10 ft. high, has one of its end walls converted into a fire-grate, *i. e.*, about half way down it is a solid plate, and for the rest of the distance consists of strong horizontal plate bars where air enters, the whole being at an inclination such as that which the side of a heap of coals would naturally take. Coals are poured, through openings above, upon this combination of wall and grate, and being fired at the under surface, they burn at the place where the air enters; but as the layer of coal is from 2 to 3 feet thick, various operations go on in those parts of the fuel which cannot burn for want of air. Thus the upper and cooler part of the coal produces a large body of hydrocarbons; the cinders or coke which are not volatilized, approach, in descending, toward the grate; that part which is nearest the grate, burns with the entering air into carbonic acid, and the heat evolved ignites the mass above it; the carbonic acid, passing slowly through the ignited carbon, becomes converted into carbonic oxyd, and mingles in the upper part of the chamber (or gas producer) with the former hydro-carbons. The water, which is purposely introduced at the bottom of the arrangement, is first vaporized by the heat, and then decomposed by the ignited fuel, and re-arranged as hydrogen and carbonic oxyd; and only the ashes of the coal are removed as solid matter from the chamber at the bottom of the fire-bars.

These mixed gases form the gaseous fuel. The nitrogen which entered with the air at the grate is mingled with them, constituting about a third of the whole volume. The gas rises up a large vertical tube for 12 or 15 feet, after which it proceeds horizontally for any required distance, and then descends to the heat-regenerator, through which it passes before it enters the furnaces. A regenerator is a chamber packed with fire-bricks, separated so as to allow of the free passage of air or gas between them. There are four placed under a furnace. The gas ascends through one of these chambers, whilst air ascends through the neighboring

chamber, and both are conducted through passage outlets at one end of the furnace, where mingling, they burn, producing the heat due to their chemical action. Passing onward to the other end of the furnace, they (*i. e.*, the combined gases) find precisely similar outlets down which they pass; and traversing the two remaining regenerators from above downward, heat them intensely, especially the upper part, and so travel on in their cooled state to the shaft or chimney. Now the passages between the four regenerators and the gas and air are supplied with valves and deflecting plates, which are like four way-cocks in their action; so that by the use of a lever these regenerators and air-ways, which were carrying off the expended fuel, can in a moment be used for conducting air and gas into the furnace; and those which just before had served to carry air and gas into the furnace now take the burnt fuel away to the stack. It is to be observed, that the intensely heated flame which leaves the furnace for the stack always proceeds downward through the regenerators, so that the upper part of them is most intensely ignited, keeping back, as it does, the intense heat: and so effectual are they in this action, that the gases which enter the stack to be cast into the air are not usually above 300° F. of heat. On the other hand, the entering gas and air always pass upward through the regenerators, so that they attain a temperature equal to a white heat before they meet in the furnace, and there add to the carried heat that due to their mutual chemical action. It is considered that when the furnace is in full order, the heat carried forward to be evolved by the chemical action of combustion is about 4000°, whilst that carried back by the regenerator is about 3000°, making an intensity of power which, unless moderated on purpose, would fuse furnace and all exposed to its action.

Thus the regenerators are alternately heated and cooled by the outgoing and entering gas and air, and the time for alternation is from half an hour to an hour, as observation may indicate. The motive power on the gas is of two kinds—a slight excess of pressure within is kept up from the gas-producer to the bottom of the regenerator to prevent air entering and mingling with the fuel before it is burnt; but from the furnace, downward through the regenerators, the advance of the heated medium is governed mainly by the draught in the tall stack, or chimney.

Great facility is afforded in the management of these furnaces. If, whilst glass is in the course of manufacture, an intense heat is required, an abundant supply of gas and air is given; when the glass is made, and the condition has to be reduced to working temperature, the quantity of fuel and air is reduced. If the combustion in the furnace is required to be gradual from end to end, the inlets of air and gas are placed more or less apart the one from the other. The gas is lighter than the air; and if a rapid evolution of heat is required, as in a short puddling furnace, the mouth of the gas inlet is placed below that of the air inlet; if the reverse is required, as in the long tube-welding furnace, the contrary arrangement is used. Sometimes, as in the enameller's furnace, which is a long muffle, it is requisite that the heat be greater at the door end of the muffle and furnace, because the goods, being put in and taken out at the same end, those which enter last, and are withdrawn

first, remain, of course, for a shorter time in the heat at that end; and though the fuel and air enters first at one end and then at the other, alternately, still the necessary difference of temperature is preserved by the adjustment of the apertures at those ends.

Not merely can the supply of gas and air to the furnace be governed by valves in the passages, but the very manufacture of the gas-fuel itself can be diminished or even stopped, by cutting off the supply of air to the grate of the gas-producer; and this is important, inasmuch as there is no gasometer to receive and preserve the aeriform fuel, for it proceeds at once to the furnaces.

Some of the furnaces have their contents open to the fuel and combustion, as in the puddling and metal-melting arrangements; other are enclosed, as in the muffle furnaces and flint-glass furnaces.

The economy in the fuel is esteemed practically as one-half, even when the same kind of coal is used either directly for the furnace or for the gas-producer; but, as in the latter case, the most worthless kind can be employed—such as slack, &c., which can be converted into a clean gaseous fuel at a distance from the place of the furnace, so, many advantages seem to present themselves in this part of the arrangement."

Faraday concludes his lecture with the following conclusive figures:—

"Carbon, burnt perfectly into carbonic acid in a gas-producer, would evolve about 4000° of heat; but, if burnt into carbonic oxyd, it would only evolve 1200°. The carbonic oxyd, in its fuel form, carries on with it the 2800° in chemical force, which it evolves when burning in the real furnace with a sufficient supply of air. The remaining 1200° are employed in the gas-producer in distilling hydro-carbons, decomposing water, &c. The whole mixed gaseous fuel can evolve about 4000° in the furnace, to which the regenerator can return about 3000° more."

2. *National Academy of Sciences.*—The fourth session of the National Academy of Sciences was held at the Capitol in Washington on the 3d, 4th, 5th, and 6th of January of the present year. The three vacancies in the Academy were filled by the election of Prof. O. N. Rood and Gen. M. C. Meigs to the Class of Mathematics and Physics, and Prof. J. P. Kirtland to the Class of Natural History. The following papers were read:

1. On a chronograph for measuring the velocities of projectiles; by J. E. Hilgard.
2. Homologies and classification of the Cephalopods; L. Agassiz.
3. Geographical distribution of North American Birds; S. F. Baird.
4. On the tables of the Moon; Benj. Peirce.
5. Metamorphoses of some Malacopterygians; L. Agassiz.
6. On chemical classification; W. Gibbs.
7. Progress of the Geological survey of California; J. D. Whitney.
8. On a method of exhibiting certain statistics of hospitals; J. L. Leconte.
9. Note on the changes which have taken place in the bar of Charleston harbor since the sinking of obstructions in the main channel, as developed by the U. S. Coast Survey; J. E. Hilgard.
10. Glacial phenomena and present configuration of the State of Maine; L. Agassiz.

11. Dimensions and proportions of American soldiers; B. A. Gould.

12. On a regulator for maintaining uniform motion, and an apparatus for recording time-observations in type; J. E. Hilgard.

13. Mineral lands of the United States, and the relation of the government to their management; J. D. Whitney.

14. Origin and formation of sedimentary rocks; J. S. Newberry.

15. Origin and distribution of petroleum in the U. S.; J. S. Newberry.

Alex. Braun, G. B. Airy, Astronomer Royal of England, R. Owen, Prof. F. Wöhler, Sir R. I. Murchison, and M. V. Regnault, were elected Foreign associates. The next session of the Academy will be held at Northampton, Mass., on Wednesday the 23d of August, 1865.

3. *Lawrence Scientific School, at Cambridge, Mass.*—The Lawrence Scientific School has received, as a New Year's gift, (Jan. 1, 1865) fifty-two thousand five hundred dollars, twenty-five hundred to be expended at once for the equipment of the laboratory, and the balance to endow equally the Chemical and Engineering departments.

Obituary.—4. Capt. James M. Gilliss, U. S. N.—Captain Gilliss, the Superintendent of the Washington Observatory, died suddenly, at Washington, of apoplexy, on Thursday, the 9th of February. The Naval Observatory, under his charge at the time of his death, was constructed from his plans, and equipped with its original instruments by him, during the years 1843, 1844, Congress having authorized its establishment by an Act passed in 1842; but only since 1861, when Maury, faithless to his country, left his post of duty, has it been under his abler direction. It would have been better for the scientific reputation of the country had it continued in his hands. An earlier Observatory at Washington, fitted up mainly by him, had been the scene of his labors from 1838 to 1842, and in the volume containing the results—the *first* volume of American Astronomical Observations—Mr. Gillis expresses in his Preface, his pleasure that "the prosecution of these observations should have resulted in the foundation of a permanent Naval Observatory."

During the three years, 1849 to 1851, Capt. Gilliss was in Chili, in charge of the U. S. Expedition for determining the Solar Parallax, and if his observations failed of all that was expected of them, it was from the want of that coöperation in the northern hemisphere which was reasonably looked for by him. The National Intelligencer, (Washington) of the day before his death, (Feb. 8,) contains his last astronomical communication—one relating to the Planet Mars—dated Feb. 7.

Capt. Gilliss was an observer of great skill and accuracy, a man of noble personal character, and a patriot in the highest sense of the word. Three of his sons have been in the recent armies of his country, and the eldest—a Captain—reached home from the Libby prison, after four months imprisonment, only the day before his father died.

5. George P. Bond.—It is seldom that astronomical science has received a more severe blow than that occasioned by the death of George Philips Bond, of Harvard College, Philips Professor of Astronomy, and Director of the observatory connected with that institution. After a lingering illness of more than a year, during which his ardor in the study of the heavens led him oftentimes to exposures entirely incompatible

with the state of his health, he closed a useful and an unblemished life on the 17th of February,—eight days after his compeer, Captain Gilliss.

As an accurate and truthful observer of astronomical phenomena, he was, without question, unequalled by any one in this country, and among the first in the world. In his short career he contributed many valuable papers of original discoveries and calculations to various periodicals and institutions in this and other countries. His greatest work, and that which gave him honor the world over, is his account of the Donati comet which constitutes the third volume of the Annals of the Observatory. To this, the palm of unrivalled excellence has been freely awarded by the best astronomical observers of Europe.[1] Well trained by his lamented and distinguished father, and taking advantage of the best telescope mounted in so high a southern latitude, he explored with searching scrutiny the great nebula of Orion, a work which he pursued with untiring zeal and anxiety in his latter days; and while we fear his waning strength may have left it incomplete in form, we are assured, and rejoice in the assurance, that abundant ability remains in the observatory to prepare it for publication.

We might dwell much longer on his astronomical history, but the necessary brevity of this notice requires that we should turn to his private life. It is rare indeed, that so many virtues are blended in any man. His innocent unpretending manners, the perfect absence of every air of vanity or pretension, crowned with an unwavering christian faith and deep sense of religious obligation, secured for him, not the mere respect, but the kindest regard of all that had the happiness of his acquaintance. W. M.

6. Dr. HUGH FALCONER.—Dr. Falconer, Vice-President of the Royal Society, died at London, on the 31st of January.

7. *The Differential Calculus;* by JOHN SPARE, M.D. Boston, 1865. Bradley, Dayton & Co. 8vo. pp. xx, and 244.—The plan of this treatise is different from that of most works upon the same subject. The aim seems to have been to treat the calculus as peculiarly the continuation of Algebra. Hence most of the applications are to problems which resemble as much as possible the ordinary problems of Algebra. The author's style is somewhat involved, and his logic not entirely free from criticism. H. A. N.

8. *History of Delaware County, Pennsylvania, from the Discovery of the Territory included within its limits to the present time, with a notice of the Geology of the County, and Catalogues of its Minerals, Plants, Quadrupeds and Birds;* written under the direction and appointment of the Delaware Co. Institute of Science, by GEORGE SMITH, M.D. 582 pp. 8vo.—This history is the work of great care and research. The chapter on the Geology of Delaware County is illustrated by a colored map showing the distribution of its rocks, and it closes with a catalogue of mineral localities. The work is illustrated by several plates, wood-cuts and maps.

[1] We are informed that, a month since, Mr. Bond received word from President De La Rue, of the Royal Astronomical Society, that the Society, at its last annual meeting, in January, had voted him a gold medal for his work on this comet.

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[SECOND SERIES.]

ART. XXVII. —*On Molecular Physics;* by Prof. W. A. NORTON.

[Continued from vol. xxxviii, page 223.]

IN a former part of this paper a succinct exposition has been given of a consistent general theory of Molecular Forces, and the Molecular Constitution of bodies, and special theories of the different states of aggregation of matter, and the processes of transformation from one state to another, as well as of the essential nature and modes of excitation and propagation of the two agencies of light and heat.[1] The conclusions arrived at were all deduced from two fundamental principles, viz:

[1] In a paper on heat-vibrations, by Mr. James Croll, published in the Philosophical Magazine, May, 1864, it is maintained that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules; also that it does not consist in excursions of the molecule or atom across centres of equilibrium, but in alternate expansions and contractions of the atom itself. It will be seen that these ideas are in accordance with the conception of the constitution of a molecule adopted at the beginning of the present memoir (p. 62), and with the theory of heat-vibrations, or heat-pulses, deduced therefrom (p. 64). The author remarks that his conclusion that "the ultimate atom itself is essentially elastic, is opposed to the ordinary idea, that the atom is essentially solid and impenetrable, and favors the modern idea that matter consists of a force of resistance acting from a center." But in the present communication the ground is taken that it is the elastic ethereal atmosphere condensed upon the atom that invests it with its panoply of power. Thus armed it becomes an efficient molecule—when associated with a kindred molecule an epitome, in fact, of the universe. In the contractions and expansions that result from the action of the central atom upon its atmosphere is to be discerned the origin not only of heat-vibrations, but of all the molecular forces; and in the varied possible movements and changes of molecular atmospheres, dependent upon their elasticity and mutual action, are to be found the essential causes of physical phenomena.

(1.) That matter exists in the three different forms of *ordinary* or *gross matter*, an *electric ether*, and a more subtile *universal ether;* and that each of these is made up of spherical atoms.

(2.) That there are two primary forces: *attraction*, and *repulsion.*

The primary force of attraction is exerted between the atoms of ordinary matter and the two ethers, and between the atoms of the electric ether and those of the universal ether; while a mutual repulsion subsists between the constituent atoms of the two ethers.

From these two postulates the conclusion was derived that each atom of ordinary matter must be surrounded by two atmospheres, one consisting of electric ether, and another of the universal ether, pervading the former; and that the atoms of the electric ether must also be surrounded by atmospheres of the more subtile universal ether which pervades the space between them. Such being the condition of things, it was assumed that the attraction of the central atom of matter for the atoms of electric ether exterior to it was propagated by the intervening universal ether, and that the same was true of the mutual repulsion exerted between the individual atoms of the electric ether.

The primary force of heat, as one of the molecular forces, was deduced from these principles, and found to have its origin in the force of molecular attraction.

We propose now to show that the characteristic phenomena of electricity, comprised under the several heads of *frictional electricity*, *voltaic-electricity*, *thermo-electricity*, *magnetism*, *electro-magnetism*, *magneto-electricity*, *induced currents*, and *dia-magnetism*, may be derived as mechanical deductions from the same fundamental conceptions.

We have thus far had no occasion to introduce arbitrary hypotheses, but have in fact discarded those hitherto in vogue, as that of the permanent polarity of atoms, and various hypotheses as to the mode by which such polarity is constituted and maintained. It will be seen that in the wide field we are now entering, the same fundamental ideas will suffice, and that the same two forces, attraction and repulsion, operating on the same three forms of matter, are alone concerned in evolving the phenomena. In every different province that we enter we but recognize new results, achieved by the same agencies, working by the same essential processes. As transcendently wonderful as is the infinite variety of Nature, no less so is the all-comprehensive unity of its origin, and the grand simplicity of its evolution. From the point of view now taken, this truth, which has long been discerned with more or less distinctness, stands forth in its full proportions. In the discussion of specific properties of different

substances, we have admitted, and shall continue to admit, only differences in degree, not in kind; and differences too, that fall within the scope of the general theory. No higher requirement than this can reasonably be exacted of any fundamental conception.[2]

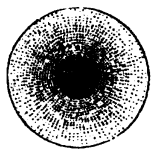
Electricity.

Preparatory to the discussion of this topic it is important to inquire more minutely than has hitherto been done into the electric condition of molecules, both simple and compound. Each simple atom, as we have seen, is surrounded by an electric atmosphere; but has this atmosphere a definite limit? and if so what is the condition of the electric ether exterior to it, and in the interstices between molecules? The equilibrium of such an atmosphere is determined by the operation of two antagonistic forces, the attraction of the central atom for each atomette of the atmosphere, and the repulsion experienced by such atomettes from all the others of which the atmosphere is composed. If we regard these forces as in no degree intercepted in their propagation, the atmosphere will extend with decreasing density to that height at which the attraction exerted by the central atom upon an atomette is just neutralized by the repulsion exerted upon the same by the entire atmosphere. The centers of repulsion and attraction will also be coincident, and the law of variation of the two forces will be the same; hence beyond the limits of the atmosphere there will be no effective action exerted by the molecule upon any electric ether that may be posited there. But the notion that there is no interception of force does not accord with the fundamental idea of the propagation of force by the universal ether (p. 238). It is impossible that a propagated force of repulsion should take effect upon an atomette of electric ether, unless the universal ether, which is the medium of propagation, be in some degree condensed upon the surface upon which the wave-force falls. Such condensation must give rise to a reflex wave, and the dispersion of a certain amount of force into the surrounding ether. Upon the principle of the conservation of force the amount of force thus dispersed must be abstracted from the original wave-force. Now if the atomettes of the electric atmosphere intercept a certain portion of the repulsion propagated from other atomettes, it follows that the center of repulsion of the whole atmosphere cannot be coincident with the center of attraction, or center of the atom of mat-

[2] The author may seem to have adopted, in the scheme of molecular forces presented on pp. 63, 64, an arbitrary hypothesis, in assuming the existence of a force of molecular repulsion between the surfaces of contiguous electric atmospheres; as the grounds upon which the theoretical inference was drawn were not stated in the course of the discussion. There will soon be occasion to offer these in another connection.

ter. It would seem also that the repulsive pulses propagated from the atomettes should be more or less intercepted by the central atom of the molecule, which should tend to displace the center of repulsion still more. For each side of the molecule this center must lie somewhere between the center of the atom and the surface of the atmosphere, as at *r*, fig. 4. In this state of things the atmosphere will have a definite limit, as before; but beyond its limits, since its repulsion must decrease more rapidly than the attraction, an effective force of attraction will be exerted by the entire molecule. As a consequence, the external electric ether will be retained in contact with the atmosphere, and press upon its surface with a certain force. Its density will decrease outward and doubtless become insensible at sensible distances. If then we confine our attention to a single molecule, we perceive that it must consist of an atom of ordinary matter surrounded with two envelopes of electric ether; of which the outer presses upon the inner, and extends indefinitely, but becomes evanescent at a distance a certain number of times greater than the diameter of the inner envelope.[1] Hitherto in speaking of the electric atmosphere of a molecule, we have alluded only to the inner and principal envelope, and have regarded the diameter of a molecule the same as the diameter of this envelope; and we shall use these terms in the same sense hereafter, unless the exception is distinctly specified.

4.



The outer electric envelopes of molecules serve to establish an electric communication between them, and play an important part in all electric phenomena. By their pressure upon the inner envelopes, or the atmospheres so-called, of the molecules, they develop a force of electric repulsion exerted outward at the surface of each atmosphere. This is the force already recognized as one of the molecular forces (p. 64). It has its immediate origin in the compression of the atmosphere at or near its surface, by the outer envelope, which increases the repulsive action of the upper portion of the atmosphere, and so brings into operation an effective repulsion at this surface, where otherwise the effective force would be zero. It is to be borne in mind that the attraction of the central atom is dynamical in its effects, and that attendant upon its exertion the outer envelopes will have an inward and outward movement; but such alternate movements will, when propagated outward, neutralize each others' effects, unless a secondary force is developed in the process. Now it is precisely such a force that is developed in the manner just explained, and this must be propagated by the electric ether

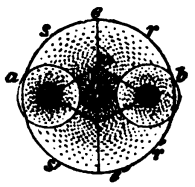
[1] It is possible that in some cases the outer atmospheric envelope may be made up of several spherical layers, separated by surfaces of no effective molecular action.

of the outer envelope, or the interstitial electric ether of bodies, as a wave-force.[4]

We have seen that the attraction exerted by the atom upon its atmosphere, proper, by forcing outward a portion of the universal ether near the surface of the atom, develops at the same time an effective molecular attractive force and what we have called the force of heat-repulsion. From the difference that obtains in the circumstances under which these forces and the above mentioned force of electric repulsion originate, it is highly probable that their constants (n and m, p. 68,) would be different.

To pass now to the case of a *compound molecule:* let a and b (fig. 5) be two similar molecules, held in equilibrium by their mutual actions, and let us enquire into the condition of the electric ether on the line crossing perpendicularly the line of their centers, at its middle point, m. At m the attractions of the two molecules for an atom of electric ether there will neutralize each other, but at all other points of this line, the resultant of the attractions will have a finite value and be directed inward toward the line of the centers. This resultant will increase in value as the distance from the line of the centers increases, to a certain point, n, beyond which it will decrease. The sum of all these resultants will act as a compressing force upon the electric ether about m, and determine its density at that point. At other points, as e, r, s, variously situated in the vicinity of the molecules, the joint attraction of the two will generally exceed the attraction of either molecule alone. It follows, therefore, that the two united molecules will be surrounded by an electric atmosphere of their own, spherical or spheroidal in form, and at the same time that the two individual atmospheres will be materially modified. This atmosphere will have an outer envelope, as in the case of a simple molecule.

5.



If we suppose several molecules thus united, the entire group will have its own proper atmosphere. The extent of this atmosphere will depend upon the normal attraction at the surface of the group; and this will also determine the density of the electric ether in the interstices between the molecules.[5]

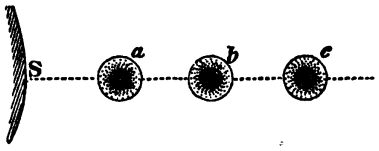
[4] The pulses of heat that are received from extraneous sources, and pass from molecule to molecule, augment this force of atmospheric repulsion. Accordingly, the value of the "constant," m, of this force (p. 67,) depends not only upon the attractive force, size, and perhaps other peculiarities of the central atom, but also upon the amount of heat received from all extraneous sources. External forces, of compression or extension, applied to a body, also tend to augment or diminish the value of m.

[5] In treating briefly of solidification (p. 209, &c.), our attention was confined chiefly to the union of simple molecules in one homogeneous mass, but there is good reason to believe that in most cases of solidification, with the exception, perhaps, of that of perfect crystallization, compound molecules are first formed, and that these combine, in various modes, to form the solid. In every such instance the formation

Let us next inquire into the true nature of the *electric polarization* of molecules.

A molecule becomes polarized whenever, from any cause, a portion of its electric atmosphere is urged around from one side to the opposite side; where there is an excess, the polarity being *positive*, and where there is a deficiency, *negative*. But it is especially important to observe that the act of polarization is distinct from the result. The one is dynamical, the other statical. The act of polarization, too, does not consist simply in a flow of a certain amount of electric ether from the one side to the other of the atom; in addition to this, a certain quantity of the ether is detached, or expelled from the atmosphere on the positive side, and a corresponding amount absorbed into it on the negative side. Accordingly, *when a molecule is being polarized, or when its polarization is increasing, there is a flow of electric ether toward its negative side, and away from its positive side. On the other hand, when a polarization once acquired is falling off, there is a flow, or an electric current, in the opposite direction.* To make this more evident, let S (fig. 6) be a surface receiving a charge of positive electricity, and *a*, *b*, *c*, &c., a series of particles of a dielectric medium. The repulsion of the positive charge on S will polarize the nearer side of *a* negatively, and the farther side positively; but the same force will urge the interstitial ether toward *a*, and the augmented attraction of the negative side of *a* will add a certain portion of it to the atmosphere of *a*. At the same time, the excess of electric ether accumulated on the farther side of *a* will act repulsively upon the upper portion of its atmosphere and expel a certain portion of it. It will also urge toward *b* the interstitial electric ether between *a* and *b*, and polarize *b*, just as S has polarized *a*. The chain of particles will therefore become polarized in succession; and at the same time there will be an electric movement, a dis-

6.



of such groups of molecules should be attended with the evolution of heat. This will be the inevitable result of the compression of the electric ether between the combining molecules (fig. 5); whether the individual atmospheres be compressed or not.

The heat of combustion and of chemical union generally, has, in all probability, a similar origin; that is in a compression of the electric ether between the molecules, or in such a compression combined with a contraction of the individual atmospheres. This topic, and the probable cause of the different effects observed in different instances of combination, will be briefly considered under the head of chemical union.

The varied forms of crystallization assumed by different substances, under similar circumstances, are probably due, in a great degree, to the diversity that obtains in the number and grouping of the individual molecules of which the ultimate compound molecules consist.

charge from one to another. To take a more complete view of the matter, the molecular polarization resulting from the electricity received by any single point, S, will not be confined to the normal line *a*, *b*, *c*, but will extend, though with diminished intensity, along lines radiating outward from S. A similar remark may be made of the flow of electricity that accompanies the polarization.

If we take account of the entire spherical surface, S, and suppose it to be surrounded by a dielectric medium, as the air, extending to an indefinite distance, each new addition to its positive charge will develop a wave of increasing molecular polarization, which will be propagated in all directions outward through the surrounding medium. This wave, if it may be so termed, will be accompanied by a spherical electric "wave of translation" that will spread indefinitely through the same medium. This latter wave will consist of a series of discharges from one spherical layer of particles to the next; while the former will consist of propagated movements confined to the atmospheres of the particles;—these movements being produced by the repulsive action of the electric ether accumulating on the farther sides of the particles, transmitted primarily through the universal ether. The quantity of electricity that moves forward from one layer of particles to the next, should be equal to the quantity received by the surface, for the determining cause of the electric wave of translation lies in the fact that the force of tension of the propagated electricity urges this quantity between the surface and the first layer of the medium.

The tension of the electricity received results from the compression produced by the expansive action of the charge, which is ultimately in equilibrium with the force by which the disturbed atmospheres of the medium tend to return to their original condition. The tendency of the free electricity to escape between and around the polarized molecules of the medium is counteracted, as will soon be seen, by a resistance developed by the polarization.

This we conceive to be the true process of *induction*. It will be readily perceived that it is in entire conformity with the experimental results obtained by Faraday, and in accordance with his theoretical ideas of the general nature of the process.

We have supposed the electrised surface to be surrounded by a single dielectric medium, extending indefinitely; but the result would be essentially the same if the waves were propagated through several such media, as air, glass, &c. If we suppose the first medium to be replaced by another, while the entire quantity of electricity propagated by it, as the polarization of the medium goes on, should remain the same, as Faraday has shown that it actually does, it does not follow that the final degree of

polarization received will be the same as before. In this respect substances may differ, or they may have different "specific inductive capacities," as maintained by Faraday. It is, in fact, just this difference of property that constitutes the difference between conduction and non-conduction, or between the different degrees of conduction or of non-conduction.

So long as the charge of electricity on the surface S, fig. 6, remains the same, the degree of polarization of the surrounding medium, or media, will continue the same; but if the charge be drawn off, waves of decreasing polarization will be propagated outward from S, which will be accompanied by electric waves of translation flowing inward toward S. The entire process that went on within the medium while the body was receiving its charge, will now be exactly reversed.

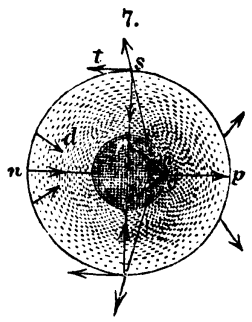
If we suppose a body to receive a charge of negative electricity, the process will also be reversed; as when a charge of positive electricity is withdrawn. The final result will be that the particles of the surrounding dielectric medium will be positively polarized on the side toward the electrised surface, and there will be a deficiency of electric ether between the surface and the first layer of the particles of the medium.

Conduction.—We have seen that the act of polarization comprises two different processes: a movement of a certain portion of the ether of the atmosphere of a molecule from one side to another, and cotemporaneously with this a discharge of ether from the one side, and an absorption of a corresponding quantity upon the other side. Now the electric constitution of two different bodies may be such that, with the one the same amount of electric movement, from particle to particle, may be accompanied by a feeble degree of polarization, and with the other by a much greater degree. The polarization is a change determined originally by the forces propagated by the universal ether, while the outward flow of the electricity is partly the result of the mutual repulsion between contiguous atoms of the electric ether.

Let us suppose two extreme cases, compatible with our general theory. (1.) Let there be an indefinite line of molecules; such that the density of the ether between them is the same as at the surface of the molecular atmospheres, and suppose a quantity of free electricity to arrive at one end of this line. It is plain that the repulsive energy of this electricity will urge before it a certain quantity of electricity throughout the whole length of the line, and that if the flow be unchecked it can exert little or no disturbing action upon the molecular atmospheres except very near their surface, and cannot polarize them. To do this the moving electricity must have a certain decided tension; that is, be present in a certain sufficient quantity at each point of the line. This would be the case of perfect, or nearly perfect

conduction, and there would be little or no polarization of the molecules. (2.) Let us suppose that the density of the electric ether is very feeble between the molecules, there being now no continuous mass of ether to give way before the electricity received at the end, the latter will, as a first effect, exert its repulsive action through the universal ether upon the atmospheres of the nearer molecules and polarize them, and this will extend through the line, and the polarization will determine the discharge in the manner already explained. This would be the case of a non-conducting dielectric medium. In all actual instances of good conduction the case lies between these two extremes. The greater part of the electric movement is confined to the outer envelope of the atmosphere, and the outer portion of the inner envelope; and the polarization resulting from the disturbance of the lower envelope, or the atmosphere proper, is slight. With non-conductors, or poor conductors, the flow near the surface of the lower envelope, around the molecule, resulting from the direct repulsive action (or elastic tension) of the electric ether, is comparatively slight; and the polarization of the lower envelope, with the attendant discharge on the farther side and absorption on the nearer side, is much greater.[6]

To obtain a more distinct idea of the entire process of imperfect conduction, we must take account of a certain resistance which a polarized molecule offers to the flow of ether around it from the negative to the positive side.[7] Let *a*, fig. 7, be a polarized molecule. The center of repulsion of its electric atmosphere will fall at some point, *o*, on the plus side of the center. If we enquire into the effective action of the molecule upon a mass of electric ether posited at *s*, midway between *p* and *n*, if the atmosphere were not disturbed the center of



[6] In the comparison made on p. 216, between the propagation of heat and the propagation of electricity, it is intimated that each is promoted by a polarization of the molecules. This is true only in a limited sense. The circumstances most favorable in general to a conduction of either electricity or heat, are a uniformly dense condition of the interstitial electric ether, which may serve as a medium for the direct propagation of the waves, or pulses, and such a condition of the electric atmospheres of the molecules that the resistance developed by any polarization that may ensue, shall be comparatively feeble. (See p. 246). When, however, the direct conductive action, by the intervention of the interstitial electric ether, is feeble, a greater facility of polarization on the part of the individual molecules would promote the discharge from molecule to molecule.

[7] The terms negative and positive, as here used, have reference to the polarization simply, which is a change in the distribution of the electric ether in the lower portion of the atmosphere. It will be seen in the sequel that in the upper portions through which the free electricity moves, when a conductive discharge occurs, there will be an excess of electric ether, or a positive state, on the side of the molecule which is negatively polarized, and a deficiency or a negative state on the opposite side.

effective repulsion would fall at r; in its supposed polarized condition, therefore, it will lie at some point m intermediate between r and o. The two forces exerted upon s would therefore act in the direction shown in the figure, and their resultant would act from s toward t. It will be readily seen that at all points between p and n, the resultant will have a tangential component directed toward n, and a normal component acting outward on the positive side, and inward on the negative side of the molecule. The density of the electric ether at n should therefore be greater than at p; and while by its elastic tension it is urged around toward p it is resisted by the tangential force just spoken of. And so if the electric ether above n is urged by the repulsion of free electricity on that side of the molecule around toward p, it has to overcome this resistance. It is to be observed that the attractive or compressing action about n, is but the result of the repulsive energy of the free electricity which has determined the polarization.

The outer circle in the figure represents the surface of the undisturbed atmosphere (inner envelope). When the molecule becomes polarized the surface of its atmosphere takes, or tends to take, the spheroidal form, with the center of figure lying on the side negatively polarized. At points near this surface, about midway between p and n, and indefinitely beyond the surface, there will be a resultant force, as shown in the figure, tending to check the flow of free electricity around or past the molecule, in the direction of n toward p.

It does not follow because the lines of direction of the attraction and repulsion meet at s under a small angle that the resultant $s\ t$ may not be equal to, or greater than the force of tension of the free electricity that may reach the molecule, since the absolute intensities of these forces must be very great. Besides, whatever may be the intensity of this resultant, it must be of a corresponding order of magnitude with the tension of free electricity, since it is precisely the resistance exerted by the polarized molecules of air contiguous to an electrised surface, that determines the tension of the statical electricity collected upon it.

When a discharge takes place through an imperfect conductor, as rarefied air, the contiguous particles of air are first polarized by the repulsion of the electrised surface, propagated by the universal ether. This determines the escaping electricity to fall upon their nearer negative sides; the molecular resistance considered above then comes into play, and if the elastic tension of the electricity is sufficient to overcome this resistance, a conducting discharge takes place; otherwise the electric movement can only be after the manner explained on p. 243, that is, as in the process of induction. According to these views, the greater resistance offered by dense than rarefied air, is to be explained

by the smaller number of resisting particles encountered, together with the diminished resistance of each particle. For it will be evident, on refering to fig. 7, that if the molecular atmosphere expands, as it will if the air be rarefied, the resistance at *s* will be diminished. Thus air rarefied to a certain degree should become a conductor of electricity. For a similar reason the surfaces of solid, or liquid bodies, should conduct electricity better than the internal portions, since the molecular atmospheres are more expanded there.[8] It will also be observed, in view of the results obtained, that imperfect conduction and non-conduction depend upon certain effects resulting from the polarization of molecules, and that non-conductors may become imperfect conductors if the tension of the charge presented to them be sufficient. But it should not be overlooked that the amount of polarization induced, and therefore of the resistance resulting from it, must depend upon the condition of the interstitial electric ether (p. 243), and the distance between the molecules; since the interval of time in which the repulsive impulses propagated by the universal ether, may come into action before the electric ether impelled from the one molecule to the next shall begin to take decided effect, must depend upon these two particulars. The quality of conduction, or non-conduction of a substance, should therefore depend to a certain degree upon the mode of aggregation of the particles; more especially as different groups of particles, or compound molecules (p. 241), may offer different degrees of resistance.

If an insulated metallic ball, *a*, be placed in good conducting communication with the ground and afterward with the charged prime conductor of an electrical machine, the electricity will flow freely along the route thus opened, and the tension of the ether passing over *a* being very feeble there will be no perceptible outward movement from *a* through the surrounding air, and no sensible polarization of the particles of air; but this will no longer be the case if the charge be too large to be conducted off with facility. The effect of connecting the prime conductor with the ground when the machine is worked is to allow a free escape to the ether, which would otherwise pass toward the first layer of air particles, and displace an equal quantity from each layer in succession and transfer it to the next.

Electric Spark.—If we suppose the insulated metallic ball, *a*, still connected with the prime conductor, to be placed near another ball, *b*, which is in communication with the ground, on working the machine the ball *a*, in receiving its charge, will give rise to electric waves proceeding outward from it (p. 243), and at the same time polarize the particles of the surrounding

[8] Surfaces of non-conductors resist the flow of free electricity after the same manner as the internal mass, that is, by their molecules becoming polarized.

air. As a consequence, a certain portion of the electric fluid on the nearer side of *b*, will be urged away and pass into the ground. This side will thus be electrised negatively by induction, and react upon the air particles between the two balls, increasing their polarization, and upon the nearer parts of *a*, drawing more electricity to them. All these changes will be attended with increasing wave movements, and increasing discharges from one particle of air to another along the line between the nearest points of the balls. The electric ether in transitu between the air particles on this line, may thus come to have sufficient density to establish a certain degree of conducting communication between the two bodies, and so to convey a sudden conductive discharge along this line. This result will be partially due to the lateral expansion, which the free electricity received by the air particles on the line will occasion in their electric atmospheres. There will be two causes in operation to produce this effect, the pressure of the stream of ether passing from one particle to the next, against the atmosphere upon which it falls, and the mutual repulsion of the particles that will thus become momentarily overcharged. We have seen (p. 247) that such lateral expansion would give rise to a diminution in the resisting force of the polarized molecules.

As the positive electricity which thus passes over to *b* spreads over its surface, it partially neutralizes its negative state, and thus tends to check the flow and interrupt the passage of the spark. There is also a sudden diminution in the tension of the electricity received by *a* from the prime conductor, which is another cause of this interruption. Apparently another cause conspiring with these two is a reaction to the sudden lateral expansion above mentioned. If *a* were previously charged and insulated, and *b* brought continually nearer to it, the mutual inductive action of the two balls upon each other would initiate the electric movement above alluded to through the intervening air, which would finally result in the passage of a spark.

The light of the spark results in part from the vibratory movements in the atmospheres of the air molecules, attendant upon the discharge. (See p. 219.) Experiments by Riess, Masson, and other physicists, have conclusively established that the electric light is partially due to the passage of highly luminous metallic particles from the positive to the negative surface. The detachment of such particles, as one result of the discharge, may be explained by the discharges, or flow of electricity that must take place directly through the ball *a*, when the conductive discharge from *a* to *b* occurs. This follows from the fact that the electricity within the ball is no longer in equilibrium under the action of the electrical shell that surrounds it, and the impulses conveyed by the electric current should tend to detach the par

ticles at the end of the metallic line through the ball. The luminosity of the detached particles is to be ascribed to the vibratory movements imparted by the discharge to the electric atmospheres of the particles.

Excitation of Electricity.—There are various special modes of exciting electricity, but they are all only so many different modes of polarizing contiguous molecules; or, more comprehensively, of effecting certain disturbances in the equilibrium of electric atmospheres. The different special causes of disturbance in these cases may all be traced to some action of the molecular forces, or of heat; and, as we have seen (p. 64), heat is also, in its primary origin, one of the molecular forces.

Frictional Electricity.—The surface of one body should not in its natural condition exercise any sensible direct electric action upon that of another, unless they are brought into close proximity; certainly much nearer than in any ordinary case of mere contact. (See p. 240 and Table I.) When, however, two dissimilar surfaces are *pressed* together, especially if they are brought within the limits at which a force of adhesion would come into operation, the unequal molecular forces of the dissimilar particles, might, as will be seen in another connection (p. 250), give rise to a polarization of the particles. The development of such polarization should establish a wave movement entirely through the two bodies (p. 242), and as the result, the surface toward which the flow occurred might have an excess of free electricity while it would be negatively polarized (p. 246); that is, the outer envelopes would, on the outer side of the molecules, have an excess of electricity, and on the inner a deficiency. The reverse would be true of the other surface. This is the probable explanation of the electrical excitement which may be obtained in a feeble degree, by simple *pressure*, as in the experiment by Haüy, of pressing the smooth surfaces of fragments of calcareous spar, quartz, &c., between the fingers. But the process of excitation by friction seems to be different. The most notable distinction to be perceived between the act of rubbing and that of pressure, is, that in the former alone the protuberent parts of the two surfaces are pressed against each other laterally. The probability, then, is that this lateral pressure of the surface particles is the immediate cause of the development of electricity by the rubbing of one surface over another. Now it is easy to see that this pressure should tend to compress the electric atmospheres of the particles laterally, and so to produce an accumulation of electric ether on their outer sides, and a discharge from thence toward the other surface. If the surfaces be dissimilar in nature, or condition, the effects of this sort of action should be unequal, and the result should be that upon the one surface there would be an excess of electricity, and upon the other a deficiency.

The particles of each body, if non-conductors, should also become polarized, after the same manner essentially as already explained (p. 242), and in this condition would serve to retain the surfaces in their disturbed state (p. 245–6). The explanation here given is sustained by the effect of heat when applied to one of the surfaces, which is to dispose that surface to take the negative state, and in fact the heat-pulses should expand the molecular atmospheres (p. 73) and tend to produce a discharge of electric ether from their outer sides. The like tendency of roughness of surface may be explained in a similar manner.

The tension of the electricity obtained should depend upon the degree of polarization which the surface molecules are enabled to retain, for upon this will depend the limit of the resistance which they are capable of offering to the free flow of the electricity from molecule to molecule (p. 246). It should therefore be independent of the velocity, the pressure, and the extent of the rubbing surfaces, as established by Peclet. The electrical state of the surface, whether positive or negative, must depend upon the mutual relations of the surfaces, and the comparative energy of the disturbing force in operation upon each (p. 249).

Voltaic Electricity.—In entering upon this topic, we must first endeavor to obtain, from our present point of view, an insight into the nature of *chemical union.* When two dissimilar molecules, *a* and *b*, are brought into close proximity, the effective action of *a* upon the atmosphere of *b* may be quite different in amount from that exerted by *b* upon the atmosphere of *a*.[9] As a consequence, the relative condition of these atmospheres may be disturbed; in other words, they may become polarized. As the act of polarization proceeds, an electric current, or series of waves (p. 243), will pass through the two molecules, and at the same time they will approach each other. As this approximation continues the density of the electric ether between them will increase (p. 241, fig. 5,) and a true electric or conducting union may thus be established between them. Under these circumstances, it would seem that finally the non-conducting resistance offered by the polarized molecules (p. 245) should be in a great degree overcome, a comparative equilibrium of tension be established between the outer envelopes of the two atmospheres, and, as a consequence, the polarization materially decrease if not ultimately disappear. In this event the final union would be due chiefly, or entirely, to the molecular attraction proper, as in the case of similar molecules. It is in this final

[9] A disturbing action from the molecular attraction, may even come into operation when the molecules are beyond the range of effective attraction, that is beyond O*c*, Fig. 1. For it is to be observed that in the action of each molecule upon the atmosphere of the next, the attractive impulses prevail over the repulsive, beyond O*c* as well as between O*a* and O*c*.

condition, essentially, that we suppose a particle of water to exist. The two constituent molecules, oxygen and hydrogen, are not polarized, or but feebly so, and they are, to a certain degree, *in conducting communication with each other.*[10]

In developing the theory of the voltaic current, let us confine our attention to the case of a single cell, consisting of water, or acidulated water, between a plate of zinc and a plate of copper. We must suppose that the first effect is a mutual polarizing action between a molecule of zinc and one of oxygen, the former being brought into the positive state and the latter into the negative state, upon their contiguous surfaces. The attraction thus developed will arrange the oxygen, with its associate hydrogen molecule, on a line normal to the surface of the zinc plate. The farther surface of the molecule of hydrogen will be brought to the same positively polarized state as the zinc plate, and will act in a similar manner upon the next particle of water; and so on from one particle to another until a complete chain of polarized molecules extends to the copper plate.

In this chain, as first established, we regard each particle of oxygen as in the negative state on the side nearest to the zinc, and each associate particle of hydrogen as in the positive state on its farther side; or at least that they are brought essentially into this condition, and that the true polarization of the contiguous sides is comparatively feeble, by reason of the conducting communication between them, resulting from the condensed state of the electric ether by which they are electrically connected (p. 241). Not only does the positive repulsion that originates at the zinc plate establish, by induction, a chain of polarized water particles, in which the farther, or hydrogen side, is in the same positive state as the zinc, but it also tends to increase the density of the electric ether posited between the oxygen and hydrogen of the individual water particles of this chain, and so to urge them asunder.[11] Before the closing of the circuit, while the mutual

[10] It is not absolutely essential to the explanation of the voltaic current that the two molecules, when combined, should be regarded as devoid of polarization.

According to the received theory of the constitution of a molecule of water, we must regard the molecule of hydrogen that combines with a molecule of oxygen as compound, and composed of two simple molecules (p. 241, fig. 5); but this in no degree effects the explanation to be given, for the compound molecule, as it is not decomposed, comports itself throughout essentially as a simple molecule would under like circumstances. It is to be observed that the process of polarization above considered does not occasion an excess of electric ether upon the entire molecule of the one substance, and a deficiency on the entire molecule of the other; since when a molecule becomes polarized it absorbs upon the one side the same amount of electric ether that it gives off from the other side (p. 242).

In all these remarks the term molecule is used in the same sense as heretofore.

[11] If the constituent molecules of each water particle were not in conducting communication, then the action transmitted along the line would serve to polarize these molecules and thus to bind them more closely rather than to separate them. This objection seems to hold against Schönbein's theory.

polarizing action between the zinc and oxygen is in continual operation, waves of positive electricity spread in an indefinite series, from the zinc plate through the cell and all the media on that side of the plate (p. 243). A corresponding series of negative waves spread in every direction through the zinc plate and the media lying without it, the electric movement in these being toward the zinc plate. When the two plates are provided with wires leading away from them, we have evidence of this wave movement, and of the polarization that has attended it, in the positive and negative states of the ends of the wires. Now if the ends of these two wires be brought together, the entire series of waves which pass through the copper and zinc plates are condensed, so to speak, upon the wires, and pass through the circuit. The entire quantity of electricity that would be dissipated from the copper plates is thus brought around to the zinc plate again. The arrival of this electricity intensifies the polarizing action going on at the zinc plate, and hastens the union of the zinc and oxygen molecule. It also determines, if not before established, the completion of the line of polarized water-particles traversing the cell. As soon as this takes place, the waves that before spread through the cell are converted into linear currents. At the same time, the electricity discharged from the zinc to the oxygen passes over by conduction to its associate hydrogen molecule, and by its impulsive and repulsive action urges the latter over to the next particle of oxygen in the chain. This particle of hydrogen, with its charge of positive electricity thus received, acts upon the second particle of oxygen in the same manner that the zinc acted upon the first, and so on throughout the chain. As the detached hydrogen particle is made, by the same force which detached it, to attract the next oxygen particle more energetically, there may be no material movement of the common center of gravity of any of the pairs of particles that are separating or uniting.

The explanation of the voltaic current that has now been given seems to accord with the established laws and phenomena of the current. The primary *electro-motive force* must consist in the energy of the natural polarizing, or chemical action,[12] exerted between the zinc and the oxygen molecule, diminished by any opposing action of the same nature that may be in operation at the copper plate. It follows from the principles of induction laid down on p. 243, that the quantity of electricity in circulation, or the *intensity* of the current, must be the same at all points of the circuit. The period of time which the zinc and oxygen particles occupy in combining should be proportional to the length of the entire circuit, supposing it to be of the same

[12] This chemical action is intensified by the coöperative polarizing action of the sulphuric acid.

material and cross section throughout; actually should be proportional to the "*reduced*" length of the circuit. During this period all the electricity set in motion by the union of the two particles should pass through the circuit; or more strictly, be urged forward past each point of the circuit, in electric "waves of translation." The quantity of electricity that moves forward in a given time should then be *inversely proportional to the length of the circuit*, other things being the same. The reason that the quantity of electricity, or the intensity of the current, is *proportional to the area of the cross section of the wire* is, probably, that the number of points of the zinc plate which are cotemporaneously in action, with the same degree of energy, would be proportional to this cross section. The *tension* of the electricity circulating in the current should be the greatest where the velocity of the individual particles of the ether is the least. Possible retardations result from the electric relations of contiguous molecules in the line being such that they become more or less polarized, and so offer a resistance to the free flow of the electricity (p. 245); besides that the process of polarization is attended with a retardation. The degree of polarization that exists at any point of the current serves as a measure of the "*resistance*" experienced by the current there.

If an *electrolyte* be disposed between the ends of the wires, the theory of its electrolysis is similar to that of the decomposition of the water in the cell. The only difference is that the ends of the wires are brought by the electro-motive force into the same positive and negative states, which the natural chemical action in the cell determines upon the zinc and copper plates.

When two or more cells are employed, the natural polarizing action at each zinc plate should be enhanced, and the tension of the free electricity at the ends of the wires of the broken circuit should be augmented. Hence there should be a more energetic force to polarize and decompose an electrolyte interposed between the ends of the wires. But it does not follow that when only good conducting wires are employed to complete the circuit the intensity of the current will be augmented by increasing the number of cells; since the principal retardation of the flow occurs in the cells, and this increases in the same proportion with the number of cells. (See Pouillet, *Elements de Physique*, ii, 732.)

The *heat* developed in the voltaic current is to be ascribed to the impulsive action of the electric ether moving in it upon the universal ether. Currents, or waves of translation, are thus developed in this ether, which fall upon the central atoms of the material molecules in the circuit, or the dense ether surrounding these atoms. The impulses thus received are given off, or pass

into the molecular atmospheres, one after another, and are finally radiated off as heat-pulses. The explanation is the same as that of the molecular absorption, and subsequent radiation of the ethereal pulses of radiant heat, already given (p. 215). When the condition of things is such that the particles in the circuit become polarized, a greater amount of heat should be developed, because a part of the electric movement within the molecular atmospheres, which was before confined to their upper portions, now occurs at greater depths, where the universal ether is more dense. Thus, when the resistance to the passage of the current becomes greater, more heat is developed. Heat may also be evolved, under special circumstances, as a consequence of a compression of the molecular atmospheres, produced by the current.

We shall see, in the remaining portion of this memoir, that it is to these same *impulses of the moving electric upon the universal ether* that are to be ascribed all the external actions of the current; as attracting or repelling wires conveying currents in the same direction with the given current, or in the opposite direction,—giving motion to the magnetic needle,—developing magnetic, or diamagnetic currents, in the compound molecules of adjacent masses,—and inducing currents in wires, or metallic bodies, in the vicinity.

[To be continued.]

ART. XXVIII.—*On the combination which takes place when Light of different tints is presented to the right and left eye;* by Prof. OGDEN N. ROOD, of Columbia College.

IN 1806, de Haldat stated that when differently colored glasses were held before the two eyes, a combination of the two tints took place in the brain, and that the resultant impression was the same that would have been produced by mixing the two tints together, and presenting the compound color to a single eye. These experiments were repeated by many good observers without success. In 1841, with the aid of a stereoscope and polariscope, Dove confirmed the general correctness of de Haldat's conclusion. In 1846, Seebeck, and, in 1849, Foucault and Regnault arrived at the same result.

The testimony of these observers has then proved that a combination of the two sensations does take place in the brain, and that the resultant impression is, in the cases they examined, generally similar to that which would be produced by the presentation of the two tints to a single retina: but with what exactitude the resultant tint obtained by the binocular method agrees with that produced in the ordinary way by rapid rotation, or by

Helmholtz's method, or how far it is in point of fact possible to predict, by the binocular method, what the result of the true mixture of different tints willl be, is a question which, so far as I know, has never been studied.

The following set of experiments was undertaken for the examination of this matter.

The first method pursued was as follows: Light of different tints, complementary or not, was presented to the right and left eye, and the resultant tint gained by binocular vision was noted: afterward, by the method of rapid rotation the two tints were mixed and presented to a single retina. In many cases the results obtained by the two methods were nearly identical, but in others there was found to be a considerable variation, so that often it was impossible to predict by the binocular method what the exact resultant by rotation would be; it sometimes being of a tint which was not at all present to the eye or mind during the binocular union.

An open lenticular stereoscope was provided with a white card board, in which were cut two square apertures one half an inch in diameter, they being placed at such a distance apart that their binocular union readily took place. The ground glass was removed, and the stereoscope held horizontally in front of a rotation apparatus to which was attached a circular card-board disc, with its two halves painted in different colors. The disc remaining at rest, the dividing line of the colors being vertical, light of different colors entered the two apertures in the card-board, and after their binocular union had been effected, the resultant was noted: the disc was then set in rotation, the stereoscope remaining in its position, and a true mixture of the two tints was obtained and compared with the first result.

Helmholtz has shown that vermilion represents the red of the solar spectrum up to the line C, that red lead answers to a portion between C and D, but not reaching up to D; also, that the pure yellow portion of the spectrum is imitated by the chromate of lead, the less refrangible blue by Prussian blue, and the more refrangible blue by ultramarine. Accordingly discs were prepared with these substances, and, in addition, some others were used: viz.: a disc colored with a bright and pure yellow, a little more refrangible than chrome yellow, but still not at all greenish yellow—its tint was about that of 107·7, in Kirchhoff's chart: for the production of a greenish yellow disc, gamboge, with a minute portion of Prussian blue, was used—its tint was that of 113·2; more of the latter color, with gamboge, gave a good greenish-blue disc. Red discs, slightly purplish, were used; also discs colored with emerald green, 170· in the

chart. In addition discs of a golden yellow, being some of the same paper I had previously used in the stereoscopic imitation of the luster of gold—its tint was 102·7; and finally purple discs were made by the mixture of ultramarine and crimson lake.

Comparison of the results obtained by the binocular and actual combination of different tints.

1. When one-half of the disc was covered with vermilion, the other with emerald green, the binocular combination was rather difficult, and the result was sometimes a red gray, sometimes a green gray; but when the disc was set in rotation, its true resultant tint was found to be a decided green, which could not have been predicted from the binocular union. That red and green should give a decided green by rotation may seem singular, but it must be remembered that I was not able to vary the *proportion* of these two tints, *each half* of the disc necessarily being of the same tint. In this case then, it merely happened that the intensity of the green was such that after a portion of it had been neutralized by the red, enough remained to give a strong green coloration to the disc. Similar results, given below, will be passed over without notice: they really furnished a delicate test for deciding whether the binocular resultant agreed with that produced by rotation, *preconceived notions* of complementary colors being thus avoided, as it was impossible to know beforehand what the result would be, when the rotation experiment was tried with *equal* surfaces of the colored papers. In several cases the tints on equal portions of some of the discs nearly exactly balanced each other, so that the resultant tint by rotation approximated to a white or neutral gray.

2. Vermilion and ultramarine gave with some ease the *same* tint by the two methods of combination; viz., a red purple: the tint was however rather more red by rotation than would have been expected from the binocular combination.

3. Vermilion and yellow (107·7) gave by rotation a tint containing much more yellow than would have been expected from the binocular examination.

4. Vermilion and purple gave the same tint in both cases, viz., a red purple.

5. Vermilion and black gave approximately the same result in the two cases; the disc was however less red by binocular vision than by rotation.

6. Vermilion and white gave the same result in both cases.

7. Orange, (red lead,) and purple gave a good approximation to the same result in the two cases; the tint was more red by rotation than was expected.

8. Orange and black gave a more neutral tint by binocular vision than by rotation.

9. Orange and white gave the same tint.

10. Chrome yellow and emerald green (170·) gave the same result, viz., a yellowish green.

11. Chrome yellow when combined with black by rotation gave a more decided yellow than would have been expected from the stereoscopic union of the two components.

12. With white the tint was the same in each case.

13. Emerald green with white gave the same tint in both cases; with black the tint was not nearly green enough in the binocular union.

14. Ultramarine with white or black gave about the same result in each case.

15. Purple and yellow (107·7) by rotation gave a decided yellow: the resultant impression from binocular vision was much more neutral.

It will be noticed that in all these experiments the colors of the discs were such either in intensity or nature that by rotation a neutralization was not to be expected; I now pass to those cases where the tints were complementary or approximately so.

1. Golden yellow and Prussian blue gave about the same tint in the two cases, viz., a nearly pure gray.

2. Vermilion and greenish blue gave approximately the same tint in both cases, viz., a gray slightly purplish: the tint by binocular vision was a little more neutral.

3. Greenish yellow and purple gave sometimes the same tint in both cases, viz., a gray with a tinge of green.

4. Emerald green and purplish red gave by rotation a good neutral gray, but I could not, by binocular vision, get exactly the same result, the tint of the resultant being either too red or too green. Both of the original colors used were very pure and bright.

Lastly, I give two combinations on which many experiments were made, viz., first, chrome yellow and ultramarine. By the stereoscopic union of the components a pure gray was obtained, or at least a gray without any tinge of red, but by rotation the color was of reddish copper hue, which could not have been anticipated in using the other method of combination. Also, yellow (107·7) and ultramarine often gave in the binocular method a neutral gray, while the true union of the colors gave a flesh tint.

It will be seen from these experiments, that while a combination does in reality take place, yet that the resultants in the two cases may very considerably differ, and farther, that there is a tendency in the binocular method to consider the resultant im-

pression a neutral one, when the method by rotation shows that this is not truly the case. This is illustrated in a striking manner by an experiment previously published by me, which I have lately repeated a number of times with the same result. A yellow glass is held before one of the eyes, a blue glass before the other, when, as Brücke rightly states, a landscape thus viewed, seems of a neutral gray if the attention be equally directed to the two impressions. When, however, the two glasses are attached to opposite openings in a blackened card disc, and set in rapid rotation so that the tints are truly mixed and a true resultant obtained, this is found (with the glasses I employed), to be a strong purple, though nothing of this kind is seen or suggested in the binocular use of the glasses. It is probably this tendency to consider the resultant neutral, that makes it so easy to combine, in binocular vision, tints produced by polarized light, and to perceive that the resultant is white.

It may be remarked in passing, that when the stereoscope is held as described before the two halves of the colored disc, and the latter is caused to revolve slowly, that the aperture in the card board appears extraordinarily lustrous, so much so that it would be possible by a little artistic arrangement of the accessories to deceive persons with the belief that they were looking at the surface of a polished metallic mirror.

Binocular union of complementary tints produced by polarized light.

For the purpose of experimenting on this point, I arranged a binocular polariscope, consisting of a large plate of polished glass resting on a black cloth, two Nicols' prisms being fastened before the two eyes in the right position, the principle section of one of them making an angle of 90° with that of the other. As objects, plates of selenite were used, fastened behind apertures in black card board. Films of common mica cannot well be employed on account of the rapid change of the tint by the least inclination. As the colors were exactly complementary, the resultant by binocular vision should always be pure white.

Results with polarized light.

1st, Red with greenish blue of 2nd order combined with some readiness to make a white. The same was true of these tints in the 3rd order; here the resultant white was *very easily* obtained. The same tints in the 4th order gave an excellent result; the components were of course pale, but the resultant white was proportionally steady. So also with the same tints in the 5th order.

2nd, Golden yellow with its complementary blue of the 2nd order, gave a good result. The same tints of the 3d order also readily neutralized each other.

3rd, Yellow and indigo blue of 1st order gave easily a very good white. The same tints in the 2nd order united with more difficulty. Union was about the same in 3rd order.

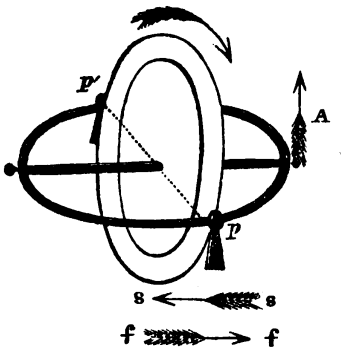
4th, Greenish yellow and violet of 2nd order combine to a white with some difficulty. The same is true of these tints belonging to the 3rd order.

5th, Green and purplish red of the 2nd order, united with moderate ease. The same tints in the 3d order united with great ease, and the resultant white could be retained very steadily.

From the above it will be seen that the union most easily takes place when the complementary tints have a low intensity. The same result can also be produced with the colored discs by shading them, when the two impressions much more readily fuse into a neutral single one.

ART. XXIX.—*On an experiment with the Gyroscope;* by Prof. O. N. ROOD, of Columbia College.

IF a gyroscope be suspended in equilibrium by pivots at p and p', and the disc be set in rotation in the direction indicated, and an attempt be made to cause it to rotate about a new axis $p'\,p$, in the direction shown at A, the disc will tend to turn in the direction $s\,s$, and if the pivots $p\,p'$ are immovable, pressure against them will be generated in the direction $s\,s$, which will continue till the disc has been rotated through an arc of 90°. If the rotation around the new axis $p\,p'$, be still continued, the pressure on the pivots $p\,p'$ becomes reversed, and the disc tends to move in the direction $f\,f$, and retains this tendency till it has passed over an arc of 180°, when this tendency is again reversed and becomes the same as in the first case, its sign not being changed till the disc has passed through an arc of 180°.



Hence, during the entire revolution around the new axis $p\,p'$, a varying pressure is exerted on the fixed pivots, alternately to the right and left hand.

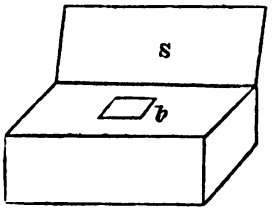
It occurred to me that by causing a gyroscope, suspended in this way, to rotate *rapidly* about a new axis as $p\,p'$, its original motion might thus be destroyed in this indirect manner. A gyroscope was selected, which, when set in rotation by a certain force and allowed to remain undisturbed, continued in motion for 16 minutes. It was connected with a set of multiplying wheels, and the instant after it had been started in rapid rotation, it was forcibly caused to revolve about the new axis $p\,p'$,

at a rate of forty revolutions per second. The resistance experienced was considerable, such as in two cases to eject the disc from its mounting, and the motion of the disc was found to be arrested in about twenty seconds, or in the $\frac{1}{4\frac{1}{5}}$th portion of the time it otherwise would have occupied.

ART. XXX.—*Description of a simple apparatus for producing lustre without the use of Lustrous surfaces or of the Stereoscope;* by Prof. OGDEN N. ROOD, of Columbia College.

IN attentively looking at plane polished surfaces of aventurine glass, I have often been for a moment unable to determine by binocular vision exactly where some of the imbedded crystals were placed, they seeming for an instant to be suspended in the air above the glass. This uncertainty depends on the fact that it frequently happens minute crystals will reflect bright beams of light to one eye but none to the other, so that one of the binocular components is wanting. The apparatus described below was contrived so as to produce the same effect even to a much greater degree, as well as to show the production of simple lustre.

A box was constructed three inches long, one and a half inches broad and deep; this was blackened, and provided with a black screen at S; the side directly under S was left entirely open, and allowed the light from a window or candle to enter. At *b* was an aperture $\frac{1}{2}$ inch square, which was covered by a black card-board or piece of blackened brass foil, in which a hundred or more pin-holes, or better, minute triangular apertures, were punched. Directly below this on the blackened surface of the floor of the box, were sprinkled about a hundred small pieces of white, red, and green paper, each being about $\frac{1}{15}$th of an inch square. When thus arranged and properly illuminated, many of the apertures appeared lustrous, like small brilliant grains of sand, from the reason that, owing to the construction, light of different intensity or color was presented to the two eyes.



If now a second blackened diaphragm of brass foil, perforated with many minute triangular apertures, carefully prepared so as not to have an indented surface, be placed above the first set of apertures, at a distance of $\frac{1}{2}$ or $\frac{1}{4}$ of an inch, brilliant points of light are seen either by one eye or the other, light from the same opening rarely reaching both eyes. It now becomes impossible to decide on the location of these points, and they often seem suspended in space, somewhere in the interior of the box, but exactly where the observer cannot determine.

ART. XXXI.—*Remarks on the Beatriceæ, a new Division of Mollusca;* by ALPHEUS HYATT, Jr.

DURING an expedition to the island of Anticosti in the summer of eighteen hundred and sixty-one, undertaken by Mr. A. E. Verrill, N. S. Shaler and myself, and prosecuted under the auspices of the Museum of Comparative Zoology, in Cambridge, numerous specimens of Beatriceæ were collected at English Head, West Point, near Junction Cliff, on the shores of Ellis Bay, and at several points between Salmon River and Fox Bay.

I have satisfied myself by a careful inspection of this large collection during the past two years, that these remarkable remains are not tree-like fossils, as originally described by Mr. Billings of the Canada Geological Survey, but a new and interesting order among Mollusca closely allied to the Orthoceratites.

I have in course of preparation a monograph upon this new order, but, as unforseen difficulties will probably delay its completion, I hope that even so slight a sketch as is here given of the most prominent characters may not prove uninteresting.[1]

The Beatriceæ are long cone-like bodies, composed of three distinct parts, or layers, in the following order: (1) A central chain of small hollow chambers; (2) a succession of concentric coniform layers; (3) an external or sub-epidermal layer.

(1.) The central chambers are imperforate, generally deeply concave, and set one upon another like a pile of Chinese teacups. The tops, as also the bases of the coniform layers, are turned toward the larger end of the fossil, and completely closed by the bottoms of the succeeding cavities. They invariably occupy the axis of the encircling coniform layers, and have thin and sharply defined walls, resembling the fossilized nacreous septa of an Ammonite or Nautilus, and are not continuous with each other or with the adjoining coniform layers.

These cavities present many variations of form in the same individual, especially in *B. undulata*, sometimes in this species even becoming reversed, convex instead of concave, or broken up into several minor chambers. Such irregularities, however, are merely local, arising from mutilation or other disturbing

[1] Although any adequate acknowledgment of the kindness shown to the members of the expedition by Prof. Louis Agassiz, Sir Edmund Head, then Governor-General of Canada, and Mr. George Ticknor of Boston, all of whom did their utmost to aid us in accomplishing our designs, would be more appropriate in the monograph alluded to above, I cannot refrain from availing myself of the present opportunity to express our sincerest thanks for the sympathy and assistance we received from them. My personal thanks are also due to my father, Mr. Alpheus Hyatt, Sr., of Baltimore, Md., to whose generosity I owe the privilege of attending the expedition.

causes, not affecting any individual as a whole. In *B. nodulosa* they remain, with but few exceptions, constantly concave, and have regular forms.

(2.) The coniform layers are separated in fully grown specimens by intervening spaces near the central line of cup-like cavities, which become gradually shallower upward and outward, and finally disappear between the closely appressed bases of the overlapping layers.

In consequence of the great length of the cones or layers, the upper or narrower portions of the intervening spaces in *B. nodulosa* are parallel with the external surface, and appear as narrow streaks of blue limestone. In the young of *B. undulata* and *B. nodulosa* the intervening spaces are not present, but the layers are easily distinguished and have the same abrupt cone-like terminations as in the adult. The cones are formed of numerous thin laminæ, whose surfaces being but partially in contact, form oblong, angular, interstitial spaces or cells, with two concave, and one convex side, the latter always turned outward. They are largest near the center, giving a porous, spongy aspect to that part of the cone immediately around the axial cavities, but become gradually smaller outward until they are not distinguishable by the naked eye between the closely packed laminæ.

(3.) The sub-epidermal or outer layer is of a dark brown color, and in well preserved specimens is ornamented and continuous without annular marks or furrows.

It was probably the structure of the overlapping partitions or cones, and the parallelism of their upper parts, which in a transverse section closely resemble annular rings of growth, that led Mr. Billings to class Beatriceæ with plants. The cellular structure is, however, not due to the presence of true cells pervading the substance of the cones, but to the regularly formed interstices, described above, between the thin laminæ of which they are composed; and the annular appearance is consequent to the overlapping of the cone-like concentric partitions, which were deposited from within, and cannot, therefore, be considered plant-like exogenous layers.

We were fortunate enough to obtain a specimen of *B. undulata* at English Head which demonstrates that the cone-like partitions are the walls of chambers that were successively occupied by a part, if not by the whole body of an animal.

It is a fragment of the larger end, broken across the terminal chamber, exhibiting the outline of the animal parts, contained within the cavity at the time of the burial of the Beatricean in its Silurian grave. The centre is filled with a core of carbonate of lime from which a variable number of lobe-like longitudinal folds impinge against the sides of the vesicular cone, corres-

ponding to angular external ridges projecting from the outer surface. The spaces between the folds are acutely angular, and answer to an equal number of broad shallow channels between the external ridges. The correspondence of this core with the exterior renders the supposition of its being the fossilized form of those parts of the animal that secreted the shell extremely probable. The differences, also, which exist between the lobe-like folds and the corresponding, angular, external ridges between the vacant, deeply cut, angulated interspaces, and the shallow, external channels, show that the inclosed body was surrounded by a mantle. If the shell had not been secreted by an enveloping mantle, the outline of the exterior would have corresponded more closely to the internal irregularities of the surface of the body, and there would have resulted a shell with prominent lobe-like folds, instead of depressed, angular ridges, and deeply cut angulated interspaces, instead of broad, shallow channels.

In attempting to refer the Beatriceæ to their proper place in the animal kingdom, we were at first much impressed by their great likeness to Cystophyllum; Mr. Verrill, however, after having made extensive comparisons between them and all the seemingly allied forms of Radiata in the Museum of Comparative Zoology in Cambridge, became convinced that their apparent affinity to the Radiate branch was nothing more than an analogical resemblance; extending, however, in a most extraordinary manner throughout the internal parts.

Mr. Verrill separates them from the Radiata principally because of the absence of any radiated structure, and the presence of the external, subepidermal, ornamented layer; features which he considers as irreconcilable with the structure of that branch.

An additional objection of considerable weight, may be founded upon the internal organization of the specimen, from English Head, above described.

This, with the fossilized remains of its former occupant still preserved within the chamber, the last of a series of other cone-like chambers, when considered in connection with the continuity of the sub-epidermal layer, and the absence of all annular furrows, shows conclusively that the shell was secreted by a Mollusk.

The Beatriceæ are very like the Hippuritidæ, both in general form and the arrangement of the component parts of the shell; but here again it may be demonstrated that the resemblance is not so close as it at first appears to be.

The geological horizon in which they occur, without other evidence, would alone be sufficient to render their affinity with the Hippuritidæ exceedingly doubtful; but, besides this, the structure, evidently, is not so closely allied to that of the Hippu-

rite as to the Cephalopod. The shell of the Hippurite is composed of three parts: first, the inner septa, second, the outer layers, which frequently form a porous mass,[3] and third, an external sub-epidermal layer.[3]

The inner septa, which supported the principal part of the body of the Mollusk, form large cavities, while the second part is made up of laminæ laid on by the mantle margin, or at least that part corresponding to the mantle margin of the Lamellibranchiates.

In Beatriceæ, on the contrary, the inner septa did not contain the body of the animal, and there are no marks whatever of a mantle margin. This objection could not be urged against their affinity with Caprinella and the like, in which the central cavities are small; but from these they may be separated by the absence of all ligamental or muscular impressions and the mode of forming annular, cellular partitions, composed of numerous laminæ, instead of a continuous series of porous or tubular laminæ.

The Hippurites, Caprina, and the like, were, with few exceptions, attached to the surfaces upon which they lived or to each other, and had short, thick, cone-like forms, affording broad bases of attachment, whereas the Beatriceæ were long, thin, almost tubular bodies, resembling the Orthoceratites, and entirely unfitted to support themselves in fixed positions.

We saw hundreds of *B. nodulosa* and *B. undulata* "in situ," but nowhere any indications of attachment, either to the rocks or to each other.

After close comparison with all the types to which these singular fossils appeared to have any resemblance, I have at length considered myself warranted in considering them as Cephalopods more closely allied to the genus Endoceras, than to any other group of that class.

They differ greatly from all the Tetrabranchiates, in the open structure of the partitions or septa between the chambers, and this character, together with the absence of a siphon, and the cone-like form of the septa, demands that they should be separated as a distinct order, for which I propose the name of Ceriolites.[4] Although distinct as an order because of the differences in the form and structure of the septa, arising from their great length, and the loose way in which the laminæ are arranged, the parts may be compared point for point with similar parts of Endoceras.

We may imagine the cone-like septa of a Beatricean to be spread apart, until their surfaces should be parallel throughout the shell; they would then be entirely separated by hollow

[3] Woodward. Hippuritidæ. Quar. Jour. Geol. Soc.; London. v. 15, part 1, page 41.

[3] Woodward. Quar. Jour. Geol. Soc., p. 46.

[4] κηριον, a honeycomb, λιθος, a stone.

chambers, as the septa are in Endoceras, and if, at the same time, the central cup-like cavities were supposed to be prolonged into cones, we should, without violence to the typical idea of the organization, have transformed the Beatricea into a shell separable from the Endoceras by only one character, the vesicularity of the septa.

The analogies which the Beatriceæ have with plants in their general aspect, with Radiates in their internal vesicular structure, and with Hippurites in the arrangement of the parts, are so close as to entirely bury, as it were, their true affinity with Cephalopoda, which only becomes obvious after diligent comparisons.

This complex nature establishes the Ceriolite as one of those strange types that not only combines, as does the "*Phoronis hippocrepia*,[6] the Annelidan homomorph of the Hippocrepian Polyzoa," the characteristic features of other groups belonging to different branches of the animal kingdom, but also of groups in other classes within its own sub-kingdom.

All these analogical relations are, so to speak, retrospective; they refer to lower ranks of life than the one in which Beatricea itself is found.

There still remains a question which I have not been able to solve in a satisfactory manner with the specimens at my command. Are the Beatriceæ internal or external shells? This problem, so difficult to settle conclusively with regard to the Orthoceratites, is equally puzzling in the structure of Beatriceæ. Their extreme length, cellular structure, and the irregularity of the ornamentation, would be almost determinative in favor of their being internal shells, were it not for the aspect of the cast of the animal in the terminal chamber of the specimen from English Head, which proves that a large portion, if not the whole of the body, was contained within the shell.

If, however, upon further examination, they should prove to be internal, which I consider doubtful, the cycle of their analogical characteristics would be completed, and they would refer not only to lower types, but have certain features in common with the more highly organized dibranchiate Cephalopods.

There are but two known species of the order Ceriolites, both occurring in the Silurian strata of Anticosti.[8]

Mr. Billings, in describing the species, states that they differ only in the external ornaments or markings.[7]

[6] Allman's fresh-water Polyzoa. Ray Soc., 1856, p. 55.

[8] Since writing the above I have been indebted to Prof. J. D. Dana for the loan of some fragments of a Beatricea, resembling *B. undulata*, from Kentucky. The remoteness of this locality affords ground for hoping that the remains of other species may be brought to light in some of the intermediate Silurian basins.

[7] Billings's Report, Canada Geol. Survey. 1853-56. p. 343.

He, however, probably did not possess so fine a series of natural sections as it was our good fortune to collect, or he would have discovered the very considerable differences which exist in their internal structure, by which they may be as readily distinguished as by the external characters. I think, therefore, it may not be amiss to redescribe the species, giving such peculiarities as have not already been mentioned.

Order CERIOLITES, Hyatt.

Family CERIOLIDÆ, Hyatt.

Genus Beatricea, Billings.

Beatricea nodulosa, Billings, is a long cone, tapering very gradually, the central chambers occupying from a fourth to a third of the transverse diameter in adults; in the young they are larger proportionally, varying from one-half to two-thirds of the breadth of the whole shell. The coniform layers are nearly parallel; the inclination at the lower part, as they trend outward to the circumference, being decreased very slowly. The external shell is closely set with tubercles and covered with granular points. The size, as nearly as could be inferred from fragments, is not over four feet long, by from three to five inches in diameter at the larger end.

B. undulata is a much larger species, one fragment found by the expedition being thirteen and a half feet long, by eight and a half inches in diameter at the larger end, and judging by the inclination of the sides, the length of the entire shell, when living, was certainly not less than twenty feet. The chambers are very small, frequently in adults not occupying more than one-tenth of the transverse diameter. The coniform layers are more widely separated in the adults than in the young; their inclination as they trend outward to the periphery is more decided, and they nowhere assume the parallel appearance of the same parts in Beatricea nodulosa.

The exterior is granulated and ornamented by ten or more prominent longitudinal ridges and intervening broad, shallow channels.

Cambridge, Mass., Dec. 6, 1864. (Residence, Baltimore, Md.)

Art. XXXII.—*The Albert Coal, or Albertite, of New Brunswick;* by Charles H. Hitchcock.

The nature of the Albert coal and the mode of its occurrence in the strata have been vexed questions in geology. Its beautiful appearance attracts the eye, while its pecuniary value gave rise to the litigation which occasioned the delivery of the diverse opinions. In this as in so many other difficult subjects time has developed much truth, and shown us that we must not insist too strongly upon seemingly well-established theories.

In this communication I propose to describe briefly the geological features of the Albert coal vein and the accompanying rocks. I shall, whenever necessary, refer to the facts observed by others in this locality, but rely chiefly upon my own observations made in 1861 and 1864, as well as upon hints derived from persons of intelligence living in the vicinity. For a knowledge of many facts relating to the distribution of the different strata, I am indebted to Mr. James Blight, of Hillsborough, N. B. It is necessary to be dependent upon others for some knowledge of the internal structure of the Albert Mine, because the Company will not allow any scientists to examine their property below the surface. I use the word *coal* as a matter of convenience, not necessarily in strict propriety.

There are four different mining properties in Hillsborough, situated upon veins of Albertite:—the Albert, (the only one worked extensively and thoroughly proved), the East Albert, the Prince of Wales, and the Princess Alexandra. The second lies east of the first, and the others north and south of the same. Hillsborough is situated upon the west bank of the Petitcodiac river, near its confluence with the Bay of Fundy.

The rocks are of Lower Carboniferous age, and belong to the Acadian coal series. Several species of *Palæoniscus, Lipidodendron, Lepidostrobus, Spherædra, and Stigmaria* occur in the shales and sandstones. Two or three miles southwest from the Albert shaft there appear older crystalline rocks, such as syenite and metamorphic slates; constituting the easternmost point of the extension of these rocks from the vicinity of St. John. The lowest rock in the Carboniferous series is the Albert shale; but I cannot state whether it crops out near the syenite. It probably does not appear anywhere on the edges of the coal basin. Nothing similar to it occurs in the Joggins' section. It should be expected to occur near and below the numerous deposits of gypsum in the eastern provinces.

This shale contains a large amount of hydro-carbonaceous matter. Certain layers of it at the "Caledonia Oil Works," by a rude process, have yielded thirty gallons of refined illuminat-

ing oil to the ton. The greater portion of the shale will sustain a fire without the aid of other fuel. Other layers are more bony, and others still highly ferruginous. It contains immense numbers of fossil fish, almost enough to make one imagine they gave the shale its inflammable character. The surfaces of many layers are glazed. The rock is very weak and abounds in small contortions of the strata. It appears in three localities. The largest has the Albert shaft in its centre; being exposed a mile or so in length, and showing best in the low ground. Small patches of shale may be seen on Peck's and Stinking Creeks, besides an unknown amount farther west at the oil-works. This series cannot be less than one thousand feet thick, as it has not yet been cut through by the shaft, and the general inclination of the bedding is very small.

The second group of strata is a conglomerate, separated from the first by a narrow bed of sandstone. Bits of Albert coal and shale constitute component parts of certain coarse sedimentary strata of this group, and render them oleaginous. The thickness is unknown, probably from 100 to 200 feet. Between the Albert shaft and the Petitcodiac, from three to four miles, this group prevails at the surface, except it be a small area of shale in a deep valley.

The next layers in ascending order are red marl and a bluish gray siliceous limestone. Above these are immense deposits of gypsum, from which most excellent plaster-of-paris is made for exportation. The highest rock in the series in this vicinity is another great mass of reddish conglomerate and sandstone.

There appears to be an anticlinal axis passing through this region, trending nearly ten degrees north of east. I have traced it from the Albert shaft to the Petitcodiac river, and have reason to believe it extends much farther, both west and east. The anticlinal structure is shown in three ways. *First*, the testimony is unanimous that there is an anticlinal in the Albert Mine. *Second*, the conglomerate dips in opposite directions in seven equi-distant localities examined over this area, but the dip is small. *Third*, the rocks succeed one another on both sides of the supposed anticlinal line in the ascending order mentioned above. On the north, we find above the shale, conglomerate, red marl, limestone, gypsum and sandstone. On the south, the order is the same with an opposite dip.

There appears to be a fault along or near this axis, displaying the usual phenomena of anticlinal fissures, and its location may easily be accounted for. The great plicating force acting from the direction of the ocean crowded the Hillsborough rocks northwesterly from the Bay of Fundy, crushing them up into a fold. The shale was not strong enough to sustain the bending; hence its layers were much twisted and fissured along a central line.

It is not likely that any of the shale rose above the surface at the time of flexure; and now that a portion of it has been laid bare by the removal of the upper rocks, the fissure and contortions show more plainly than in the overlying tough conglomerate to the east, which being narrower will naturally contain less of any foreign matter that has subsequently been injected into it.[1]

Some relics of this great force are now perceptible at the Albert mine, showing that the pressure is still exerted, perhaps as strongly as at any time of its manifestation. This phenomenon is much more noticeable than the "swellings of the walls," so common in deep mines. As soon as the coal is removed, strong timbers are put in to keep open the drifts, but in a short time these cross-pieces are split and crushed by the powerful force pushing the walls together. And when the timber is destroyed, the walls shut in, closing with a great noise as loud as thunder for hours, but not so near the workmen as to interfere with their progress. Not merely do the walls close, but frequently large fissures are produced behind the vein, so that the miners can clamber up and down new crevices. Large masses of rock are sometimes detached from either wall, in consequence, filling up the drift. We might explain the falling of fragments by gravity, but not so easily the crushing of the timbers.

The coal shows the effects of the crushing process no less plainly. It is much broken, even to grains, and needs no pick for its removal from the vein. It will flow as easily as heaps of corn, and therefore pains are taken to tap the vein in the right place, and at the proper time. If by oversight the main shaft is not walled up very tight, the coal will stream through the crevices between the beams, to the great inconvenience of the workmen.

The first outcrop of Albert coal was discovered by John Duffy, fifteen or sixteen years ago, in a deep ravine on Frederick's brook. The vein was about four feet wide, but by working upwards twenty-five feet into the bank, it thinned out to two or three inches. Duffy drifted about 300 feet on the course of the vein above the water-level, and sunk a shaft sixty feet, where the coal is said to have attained a width of ten feet. He then disposed of the property to Cairns, Allison & Co., who held it at the time of the litigation, but have now mostly transferred their shares to other parties, holding them under the same charter.

To describe the numerous variations in the course, thickness and shifts in the Albert workings, so far as known, would be unnecessarily tedious. Percival describes them for the first 200

[1] The fact of the existence of the coal in a vein occupying an anticlinal dislocation was maintained by Messrs. Robb and R. C. Taylor in their Joint Report upon the Albert Mine in 1851. See *Proc. Am. Phil. Soc.*, vol. v, p. 242. Their report was accompanied by chemical analyses by Dr. C. M. Wetherill, who made the material a variety of asphaltum and named it *melan-asphalt*.

or 300 feet of the descent, occupying more than three pages of the size of this Journal in a little more than catalogue style of enumeration. I am assured by the manager, Capt. Byers, that, from that depth to the bottom of the shaft, 950 feet, the character of the irregularities has not changed. In brief, the peculiarities of the mine are the following.

The general course of the vein is N. 65° E., but the coal is repeatedly heaved southward by small faults. Its inclination is northwestward from 75° to 80°, often vertical. The body of the vein is extremely irregular, constantly expanding and contracting, both. laterally and vertically. What is too narrow to be worked in one level enlarges to six and twelve feet, a hundred feet lower, or the reverse; but in general the width increases in following down the vein. At the time of Percival's examination, the vein was not considered workable 170 feet west from the old shaft. At lower levels the yield is renumerative 700 feet west and 2300 feet east of the new shaft, which lies several rods west of the first. In consequence of the uncertainties in the character of the vein, it is found necessary to accumulate a large supply of the coal during the suspension of navigation, so that there will always be enough stock on hand in the warmer months to load the vessels without delay. Whenever a displacement is met with the vein is not lost, because a film of the coal remains in the slip to indicate the location of the heaved portion. The widest part of the vein is said to be twenty-eight feet.

The narrow portions of the coal are invariably contained in a harder rock; where the rock is softer the vein is larger. "Horses" are common. In such cases the cavity above, out of which the horse fell, is found to be filled with coal; so that the width of the coal at that level is equal to the usual width *plus* the width of the horse. Numerous small branches run off into the shales from the main vein. These are short and might be described as irregular and branching spines from a main stem. Many of the fragments of rock taken from the mine show these small injections. The most striking proof of the proper character of the mass is afforded by the edges of the strata in contact with the coal; they are coated with Albertite, while the surfaces are covered only when enclosing one of the small lateral branches, a few inches long.

With the facts now presenting themselves to the explorer, I think no one would call the Albertite mass a bed. It occupies an irregular fissure along an anticlinal line, and the deep workings have failed to develop the lower anticlinal branching of the coal anticipated by the advocates of the bed theory. The numerous branches are unlike any phenomena connected with beds. There is no fire-clay to form the floor of a bed; and, in addition, the common adherence of the coal to the edges of the strata, rather than slickensides—I do not mean those in the hori-

zontal slips—seems to complete the evidence that the coal does not occur between stratified planes. To disclaim a bedded character casts no reflections upon the observations of the distinguished geologists who have decided otherwise; because they started with erroneous premises. To them the idea of coal in a vein was preposterous. It appeared as great an anomaly as it would be now to find Niagara fossils in the Potsdam group.

The vein-character of the deposit is seen more distinctly in the smaller openings. On the East Albert property two shafts have been commenced near the anticlinal line in the conglomerate over the shale. These reveal, at the depth of thirty feet, nearly six inches width of a richer and more beautiful coal than the Albert, gradually thinning out to the width of coarse paper at the surface, and most unequivocally cutting vertically across nearly horizontal layers of sandstone. As before, we have here the phenomena of shifts constantly working the vein southward, and a slight leaning in the same direction. Following the line to the Petitcodiac, there are seen other openings upon the vein of less extent.

The two veins crossing the anticlinal are very interesting. Upon parallel lines about a mile apart, their course is N.E. and S.W. One appears to intersect the principal vein very near the Albert shaft. The intersection of the other is concealed by a great depth of alluvium. It cannot be said that the coal is likely to prove more abundant at these intersections, as is the case at the union of metallic lodes, yet the similarity of the two classes of veins is such as to warrant the exploration.

These two side veins cut the strata nearly at right angles to the dip. The following is in general their nature, as observed in half a dozen openings. The conglomerate with a gentle dip is traversed by vertical joints, two of which parallel to each other, and from two to seven feet apart, are filled with threads of Albertite, occasionally enlarging to bunches an inch thick. Between them are branching threads of the same material, joining the lateral seams at various angles. I think there are no branches upon the outside walls. The whole field reminds one of an area of tin veins. Like the others, this vein-field leans slightly southward. It will be interesting to watch the development of these veins to see whether they will develop like the Albert. Their persistency and ability to cut through the strata render them worthy of attention. But no one ought to expect to discover a large vein till the threads have been followed down to the subjacent shale. The surface at the Albert mine is more than a hundred feet lower geologically than the bottom of the East Albert shaft where the coal is nearly six inches wide.

I think the following conclusions may be drawn legitimately from the foregoing and kindred facts.

1. *The Albert coal occurs in true cutting veins, not in sedimentary beds like ordinary coal.*

2. *The Albert coal was originally in a liquid state, was injected into vertical fissures, and subsequently hardened into a substance resembling jet.* The liquid may have been derived from vegetable accumulations, or possibly in part from the abundant ichthyic remains in the shales. Whether the shales were originally oily, as now, and the fissures subsequently filled with a viscid fluid derived from them, or whether the charging of the fissures imparted an inflammable character to the rock, I will not conjecture, though it is easy to satisfy one's own mind. The cavities of the Albert coal occasionally hold liquid petroleum, and those in the adjacent shales more often. A few quarts of petroleum have been brought up from borings along the line of both the Albertite veins on the east side of the Petitcodiac. With the hardening, the hydro-carbonaceous liquid received oxygen into its composition.

3. *The Albert coal must be compared with the asphaltic and bituminous veins found in the Quebec group in Canada.* It there "fills veins and fissures in the limestones, shales, sandstones, and even in the trap rocks which traverse these." "In other cases, it fills fissures several inches in diameter, so that it has been mistaken for coal, and attempts have been made to work it at Quebec and elsewhere. The mineral is never, however, in true beds like coal, but is always confined to veins and fissures which cut the strata." "The matter is of a shining black color, very brittle, breaking into irregular fragments with a conchoidal fracture." (*Geol. Canada.*) The Quebec coal is like the Albert in the small amount of the ashes, but contains more carbon.

4. *These carbonaceous veins are analogous to veins of petroleum.* The borings for petroleum in Ohio and Western Virginia are most successful along lines of fracture, particularly an anticlinal axis. The description of the chasm filled with oil would undoubtedly be given in words similar to those used respecting the Albert vein, if we could sink shafts and drive on the course. The views of Prof. Andrews in this Journal, ([2] xxxii, 85,) respecting the location of petroleum, are very just, and show that it often occurs along anticlinal faults. The immense yield of many oil-wells certainly suggests the presence of more than the "horse-cavities" filled with the liquid.[2]

5. The carbonaceous veins, such as the Albert coal, Canadian asphalt and liquid petroleum, while possessing many characteristics of metallic lodes, will be found to differ from them in some

[2] A valuable paper, by T. S. Hunt, in this Journal, [2] xxxv, 157, 1863, upon Bitumens, etc., presents the general conclusions stated above. His data were derived both from analysis of mineral combustibles and explorations in petroleum districts.

Sir William Logan mentions the occurrence of petroleum springs for twenty miles along a fold in the stratification in Gaspé in 1844. J. P. Lesley has described a vertical vein of asphaltic coal, precisely like the Albert, in Ritchie Co., W. Va.

respects. These particulars will be ascertained fully by the immense enterprise now manifested in sinking for petroleum. We can anticipate differences in respect to the limited depth, little variations of thickness at intersections, irregular yield, and origin of the carbonaceous veins. A proper knowledge of them may lead to some modification of terms in our definitions.

87 Park Row, New York, Jan. 23, 1865.

ART. XXXIII.—*Detection of the adulteration of Essential Oils with Oil of Turpentine by the Saccharimeter;* by Dr. JULIUS MAIER, Assistant in the School of Mines, Columbia Coll., N. Y.

THE essential oils, especially the expensive ones, are mostly adulterated with oil of turpentine. It is often difficult to detect this adulteration, especially when the adulterated oil gives similar reactions with oil of turpentine.

With the saccharimeter it is possible not only to detect the adulteration but even to find out the quantity of oil of turpentine mixed with the other essential oils. A large number of oils, particularly those belonging to the camphenes, the carbon of which is in the proportion of 5 to 8 to their hydrogen, have an action on the polarized light, deviating the light either to the right or the left hand side. These optical researches have been made by Biot, Soubeiran, Capitaine, Gladstone and Berthelot, in order to establish the constitution of the camphenes. I made some researches to detect the adulteration of the essential oil with the oil of turpentine.

For that purpose, a chemically pure oil of lemon which I had prepared myself, was tested in a saccharimeter, the tube of which was 200^{mm} long.

The deviation was +137°·296 for the middle yellow ray. The oil of turpentine, used for the research, prepared by myself, had a specific gravity of 0·865 and gave a deviation of −73°·135. A mixture of equal volumes of both these oils showed a deviation of +30°·65. The calculation gives a deviation of +32°·081 in the following manner:

½ vol. oil of lemon	= +68·648
½ vol. oil of turpentine	= − 36·567
1 vol. mixture	= +32°·081

A mixture of 2 vol. oil of lemon with one vol. oil of turpentine gave a deviation of +65°·34; from the calculation results a deviation of +67°·152, as follows:

⅔ vol. oil of lemon	= +91°·531
⅓ vol. oil of turpentine	= − 24°·379
1 vol. mixture	= +67°·152

I made the same researches with pure oil of juniper, which I had prepared myself, and arrived at the following results:

The oil used for the experiment had a specific gravity of 0·858 and showed a deviation of −5°·970. The oil of turpentine employed was the same as in the above mentioned experiment. A mixture of equal volumes of oil of juniper and oil of turpentine showed a deviation of −40°·84; the calculation gives a deviation of −39°·553 as follows:

½ vol. oil of juniper	= − 2°·985
½ vol. oil of turpentine	= − 36°·568
1 vol. mixture	= −39°·553

From this it is proved that the quantity of the adulterating oil of turpentine can be detected through the medium of the saccharimeter. But if the essential oil is adulterated not only with oil of turpentine but also with another optically active oil, the saccharimeter test is of no value. In order to find out the quantity of the adulterating oil of turpentine from the deviation showed by the oil of turpentine, by the adulterated oil, and by the mixture, the numbers of deviation have to be brought in reference to a common distinct starting point. This starting point generally is the power of rotation, that is the deviation of the respective oil as calculated for a tube of 100^{mm} length, and a specific gravity of 1. This power of rotation, the worth of which is generally expressed by $[\alpha]$, is the following for:

Oil of turpentine	$[\alpha] = -42°{\cdot}275.$
Oil of lemon	$[\alpha] = +80°{\cdot}573.$
Oil of juniper	$[\alpha] = -\ 3°{\cdot}479.$

The quantity of oil of turpentine employed for the adulteration, is calculated as follows:

a	the power of rotation of the pure oil.
b	" " " " " " oil of turpentine.
c	" " " " " " mixture.
m	" quantity " " "
x	" " " " adulterating oil of turpentine.

The quantity of the pure oil as contained in the mixture is $=m-a$, and the power of rotation of this quantity is $=(m-x)a$; the power of rotation of the oil of turpentine $=-bx$, and the power of rotation of the whole quantity of mixture $=mc$; hence results the following equation:

$$(m-x)a-bx=mc$$
$$ma-ax-bx=mc$$
$$ma-mc=ax+bx$$
$$\frac{m(a-c)}{a+b}=x$$

To show this calculation by an example, the power of rotation is supposed to be—

of the pure oil of lemon $= +80°{\cdot}573$
" " " " turpentine $= -40°{\cdot}275$
" " mixture $= +18°{\cdot}70$
the quantity of the mixture $= 20$ c. cm.

$$(20-x)\ 80{\cdot}573 - 40{\cdot}275x = 20 \times 18{\cdot}70$$
$$7611{\cdot}46 - 80{\cdot}573x - 70{\cdot}275x = 374$$
$$1237{\cdot}46 = 122{\cdot}848x$$
$$10{\cdot}0 = x.$$

The mixture contains equal parts of the pure oil and the adulterating oil of turpentine.

Optical behavior of several essential oils.

Tested oil.	Specific gravity.	Power of rotation.	Observer.
Oil of absinth,	0·973	+ 20°·67	Soubeiran & Capitaine.
Oil of orange blossoms, first product,	0·835	+127°.43	
Oil of orange blossoms, second product,	0·837	+125°·59	
Oil of bergamot,	0·850(?)	+ 29°·28	Biot.
Oil of bergamot, first product,	0·850	+ 49°·396	Soubeiran & Capitaine.
Oil of bergamot, last product,	0·877	− 6°·573	
Oil of caraway seed,	0·897	− 11°·7	Maier.
Oil of lemon,	0·848	+ 80°·484	Biot.
Oil of lemon,	0·852	+ 80°·573	Maier.
Oil of lemon, (Grasse,) first product,	0·844	+ 79°·749	Soubeiran and Capitaine.
Oil of lemon, (Grasse,) last product,	0·853	+ 78°·156	
Oil of lemon, rectified,	0·854	+ 80°·916	
Oil of copaiva balsam,	0·881	− 34°·18	
Oil of copaiva balsam, (Para,)	0·898	− 28°·553	
Oil of cubebs,	0·929	− 40°·159	
Oil of cubebs, free from water,	0·914	− 39°·40	
Oil of elemi,	0·852	− 90°·30	Deville.
Oil of juniper,	0·855	− 3°·521	Soubeiran & Capitaine.
Oil of juniper,	0·858	− 3°·479	Maier.
Oil of turpentine,	0·8722(?)	− 39°·950	Biot.
Oil of turpentine,	0·860	− 43°·38	Soubeiran & Capitaine.
Oil of turpentine,	0·865	− 42°·25	Maier.

Gladstone (Chem. Soc. Jour., [2], ii, 1,) has given a more complete table on the power of rotation of most of the essential oils. My researches were restricted to the few above mentioned.

ART. XXXIV.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

(Concluded from p. 150.)

§ 14. *The Lunar Distances.*

As Kepler's third law was deduced from the planetary orbits alone, so was the law of Titius. But it was shown to be a consequence of the law of universal gravitation, and therefore itself universal and applicable to any system—hence, also to the lunar systems. Now the law of Titius, as modified above, has been found to be identical with the equality of the intervals of time in the history of any system. Therefore, also, this law (38) must apply to the lunar systems. This we now will show.

A. *The Lunar System of Jupiter.*

The *Jovial World* is the youngest of those great lunar systems that adorn the exterior planets. (This Journal, xxxvii, p. 45.) Therefore, it is the most regular yet of any, and our law (38) must very closely harmonize with the actual distances of Jupiter's moons. It is easily found that $\gamma=2$, again, as for the planetary distances; and that $\alpha=4$ and $\beta=3$ radii of Jupiter. Thus (38) is for the Jovial World,

$$a_t=4+3\times 2^t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (64)$$

		t.	Distance. Calculated.	Observed.	Fall.
Moon	I.	0	7	6·049	·951
"	II.	1	10	9·623	·377
"	III.	2	16	15·350	·650
"	IV.	3	28	26·998	1·002

The "fall" of a moon is the distance it has fallen toward the planet in virtue of the resisting ether. That this fall corresponds to the age, mass and density of the different moons has been shown in our previous article. (This Journal, xxxvii, 45.)

The calculation of t from the observed distances gives for the 2d, 3d and 4th, respectively, ·907, 1·92, and 2·94, which only deviate by ·09, ·08 and ·06 from the theoretical values 1, 2 and 3; and all values being *too small* shows that these moons are correspondingly nearer the primary, having approached so much on account of the etherial resistance.

B. *The lunar world of Saturn*

is next in age, hence not quite so regular as that of Jupiter. We find that (38) represents the distances of the eight moons if the constants are

$$a_t=4+0{\cdot}35\times 2^t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

as will be seen from the following table:

Moon.	Distance. Calculated.	Observed.	Difference.
I. Mimas, . .	4·35	3·4	+·95
II. Enceladus, . .	4·70	4·3	+·40
III. Tethys, . .	5·4	5·4	·0
IV. Dione, . .	6·8	6·8	·0
V. Rhea, . . .	9·6	9·6	·0
VI. Titan, . .	15·2	22.2	−7·0
VII. Hyperion, . .	26·4	28·0?	−1·6?
VIII. Japetus, . .	48·8	64·0	−15·2

Excepting for a moment the 6th and 8th moon, we see but small differences; Mimas and Enceladus being too near Saturn, appear to have but very small mass, which conclusion is strengthened by the fact that it required Herschel's great telescope to discover them (1789). The next three almost exactly harmonize with this law; they are, therefore, not only larger than the first two, but also much alike. They were discovered by Cassini, first the fifth (Rhea) in 1672, and later (1684) Tethys and Dione. As the latter were discovered by the same observer, the difference in date is, perhaps, alone due to the greater nearness to the disk of the primary. Hyperion is even lower than any, and, therefore, smaller than even the interior ones. This is confirmed by its discovery, which was not made till 1848, by Bond and Lassell. But the sixth, Titan, and the eighth, Japetus, are much farther distant than (65) gives; thus proving them to have much more considerable mass (or rather v (37) is less, which in general will be the case if the mass is greater). This is fully confirmed by the date of discovery: Titan being the first discovered of all, (by Huyghens, 1655), and Japetus the second, (by Cassini, 1671). These estimates of the masses are further corroborated by Humboldt,[1] who calls Titan "the largest of all known secondary planets." Compare another theoretical estimate, (this Journal, xxxvii, 46), leading to the same results.

C. *The lunar system of Uranus*

is exceedingly important on account of the plane and direction of its motions. We have tried to show that this very position affords one of the most conclusive confirmations of the nebular theory. (This Journal, xxxvii, 50.) Here we will consider the arrangement of the individual members of the system.

We know it to be the oldest, because it is the most distant system of which we have definite knowledge. The original distances and the original harmony of these distances is therefore here most deranged. We cannot even with any degree of certainty consider the moons to be now in the same order of suc-

[1] *Cosmos*, i, Harper's edit., p. 95.

cession as at first. At the same time, observation has as yet hardly determined the number, much less the exact distance of the different moons. Therefore, we give the following more for the sake of completeness than with the view of adding any important confirmations of our law.

We have seen that the nearest luminaries may be equi-distant, and that the farthest may succeed at distances that form a geometrical progression--see (61) and (63). If the distances, as given by Herschel, and the times of revolution, as given by Lassell,[2] are exact, we may represent the distance of the first six moons by

$$a_t = 7{\cdot}5 + 3t, \quad \ldots\ldots\ldots \quad (66)$$

corresponding to (62), and the distance of the sixth, seventh and eighth by

$$a_t = a_6 . 2^{t-6}, \quad \ldots\ldots\ldots \quad (67)$$

corresponding to (61).

		Distance. Calculated.	Observed.	Difference.
Moon	I $= 7{\cdot}5 + 0 \times 3$	$= 7{\cdot}5$	7·5	·0
	II $= 7{\cdot}5 + 1 \times 3$	$= 10{\cdot}5$	10·5	·0
	III $= 7{\cdot}5 + 2 \times 3$	$= 13{\cdot}5$	13·1	+ ·4
	IV $= 7{\cdot}5 + 3 \times 3$	$= 16{\cdot}5$	17·0	− ·5
	V $= 7{\cdot}5 + 4 \times 3$	$= 19{\cdot}5$	19·8	− ·3
	VI $= 7{\cdot}5 + 5 \times 3$	$= 22{\cdot}5$	22·7	− ·2
	VII $= 2 \times 22{\cdot}5$	$= 45{\cdot}0$	45·5	− ·5
	VIII $= 4 \times 22{\cdot}5$	$= 90{\cdot}0$	91·0	−1·0

If these observed distances really are correct, then this remarkable discontinuity will enable us to determine the lunar masses long before observation can ascertain them.

D. *Conclusion.*

The lunar system of the *Earth*, consisting of but *one* moon and that of *Neptune*, which comprehends one or two, cannot, or do not afford any chance to test our law. But we have seen that the systems of Jupiter and Saturn fully confirm our law (38), if due regard is had to the individual mass and volume—or density and radius—of the several moons. Even the system of Uranus, as far as known, does not deviate from it except in so far as it offers the two extreme limits of the law, probably on account of the high age and a close similarity between the masses of the first six moons.

Therefore we may say that as far as observation on the lunar systems goes it is embodied in our law (38), or *in every lunar system the consecutive moons were formed at equal intervals of time.*

[2] See Schweigger in Astronomische Nachrichten, No. 832, Beilage.

§ 15. *The incommensurability of the periodic times.*

By the third law of Kepler we have, if T_t and $T_{t'}$ are the periodic times of two planets,

$$\frac{T_{t'}}{T_t} = \left(\frac{a_t'}{a_t}\right)^{\frac{3}{2}}, \quad \ldots\ldots \quad (68)$$

or by (38),

$$\frac{T_{t'}}{T_t} = \left[\frac{\alpha+\beta\gamma^{t'}}{\alpha+\beta\gamma^{t}}\right]^{\frac{3}{2}}, \quad \ldots\ldots \quad (69)$$

which expression will not generally make $T_{t'}$ and T_t commensurable. Thus we see that our law accounts for another important condition of stability of the system, (see § 3, 1).

But as the distances are continually decreasing, and at different rates, (this Journal, xxxvii, 41, gives the numerical values of these rates), we perceive that *in time such commensurability may take place* between any two planets.[3] Such is actually the case between Jupiter and Saturn, as discovered by Laplace.

The distances were (see § 13) for Jupiter $a_5 = 520$, for Saturn $a_6 = 1000$, giving for (68) the continued fraction $2(1, 1, 2\ldots)$ having the approximations,

$$\frac{2}{1}, \frac{3}{1}, \frac{5}{2}, \frac{13}{5} \quad \ldots$$

or $T_6 : T_5$ approached originally to 5 : 2; now it is very nearly so.

For Venus and the Earth the original distances 70 and 100 give the approximations,

$$\frac{1}{1}, \frac{2}{1}, \frac{5}{2}, \frac{12}{7}, \frac{29}{17} \quad \ldots$$

whilst Airy has found the commensurability 13 : 8 or nearly our 29 : 17 [13 : 8=29 : 17·8].

In the *lunar systems* such commensurability is common; and it is for the satellites of Jupiter that Laplace demonstrated[4] the great proposition, *if such commensurability exists but approximatively it will become exact in time.*

Having seen that the change in distance produced by resistance will make the ratio approach commensurability, it therefore, as we stated before, will become rigorously so.

From (68) we find easily that the ratio will be 2 if the distances are in the ratio of $2^{\frac{3}{2}} : 1$, or (by continued fractions) as the approximative fractions,

$$\frac{1}{1}, \frac{2}{1}, \frac{3}{2}, \frac{8}{5}, \frac{19}{12}, \frac{27}{17} \quad \ldots$$

[3] Grant (*History of Physical Astronomy*, London, 1852, p. 93,) states that the libration of the jovial moons is "independent of the effects of a resisting medium," meaning that it will be preserved notwithstanding such medium. This is probably a mistake, for it would depend upon the relative magnitude of the resistance and the perturbation.

[4] Méc. Cél., vol. viii, Ch. xi, § 15. We express the proposition in more general terms.

For Jupiter's satellites we have $a_2=16$, $a_1=10$, or $a_2 : a_1=8:5$; and $a_1 : a_0=10:7=8:5{\cdot}6$, therefore we find the periodic time of the second moon twice that of the first, $T_1=2T_0$, and the periodic time of the third twice that of the second, $T_2=2T_1$; hence Laplace's famous relation between the mean motions,

$$n_0+2n_2=3n_1.$$

In the system of Saturn similar relations obtain. At first we had (see § 14, B) for the distance of Tethys (III) and Mimas (1) the ratio 5·4 : 4·4=8 : 6·6, while the duplication of the periodic times requires the ratio 8 : 5. But Mimas has approached Saturn the most, and thus this proportion (now 5·4 : 3·4=8 : 5·04) has been brought about.

For the fourth and second we had originally, Dione : Enceladus=6·8 : 4·7=8 : 5·5, or likewise sufficiently near 8 : 5 that the duplication of the periodic time should become almost rigorous.[5]

The lunar world of Uranus is particularly noted for such duplications, from the fact that Schweigger, as early as 1814, on such grounds predicted the existence and gave the orbits of the two innermost moons of Uranus, which were discovered by Lassell in 1851. The coincidence is very remarkable, as will be seen from the following:[6]

	Schweigger, 1814.	Lassell, 1851.
Uranus, I moon, - - -	2·1767 days,	2·5117 days.
II - -	4·3534 "	4·1445 "

and the IV (or II of Herschel) having a period of 8·7068 days approximates to the further duplication of the periodic times. Also the period of III is about half the IV period, the former being 5·8926 days, the latter 10·9611.

Taking only the first two decimals we find by means of continued fractions the following approximations:

$$\text{II to I or } \frac{441}{251}=\frac{1}{1}, \frac{2}{1}, \frac{3}{2}, \frac{5}{3}, \frac{28}{17}, \frac{33}{20}, \text{ etc.}$$

$$\text{IV " II " } \frac{871}{441}=\frac{2}{1}, \frac{19}{9}, \frac{21}{10}, \frac{40}{19}, \frac{61}{29}, \text{ etc.}$$

$$\text{V " III " } \frac{1096}{589}=\frac{1}{1}, \frac{2}{1}, \frac{13}{7}, \frac{93}{50}, \frac{199}{107}, \text{ etc.}$$

thus proving that only the fourth (Herschel II) and second (Lassell II) have periodic times nearly in the ratio of 2 to 1.

The other instances adduced by Schweigger, and especially the first, do not seem to have any claim to be considered as real duplications. Still it is evident that the configuration of the Uranian-system is such as approaches to simple ratios between the periodic times; and if the perturbating force arising here-

[5] Herschel, *Outlines of Astronomy*, § 550.

[6] Schweigger; Ueber die Auffindung der ersten Uranustrabanten durch Lassell. Astronomische Nachrichten. 1852, No. 832.

from is greater than the effect of resistance, these ratios and the corresponding configuration would become permanent. It is not improbable that an analysis of the lunar system of Herschel's planet will throw much light on the future configuration of the solar world by ascertaining the exact relation between perturbation in commensurable revolutions brought about by resistance and the continued influence of the latter force on such commensurable motions.

Though this latter question cannot at present be fully answered, we have proved in this paragraph that not only the general incommensurability of the periodic times ensuring the stability of the system, but also the deviations therefrom are accounted for by our law (38).

§ 16. *History of the Solar System.*

Believing that we have, in the preceding pages, brought forth some further arguments in favor of the nebular hypothesis, we may be permitted in a very few words to sketch the grand history of the material universe as it is seen in the light of this theory. The philosophers of old called Man a Microcosmos—we compare the Universe, the Macrocosmos, to man, thereby intimating that as Man has a parentage, growth and decay, i. e., a *history*, so has the Macrocosmos.

The history of the *material* world may be divided into four periods or ages, corresponding to those given in a note to §6. (Compare Guyot's views in Dana's Geology—chapter on "Cosmogony").

In the beginning *God created the heavens and the earth.* And the earth was *without form and void,* and *darkness* was upon the face of the deep. And the spirit of God moved upon the face of the waters. (*Genesis*, I, 1, 2).

The material universe was created *not* in its present form, but without form; it was *void* and *dark*; but the spirit of God pervaded it, and planned it such that his All-Foresight, or Providence, might also be manifest in the material world. *This* is really *the Creation*—it is merely stated, not described, for it is inconceivable to mortal understanding. It is too awful, our mind is lost in reflecting thereon; hence the divine writer merely mentions it at the beginning, and, to give fullness to his picture and adapt himself to our understanding, describes the first three great ages as real creative acts, though mere consequences of the unfathomable word given in the *first* verse of Genesis. We believe that the first five verses of Genesis have never before been fully understood in their deepest sense. We shall in the sequel keep constantly before our eyes both this, the *revealed History of the Cosmos*, and *science*, deduced from the revelation we have in the present form of nature.

Since Genesis merely states that the universe (i. e. heaven and earth) was *formless*, *void* of any organized being and *dark*, it is science alone that can give us any idea of the constitution of the universe as it came from the hands of the Creator. But as science is progressive, our ideas of the primeval condition of the Cosmos must progress correspondingly, or rather with advancing science our eye pierces farther and farther back into the dark past, approaches more and more to the mysterious and almighty "Fiat." As these approaching steps represent greater and greater series of ages, we infer that the *Fiat* lies infinitely far behind us, and can never be reached by human thought. We experience in regard to the age of Cosmos by penetrating farther and farther into the dark past with our spiritual eye, the same that we feel in piercing, by means of more and more powerful telescopes, farther and farther into the world-filled abyss of space. Here, if looking through a giant telescope we find ourselves surrounded by a boundless space filled with the wonders of the Creator; and if ardently searching in the existing documents of nature for records of her past, we behold infinity also here, the infinity of time, *eternity*, teeming with wonders no less astounding. The beautiful poem of Schiller, "*die Grösse der Welt*," is true both as to the extension and the duration of the World.

The ancients most frequently thought that the world left the hands of the Creator in the shape it now is. Even Newton himself was unable to see farther back. To him the Creator was but a tinker, forming his wheeling globes and wheeling them around their axis, putting them one by one and one by one to their very place in his clockwork—to him an unorganized machine to run on and on forever in the same shape. But Huyghens, and Newton himself, by discovering the generic cause of the figure of the earth aimed the first blow at this base idea, which nevertheless has found its advocates even to the present hour, especially among theologians. The corner-stone being broken out of the system it has been crumbling down. Geology has restored the lost history of the earth, and the nebular theory has traced this earth to the sun as her mother. Thus creation was now identical with the productions of the rotating mass of matter, i. e., of the chemical elements.

We have attempted to show that both rotation and the elements come from the forces wherewith the ONE matter (Urstoff) was endowed (see § 6). It is highly interesting to see how the first verse of Genesis has been understood by scientific men. It will at the same time more clearly set forth what we implied above when saying that science is approaching to the true original condition of Cosmos by making steps representing longer and longer periods of time.

"In the beginning God created the heaven and the earth" means according to

Newton, 1686: a direct, immediate creation of every globe as it is now.

Huyghens, Hutton, and modern Geologists: a direct creation of the heavenly globes as *fiery masses*, circulating in the system as they do now.

Kant, 1755, Laplace (later): a direct creation of a rotating mass of chemical elements; giving rise to the planetary system.

We, in 1854, conceived this rotating mass of elements to be the product of a created nebula consisting of but *one single element.*

We will now contemplate the different ages manifest in the development of this Urstoff.

First Day or Age.—The atoms of "Urstoff" combine—light (and heat) and the *chemical elements* result. The mere production of light would not entitle it to be considered one of the days of creation; but light is by the divine writer taken as a type to represent itself, and the less obvious, though much more important, chemical elements. It was not so much the light as the formation of the elements, the basis of modern physical science, which characterized the first day. We think that a rotation was also produced hereby. (This Journal, 1864, vol. xxxvii, p. 52.)

Second Day or Age.—Formation of the planetary orbs with their satellites.—The nebula developed itself into a great number of similar planetary nebulæ, which again gave birth to similar lunar nebulæ. Thus we see here the simplest kind of "life," reproduction by division, as exhibited by many plants, and even animals, which to distinguish them as such from inanimate matter, have another mode of reproduction *besides.* The planets represent the children, the moons the grandchildren of the sun.

Third Day or Age.—The fiery balls resulting from this subdivision cool down and are shaped, as Geology has ascertained in relation to our own earth.

The Fourth Age of the *inorganic era* is the present. We have shown that the further characteristic of life, namely, death, is not restricted to the organic but is participated in by inorganic nature (this Journal, [2], xxxvii, 56). As every breath of our lungs is a differential of decay—so every rotation of the earth giving us the enjoyments of another day, and every revolution charming us with the succession of the seasons, brings our own mother earth nearer to her grave.[1]

[1] We beg the scientific reader's pardon for these paragraphs, which do not belong to this place. But we felt it urgent to say at least this much, as some, even to-day, are apt to base the cry of "heretic," "infidel," etc., on any such deviation from the beaten path in their dogmas. The nebular hypothesis has richly participated in the abuse heaped in its day on the Copernican system, and on some leading doctrines of geology. Even yesterday, I found, in one of the leading religious quarterlies, Laplace called an "atheistic dreamer"! We wrote this paragraph as a protest against such imputations.

§ 17. *Conclusion.*

The principal results arrived at in this paper are

1st, A simple mechanical *theory of spiral nebulæ.*

2d, A more accurate determination of the orbits; and above all,

3d, *The discovery of the true law of the planetary and lunar distances.*

4th, The determination of the periodic times as a function of the distances—or borrowing this third Law of Kepler from the theory of gravitation, we have therein almost a theoretical demonstration of *the equality of the intervals.*

As (38) what we have repeatedly called "our law" is very much like Bode's, or rather Titius's, law, we apprehend that the propriety of thus naming (38) will be doubted. To set this point in clear light we refer to a similar, though undoubtedly grander, case in the history of science.

The law of Titius was exclusively derived from observation. It is empirical, as is the third law of Kepler. It is, moreover, not exact, neither in its general form nor in its numerical results. But neither is the famous law of Kepler exact, though, on account of the different circumstances connected herewith, this latter law agrees better with the numerical data of observation than Titius's law.

Newton discovered the true form of Kepler's law by deducing it from a higher law, that of universal gravitation. Instead of Kepler's form, C being the same constant for all planets,

$$\frac{T_2}{a^3} = C, \quad . \quad . \quad . \quad . \quad . \quad (70)$$

Newton found that the true law is

$$\frac{T^2}{a^3} = \frac{\mu}{4\pi^2}(M+m), \quad . \quad . \quad . \quad (71)$$

μ being the constant of gravitation, hence the same for all planets; hence,

$$C \propto (M+m). \quad . \quad . \quad . \quad (72)$$

That is, Kepler's constant C is proportional to the sum of the mass M of the sun and the mass m of the planet. By farther analysis it is found that C even is dependent on *all* the masses and distances in the system.

So also in our case. We have given the true expression of Titius's law by extending it to Mercury and have accounted for the deviations of nature from the law, by demonstrating that it is a necessary consequence of the higher law, viz: *the intervals between the abandonment of the different orbs of the same system are equal* (see § 13). *Now this is what we claim as our law.* As Newton deduced and corrected Kepler's law by his law of *equal gravitation*, so we have deduced and corrected the law of Titius by our law of *equal intervals.*

We referred to (38) as our "law" because it is a *consequence* of our law, and certainly our formula; we did not intend to obliterate the merit of Titius, as will be seen wherever we have mentioned his name.

There is yet another circumstance which makes our demonstration of the law of planetary distances so important. It is the touchstone of the nebular theory; for as this ascribes the formation of the planets to the slow descent of cosmical matter to its center, it has to be proved that such descent will give exactly the actual system. Already Plato held that[7] "the motion of the planets is such as if they had been all created by God in some region very remote from our system, and let fall from thence toward the sun, their falling motion being turned aside into a transverse one whenever they arrived at their several orbits." Galileo was the first who subjected this "concetto platonico" as he calls it to a numerical calculation based upon the laws of falling bodies as discovered by him. He finds an admirable harmony between his calculations and the actual velocities and distances as they were known at his time.[8] Next after him, Newton took the matter in hand, and in his third letter to Dr. Bentley he gives as his result, that it is impossible to account for the configuration of the system in the manner of Plato and Galileo. This result is based upon his assumption of a vacuum. By taking the influence of a resisting medium into account, we have proved that the Platonic idea as embodied in the nebular hypothesis *does* lead to the present configuration of the solar world. We make these remarks to show that the idea we advocate is old and venerable; we hope, at some other time, to give the highly interesting history of the law of planetary distances, including the application of the Phyllotaxis, (Pierce, Agassiz,) the radius of gyration, (Kirkwood,) the regular polyhedra, (Kepler, Plato,) etc.

How grand and beautiful is the harmony of the planetary world! What an admirable *unity of plan* is manifested therein! As *now* the planets are slowly sinking to the sun, so they have *always* been sinking since the moment of their creation as a nebulous mass; the same motion that now brings them nearer to their death has caused their formation, has brought them to life! And how sublime is the plan of creation! To call forth the harmonious system of the solar world with all its multiform aspects and dependencies fit to support life throughout almost endless ages—nothing but a collection of matter endowed with its

[7] Brewster, *Life of Newton*, Ch. 16.

[8] *Dialogo intorno ai due massimi Sistemi del Mondo, Tolemeico e Copernicano.* Gjornata I, (ed. *Opere*, Firenze, 1842. Vol. i, p. 34-35.) He finds: le grandezze dei cerchj, e le velocità dei moti s'accostano tanto prossimamente a quel che ne danno i computi, che è cosa maravigliosa.

molecular forces was placed in a little spot of the house that contains many mansions besides. This matter slowly collected together. In thus following the force of attraction planted in it by eternal love, the whole great life of the solar world was awaked; and as the pulsation of the heart in man indicates the fleeting moments of his life, so the pulsations of that great whole, succeeding each other at equal intervals, gave each one birth to a new world to mark the historic epochs of the Universe by its position and to roll on for ages, a revelation of the Great Author, until, always following the same attractive force, it in death finds rest at the bosom of the planet-mother, the sun. And then—this grand system remains as a mere lump, a *Cosmic Fossil,* suspended in space, where perchance some higher being may meet with it, touch it, investigate it, and construct its whole past history, as the geologist in our days studies the history of a fossil shell!

Iowa City, Iowa, July—November, 1864.

ART. XXXV.—*The determination of the height of Auroral Arches from observations at one place;* by H. A. NEWTON.

IN the displays of the Aurora Borealis the luminous cloud often takes the form of an arch. Sometimes the lower boundary of the auroral light is arch-shaped. Beneath is a dark segment, while perhaps streamers run upward from the mass of light. Again, there is sometimes a bank of light in the north, resting apparently on the horizon. The upper boundary of this bank forms a more or less regular arch. Again, there is sometimes a narrow band or bow of light, spanning the heavens, coming down to within two or three degrees of the horizon at each extremity, having one or both of its edges sharply defined, and being often only two or three degrees in breadth.

The arch in each of these three cases may be incomplete, or broken, or otherwise irregular. But there is a manifest tendency to form a regular curve. This curve, that is, the boundary line of the arch, or the axis of the bow, is rarely, if ever, an arc of a great circle. It cuts the horizon at points notably less than 180° from each other. It has apparently the same law of formation in each of the three cases. Its peculiar shape is therefore probably due to a single cause.

There is no reason to believe that each observer sees a different arch, just as each sees his own rainbow. There is no center of light beneath the arch, and moreover a decided parallax is very frequently found. The curve of the auroral arch has then a definite locus in the atmosphere.

This curve is not caused by mists in the atmosphere obscuring and revealing parts of an indefinite cloud. For the arch has little or no relation to the horizon, and cuts it at all angles.

It is not a straight line, for the arch does not cut the horizon at points 180° from each other.

The arch resembles the projection of a portion of a circle, or a sphero-conic. The venerable Hansteen has in two instances seen at Christiana nearly the whole ellipse. Prof. Twining has observed, at Middlebury, Vt., in one instance at least, an arch in which the extremities of the major axis of the ellipse were visible above the horizon.

It is reasonable to infer that, in general, the locus of the light is parallel to the earth's surface. For the arch has the same general form at all places, as will be seen in the diagrams of Mairan and others.

This leads naturally to the hypothesis of Hansteen,[1] that the auroral arch is a real ring, which in its normal form is parallel to the earth's surface, and is symmetrically placed about the magnetic pole. The dark segment is seen when we look beneath the ring into space beyond. The bank of auroral light is a similar broader or more distant ring.

The results of Prof. Loomis's investigations respecting the geographical distribution of the aurora[2] confirm and modify this conclusion. He shows that there is a narrow belt of an elliptical form surrounding the magnetic and astronomical poles of the earth, and at a considerable distance from them, which is the region of the greatest and most frequent displays of the aurora. It is reasonable to infer that an aurora of considerable intensity would naturally take a form symmetrical with this narrow belt of the earth's surface. The portion of the curve which we see at any instant should be regarded as part of a circle whose center is the center of curvature of the nearest portion of this belt.

To obtain the parallax of the auroral cloud, observations at two distant stations have been necessary. These have to be made upon a moving object, the time of whose appearance cannot be predicted. It is only by a happy chance that good observations can be secured. If the height can be computed from measures made at a single station, a great advantage is gained. A second observer is not essential, if the position and shape of the auroral cloud is assumed to be as described above.

The distance on the earth's surface from the observer to the center of curvature of the nearest portion of the belt of frequent auroral displays can be measured. Represent this by d. Let the apparent altitude of the auroral arch be h, and its amplitude on the horizon be $2a$. Let x be the height of the auroral cloud

[1] Mèmoires de l'Académie Royale de Bruxelles, tome xx, p. 118. See also B. V. Marsh, this Journal [2], xxxi, 311.

[2] This Journal [2], xxx, 89.

from the earth, R the earth's radius, b the distance from the observer to the point on the earth directly under that part of the cloud which forms the vertex of the arch, and c the distance in like manner from the observer to the point underneath that part of the arch seen in the horizon. We have then these equations,

$$R+x=R\sec c=R\cos h\sec(b+h); \quad . \quad . \quad . \quad . \quad (1)$$

and since d, $d-b$, and c, are the three sides of a spherical triangle, and a the angle,

$$\cos(d-b)=\cos d\cos c+\sin d\sin c\cos a. \quad . \quad . \quad . \quad . \quad (2)$$

From these equations b and c may be eliminated, and x found in terms of a, d, h, and R.

From (1) $\cos(b+h)=\cos h\cos c$, and hence

$$\begin{aligned}\cos(d-b)&=\cos\{d+h-(b+h)\}\\&=\cos(d+h)\cos(b+h)+\sin(d+h)\sin(b+h)\\&=\cos(d+h)\cos h\cos c+\sin(d+h)(1-\cos^2 h\cos^2 c)^{\frac{1}{2}}. \quad (3)\end{aligned}$$

Equating (2) and (3) and dividing by $\cos c$,

$$\cos d+\sin d\tan c\cos a=\cos(d+h)\cos h+\sin(d+h)(\sec^2 c-\cos^2 h)^{\frac{1}{2}}. \quad (4)$$

But $\cos d=\cos(d+h)\cos h+\sin(d+h)\sin h$. Substituting, dividing by $\sin(d+h)$ and placing $\sin\varphi$ for $\sin d\cos a\operatorname{cosec}(d+h)$ we have

$$\sin h+\tan c\sin\varphi=(\sec^2 c-\cos^2 h)^{\frac{1}{2}},$$

Reducing,

$$\tan c=2\sin h\sin\varphi\sec^2\varphi.$$

Hence, to compute the altitude of the auroral arch above the earth's surface, we have the three equations,

$$\sin\varphi=\sin d\cos a\operatorname{cosec}(d+h),$$

$$\tan c=2\sin h\sin\varphi\sec^2\varphi,$$

and

$$x=R(\sec c-1).$$

To apply these observations to particular cases I have selected 25 or 30 auroral arches, observed by Pres. Stiles, Prof. Olmsted, Mr. Herrick, and Mr. Bradley. They were all observed at New Haven, except those in the year 1860, which were seen by Mr. Bradley at Chicago. Most of these observations are from the Auroral Registers of Mr. Herrick and Mr. Bradley. These Registers will form part of a volume of memoirs about to be published by the Conn. Acad. of Arts and Sciences. The value of d is assumed to be 32°, which is also very nearly the distance from New Haven to the magnetic pole of the earth. In the following table are given the dates of the auroras, the observed apparent altitudes and amplitudes of the arches, and the computed values of x in miles, and in kilometers.

In selecting from the Registers the arches for this table, I have omitted those which were low in the north, as the horizontal extent is then concealed by the mists. I have also omitted those of which the observations were indefinite, or seemed imperfect.

Table of the observed altitudes and amplitudes of auroral arches, with the computed heights above the earth's surface.

Date.	Altitude.	Amplitude.	Height in miles.	Height in kilometers.	Observer.
March 27, 1781	66½°	165°	33	53	Stiles.
Sept. 21, 1840	10-12	120	52-68	84-109	Herrick.
March 6, 1843	5-8	100	42-83	67-133	"
April 13, 1845	8	100	83	133	"
April 27, "	10	90±	155	250	"
Oct. 9, "	8	100	83	133	Bradley.
Oct. 19, 1846	8-10	80±	165-214	266-345	"
Dec. 9, "	5	50	281	452	Herrick.
May 15, 1847	7-8	80	142-165	228-266	"
June 12, "	5	50	281	452	"
Aug. 4, "	4	60	154	248	Bradley.
" " "	7	90	98	158	"
Sept. 29, "	10	70-80	290-214	467-345	Herrick.
Nov. 25, "	10-15	100	111-183	179-295	"
May 18, 1848	7-9	75±	168-222	270-358	"
Oct. 23, "	5-6	70	134-165	215-266	Bradley.
March 18, 1849	10-15	100	111-183	179-295	Herrick.
April 6, 1850	10-12	90	155-193	250-310	Bradley.
Feb. 18, 1851	23	120-130	154-104	248-168	"
March 18, "	12	120	68	109	Olmsted.
Sept. 29, "	8	90	118	190	"
Sept. 29, 1852	10-15	100	111-183	179-295	Herrick.
March 26, 1860	6±	90±	80	129	Bradley.
March 27, "	10	90-100	155-111	250-179	"
July 4, "	10-12	100+	111-140	179-226	"
Aug. 12, "	5	90±	62	100	"
" " "	7	90±	98	158	"
Aug. 17, "	8	100±	83	133	"

The average height as indicated by the table is 134 miles, or 215 kilometers. The observations were not taken with reference to the use here made of them, and the results can therefore be regarded as only approximately correct. Mr. Bradley informs me that his method of determining the amplitude of the auroral arch was to place himself with his face directly towards one end, and then compare the amplitude with the arc of 90°, which was easily estimated. He was accustomed to consider the curve of the arch as continued down until it cut the horizon.

This method of determining the height of the auroral arch is imperfect in that it supposes for it a given regular form. In fact the auroral cloud is usually more or less irregular. Yet in view of the very great difficulties in securing good observations for parallax at two stations, the method is believed to possess very decided value, both independently, and as a check upon other measurements. It furnishes moreover the means of determining not only the magnitude of the auroral cloud, but also the breadth and height of the streamers which often rise from the arch-shaped mass of light.

Yale College, March 18th, 1865.

ART. XXXVI.—*On the Iron Ores of Marquette, Michigan;* by J. P. KIMBALL, Ph.D.

IT is proposed in the present article to record a few observations made, in the month of May last, in the Iron region of Marquette county, Michigan, with the view of elucidating certain obscure points in the geology of the immense beds of earthy red hematite, specular and magnetic iron ores, with which this district abounds to an unparalleled extent, constituting it beyond any doubt the most opulent source of iron in the world yet discovered. It was not my privilege carefully to explore this region beyond a limited district near the middle. I am therefore unable to add any information as to the geographical extent of the iron deposits to the published results of Foster & Whitney's survey. Indeed, the following contribution to the knowledge of this region is chiefly by way of observation of the instructive rock-cuttings in the iron mines, or rather quarries, and in one particular cutting of the Peninsula Railroad, all of which have been presented to geological study since the close of the government survey; and, inasmuch as neither population nor industry has advanced beyond these quarries, no other advantage to the geologist at present exists over the means of exploration employed by the United States geologists at that time. It is appropriate to remark in this connection that it must certainly be a cause of great regret to all friends of natural science and mineral industry in this country, that the federal government has omitted to publish the special report upon the Marquette Iron Region by Mr. J. D. Whitney, the results of a second geological exploration.

In their survey of that portion of the large area of the Northwest exclusively occupied by schistose, metamorphic and crystalline rocks, which is included in the northern peninsula of Michigan, Messrs. Foster & Whitney in 1851 marked out the outlines of two distinct systems of crystalline rocks, one of which was defined as metamorphic, their Azoic proper; while the other, distinguished as a great development of granitic rocks outstretching in separated expanses, was described under the name of granite belts or ranges. The relation to each other of these two systems was hardly traced to conclusiveness owing to the concealment of their conditions of contact; but the latter was assigned to an origin later than the Azoic series upon the testimony of disturbances of the upper metamorphic strata by intrusive masses of the granite, and earlier than the Silurian, as the lower beds of the Potsdam sandstone are seen to rest undisturbed around them.[1] The distribution of the Azoic and gran-

[1] Report on the Geol. of the Lake Superior Land District, Part II, 1851, 44–48.

ite rocks, as laid down by the federal geologists, is presented in their map of the district between Keweenaw Bay and Chocolate River, to which reference can readily be had without encumbering these pages with a verbal recital.

The metamorphic or Azoic rocks, consisting of gneiss, hornblendic, talcose and chloritic slates, and beds of argillite, novaculite, quartzite, conglomerate, saccharoidal marble and crystalline limestone, were described as existing under conditions of great displacement, and displaying evidences of metamorphism, particularly in vicinity to the line of contact with the granite or igneous outburst. The occurrence of granite within the area of their distribution was recognized in the form of intrusive masses, while the greenstones and some of the schists, both following the general stratification, were regarded as trappean overflows, the former as an igneous product, the latter as pulverulent greenstone in the form of volcanic mud. The iron-ores of this series of rocks were described as alternating with the trappean ridges in the form of intrusive masses, and the remaining phenomena of their dissemination in many rocks of the series, were ascribed to processes of elimination and mechanical admixture prevalent during the accumulation of the original sediments. This explanation was believed to meet the mode of occurrence of the larger masses, which, at the time it was proposed, had not been extensively uncovered, and manifestly, as appears in the concluding paragraph of the chapter on iron-ores,[2] was not meant to be extended to beds of specular and magnetic oxyds of iron included within metamorphic strata, with a conformable range and dip, which indeed the federal geologists explicitly state their disposition to regard as the result of aqueous deposition.

The granite belts were defined in general terms to occupy an area of more than 2000 square miles, and, within the boundaries of Michigan, to be developed in two distinct ranges or spurs, of which the northern, including the Huron Mountains, forms the coast between Presqu'isle and Granite Point, and expands westward to a width of 25 miles. Along its southern intersection with the metamorphic series—its longest direction—it is laid down as continuous for sixty miles. It is separated by a zone of metamorphic rocks of some fifteen miles in width, which embraces the present iron industry of the region, from the southern belt, which is of very irregular outline, though nearly parallel for a distance of 36 miles along its northern intersection with the metamorphic belt. The large expanse of crystalline rocks occupying the undeveloped tract of the northern part of Wisconsin, stretching across the head waters of the Mississippi to

[2] Foster & Whitney, ibid, 68.

the north and west of Lake Superior, was considered by the federal geologists to be the continuation of these granite belts. The granite was described as forming numerous parallel ridges bearing east and west, and in both belts to indicate leading axes of elevation. Gneiss was observed to flank the granite both on the north and south with intercalated beds of quartzite, potash, feldspar and hornblendic schists, the whole dipping uniformly from the axes of elevation. Hornblende sometimes was seen to replace mica, forming a light grey syenite. The granite south of the metamorphic belt is generally made up of feldspar and quartz, mica being almost always wanting, or very sparingly present. Feldspar is the most predominating mineral, large bodies of flesh-red orthoclase being of common occurrence. Two systems of dykes, and extensive intrusive masses of greenstone —the larger of a bearing uniform with the axes of elevation, the smaller being N.E. and S.W.—were indicated as occurring throughout the distribution of both belts. These are intersected by veins of quartz.[3]

A most engaging, and, at the same time, a very wide field of inquiry, is presented to geologists in addressing their attention to the ancient crystalline rocks of the Northwest, with the object of resolving the distinctive distribution of the Laurentian and Huronian series.

Although no detailed description of the crystalline rocks of the northern peninsula of Michigan has been given, so far as I am aware, since the close of the survey of the Lake Superior Land District, a noticeable disposition prevails among geologists to assign to the metamorphic strata, distinguished by Messrs. Foster & Whitney as Azoic, an equivalency with the Huronian series of Canada.[4] Such a tendency appears to be due to the investigations upon the south shore of Lake Superior of Mr. Alexander Murray, of the Canada Geological Survey,[5] the results of which have been employed by the provincial geologists in their comparative study of the crystalline rocks of Canada and its bordering districts.

The lines of demarcation between the Azoic strata and the granite, as laid down by the Land District Survey, appear to have been determined by the succession of beds rather than upon any evidence of unconformability. Orthoclasic gneiss, succeeded by alternating beds of quartzite and hornblendic slates, flanks the granite as it falls away from its axes of elevation, and the points at which these have been observed to be succeeded by argillaceous, chlorite, and talcose schists, and beds of conglomerate, afford the only clue to the boundaries of the

[3] Foster & Whitney; ibid, chap. iii.
[4] Geol. of Canada. 66, 596; Dana's Manual of Geology, 143.
[5] This Journal, [2], xxxi, 394.

two systems of rock, thus far presented. It has not been claimed for the information upon the geological structure of the region in question, that the relation of the Azoic series with the granite ranges, has been established by sufficient or definite data. This must continue to remain an open question until it is decided by patient and careful observations, facilitated by the development of the district, which, up to within a short period, was almost unvisited by white men, and still presents formidable physical obstructions to the work of geological exploration, scarcely less in degree to those experienced by the Land District Survey, and explained in the report of Messrs. Foster & Whitney. The recent direction of attention to the Silver-lead region, so-called, or the Huron Mountains, has served somewhat to open this obscure region during the past summer, by the laying out of roads, and the commencement of mining operations. Although the district has been pretty thoroughly explored during the past season in a spirit of mining adventure, it has engaged no particular study with a view to scientific results. It is proper to state, however, that upon the evidence of explorers and collections, obtained at Marquette, the metamorphic character of the Northern range is demonstrated by numerous instances brought to my notice, of the extensive development in that region of orthoclasic gneiss—often granitoid, quartzite, massive feldspars, talcose, hornblende and chlorite slates, seams of graphite and magnetic iron ore. My own observations in the Iron region impressing me with the indigenous character of the larger masses of diorite and granite represented within the defined area of the metamorphic strata, and their entire distinctness from intrusive dykes or erupted masses, and concurring in the recognition of these strata by Mr. Murray as Huronian, I am disposed to regard the entire region as of metamorphic character, all of whose larger masses of crystalline rocks are indigenous, and to be divisible into the two formations, Laurentian and Huronian: the former formation probably forming the surface of the areas known as the granite ranges, while the latter probably occupies, with minor deviations, the limits laid down for the crystalline schists comprehended under the name of Azoic.

This view is fortified by the fact that the Huronian or Azoic series is interposed between the granite or Laurentian rocks and the Potsdam sandstone, thus bringing the Laurentian in its proper place below the Huronian; while the only argument militating against it is the observation by Mr. S. W. Hill, of intrusive granite at one point cutting the slates near their southern line of intersection with the main granite, whence is derived the inference that the slates antedate the granite. The indigenous character of the greater part of the granite, however, throughout its

general distribution, connected with the fact of its coëxtensive association with metamorphic sedimentary strata, tends in this instance to invest the intrusive granite with characters distinct from those of its greater development, and to impart to it the special phenomena of an independent dyke. Apart from the evidence of superposition, the lithological features of the Granite Belts, so far as they have been indicated, bear a marked analogy with the standard characters of the Laurentian series as determined in Canada and upon the north shore of Lake Superior.

In a private letter from Mr. T. Sterry Hunt, acquainting me with some of the unpublished leading results of Mr. Murray's examinations upon the south shore, I am informed that this geologist, whose opportunities for comparison cannot be too highly appreciated, describes the islands and some points on the main land north and northwest of Presqu'isle as of Laurentian gneiss with hornblende, epidote, and pyroxene, arriving at this conclusion through the observation to the south of these points of the greenish slates, greenstone, dolomitic limestones and conglomerates, associated with the beds of iron ore, and their relations to the gneiss. This latter series he judges to be the equivalent of the Lower slate conglomerate, or chloritic slates, at the base of the Huronian system, regarding them here as retaining nearly the same relative conditions as upon Lake Huron where the dolomite marks the division between the Upper and Lower slate conglomerates.

The gneiss which marks the boundary of the granite belts, and accordingly characterizes the top of the Laurentian series in this region, the same as it is elsewhere represented, is succeeded by dark colored hornblendic schists, which consequently represent the base of the Azoic or Huronian series. These schists are followed by a series of augitic rocks and schists, interstratified with magnesian hydrous rocks and slates, the two kinds of rocks being represented on the one hand by hypersthene, pyroxene, and bedded diorite passing into dioritic slates, and on the other by talcose and chloritic schists. The former character of rocks prevails to such an extent as to impart to the lower members of the Huronian series a distinctive augitic aspect. The several rocks composing this augitic zone are commonly of a greenish color, and vary in this respect chiefly as to shade, resembling in this particular the Lower slate conglomerate which marks the base of the series in Canada, and from which they seem to differ only in the absence of pebbles and boulders from the subjacent Laurentian rocks, which there form a distinguishing feature. Messrs. Foster & Whitney, however, describe as occurring upon Little Presqu'isle a pure white feldspar containing acicular crystals of hornblende, passing into separate beds of each of these component minerals, and enclosing

angular fragments of hornblende and chlorite slates, jasper and a green magnesian mineral.[6] It seems not unlikely that this rock in its further distribution may possess more decidedly the characters of a conglomerate, and accordingly the more closely bear out the lithological analogy between the lower members of the Huronian series near Marquette, and their well ascertained characters north of the great lakes.

The hornblendic beds are succeeded by thick beds of pure quartzite displaying ripple marks, passing into quartziferous conglomerate on the one hand and siliceous slates on the other. The more massive beds of this description form a prominent feature of the country through their outcrop in an east and west ridge traversing the middle of the Azoic or Huronian area. They dip at high angles to the south, though numerous less elevated beds dip to the north. They are associated with greenish hornblendic slates and more or less crystalline diorite, and at their base with bands of dolomite somewhat siliceous and highly altered. Overlying this quartz zone, the probable equivalent of the Upper slate conglomerate of the North Shore, occur talcose, argillaceous and siliceous schists interstratified with specular iron ore, earthy red hematite and specular conglomerates, the latter resting immediately upon thick masses of hornblende rock blending into bedded diorite and hyperite on the one hand, and on the other into chlorite schists, and siliceous serpentines. At some points a dark colored quartzite resembling greisen except in the presence of hornblende instead of mica, and clearly of detrital origin, next underlies the ore beds.

Possessing the same stratigraphical conditions as the schistose rocks, while many varieties of them are represented in the schists by their exact counterpart as to composition, the crystalline Huronian rocks must be regarded as essentially metamorphic, while in a comprehensive view of the whole series it is seen that together with variable conditions of deposit, it indicates variable degrees of local metamorphism.[7] Plentiful evidence exists of the blending of a rock of one character into that of the other, or the continuity between crystallized and schistose beds. The diorites and hyperites of this region are never porphyritic, but on the contrary are so fine-grained as rarely to admit of the mechanical separation, or even the identification of their ingredients.

Besides the indigenous crystalline rocks distributed throughout the Huronian series, exotic or intrusive crystalline rocks are met with, but only in the form of dykes, and limited to a narrow distribution. The numerous elevated protuberances and ridges of granitoid gneiss, rising above the uppermost Huronian

[6] Foster & Whitney's Report, ii, p. 18.
[7] Compare Foster & Whitney's Report, ii, 16.

schists which rest upon their flanks apparently unaffected except by the regular convolutions of the entire series, are undoubtedly of Laurentian age. Two systems of greenstone dykes, according to Mr. Hill, as cited by Messrs. Foster & Whitney, traverse the gneiss—the one bearing N. from 70° to 80° W.—the other N. 10° E., and the same authorities have noticed many instances of the intersection of the schists by intrusive granite, especially near the borders of the granite ranges.[8] Not having enjoyed the opportunity of following Mr. Hill's notes, I am unable to confirm these observations; but it may not be inappropriate to suggest the probability that the larger and more persistent bodies of greenstone bearing approximately east and west—that is conformably with the axes of the folds which constitute a regular system of flexures coëxtensive with the distribution of the Huronian series in the vicinity of Marquette, are in reality indigenous greenstone, and a portion of the development of the diorite upon which repose the upper members of the series, and which, as will hereafter be shown, is uncovered along most of the ridges of the region. No instances of trappean overflows in this region have been brought to notice. Quartz veins, sometimes carrying the sulphurets of copper, lead, and iron, are of frequent occurrence, occupying fissures evidently of an extensive vertical range. Workable veins of copper pyrites have recently been discovered. This is another point of agreement between the features of the Huronian series as developed in this district, and upon the north shore of Lake Huron, where copper-bearing veins are frequent, of which those of the Bruce, Wellington and Wallace mines are well known examples.

It is urged by Prof. Rivot of Paris, that the crystalline and schistose rocks south of Keweenaw Bay entirely correspond with the rocks of Keweenaw Point; for he asserts the syenite of the Bohemian Mts. to be equivalent to the Granite Ranges, or to the granite of the Huron Mts.; and the metamorphic schists of the Marquette district to be the stratigrapical equivalents of the intercalated greenstones, amygdaloids, sandstones and conglomerates, known as the Upper copper-bearing series, or the Quebec Group. Mr. Rivot thus distinguishes between the general characters of the rocks composing the Bohemian Mts. and the overlying series of beds, and believes to have discovered in the former, which he regards as erupted, the source of the elevation and displacement of the latter, which he regards as more or less completely metamorphosed sediments.[9]

Not to discuss at present the peculiar notions of Mr. Rivot, it will be well to advert very briefly to certain remarkable misconceptions upon which they are based. First: There seems to be

[8] Foster & Whitney's Report, ii, 44.

[9] Rivot, Notice sur le lac Supérieur—Extrait des Annales des Mines, x, p. 50.

no evidence in support of Mr. Rivot's assumption that the greenstones associated with the hornblendic schists of the Marquette district are, like the copper-bearing rocks of Keweenaw Point, "traversed by veins containing copper and native silver." On the contrary, the vein phenomena of the crystalline schists forming the Azoic belt of Marquette county, are decidedly characteristic of the Huronian series, the lodes of which carry the sulphurets of copper unaccompanied by silver, and are altogether without the distinctive properties of the native copper lodes of Keweenaw Point. Second: The Paleozoic strata among which the greenstones of Keweenaw Point are intercalated conformably, and within the sectional development of which they are included, rest unconformably upon the crystalline schists of the Marquette district, and hence the stratigraphical identity between the rocks of these two districts, inferred by Mr. Rivot, is clearly out of the question. Third: The difference between the rocks composing the Bohemian Mts. and the superincumbent series is one merely of special lithological characters, and not one of general structure as assumed by Mr. Rivot. The syenite of Mt. Houghton has recently been found by Mr. W. H. Stevens, of Copper Harbor, to be intercalated upon the Reliance location with several massive beds of sandstone, conglomerate and amygdaloid, thus establishing uniform conditions of composition and structure to prevail throughout the entire series of the north-dipping copper-bearing rocks of the South shore.

Mr. Rivot was one of the first to assert the metamorphic character of the greenstones of the Marquette district, and, upon grounds of structural analogy alone, opposed the generally received opinion that the bedded syenites, diorites, dolerites, chloritic, zeolitic and epidotic amygdaloids, and so-called traps of the copper-bearing series, are erupted masses, having originated from successive overflows during the accumulation of the Paleozoic sediments in a horizontal position. Notwithstanding the stratigraphical arrangement of this series upon the South shore entirely bears out the theory of their metamorphic origin, certain observations of the Geological Survey of Canada upon the North shore tend to strengthen the view advanced by Messrs. Foster & Whitney.[10] If the explanation of trappean overflows on Thunder Bay, resting upon the authority of Sir William Logan, be confirmed by future investigation, the North shore must be regarded as the geological type of the Lake Superior country, and the absence of similar phenomena upon the South shore attributed to effects of denudation. If, on the other hand, the trappean overflows of Thunder Bay be found disconnected with the lower traps of the Upper copper-bearing series, but be ascertained really to belong to the system of dykes which traverse

[10] Geol. Survey of Canada, p. 70.

this series on the North shore, and which have no counterparts in Michigan, it will remain for special lithological investigation to complete the evidence of the metamorphic origin of the Upper copper-bearing rocks of Lake Superior on the grounds of analogy with the conditions of their development in Canada. That the question has been left open by the Canadian geologists, is apparent from the fact that the stratified greenstones of this series, in a chapter by Mr. T. Sterry Hunt, are described as the stratigraphical and lithological equivalents of the diorites of the Quebec Group examined by him in Eastern Canada, which he asserts to be altered sediments, and to pass into chloritic, epidotic, and hornblendic slates.[11]

Allusion has already been made to the regularity of the flexures of the Huronian strata in the Marquette district, and to the general parallelism, and approximate east and west bearings of their axes. This regularity prevails throughout the Iron region proper, of which the hills and valleys conform to the resulting system of undulations—thus corresponding to anticlinal and synclinal arches and basins. The distance from one axis to the next of the same character varies from one half of a mile to one mile. Departures from the uniformity of direction occur at different points, but they may generally be directly traced to local causes of displacement. The plications in the strata which thus impart to the district its topographical configuration, have been subjected to powerful agencies of denudation, and, consequently, by the unequal operation of these agencies, are more or less imperfectly preserved. Hence their regularity is less superficial than stratigraphical; for while through the general denudation of the district, in common with the larger portion of the continent, by glacial action, the original surface of the rocks has been removed in a comparatively even manner, local agencies of erosion and excavation have served to break the continuity of ridges, and entirely to obliterate prominent outcrops over considerable areas.

Of the various indications of drift agencies with which this region abounds, the rounded and polished rocks or "*roches moutonnées*," and the glacial furrows which these surfaces retain, are the most frequent and instructive. It is almost impossible to meet with an outcropping rock whose character is such as to resist weathering influences, the surface of which does not exhibit one or the other, or both of these appearances. The course of the glacial grooves is invariably N. from 50° to 70° E., quartering with the direction of the valleys. Two, and not seldom three, systems of furrows can be traced, varying however but few degrees and quite alike as to distinctness. The rounded, polished and grooved surfaces are seen upon the flanks of the

[11] Geol. Survey of Canada, pp. 612, 614.

ridges, as well as upon their summits, and are invariably revealed by the removal from the surface of a rock of the alluvium and glacial drift by which it has been effectually protected. It is only in the outcrop of fissile schists that these appearances are wanting. The dense vegetation and the undisturbed covering of drift, aided by the deep snows of winter, almost entirely prevent the weathering of the rocks, so that the phenomena of glacial action are exhibited in this region in uncommon perfection. Fragments of rock occurring near by *in situ*, and boulders of specular iron are scattered throughout the drift, which here generally in fact appears to be made up of material derived from the immediate region, as seen especially in the circumstance that it is exceedingly ferruginous, while the source of the iron as a local detritus is evidenced by its exclusive presence as anhydrous sesquioxyd. Foreign boulders of gneiss occur however, considerable in size and number within limited areas of distribution.

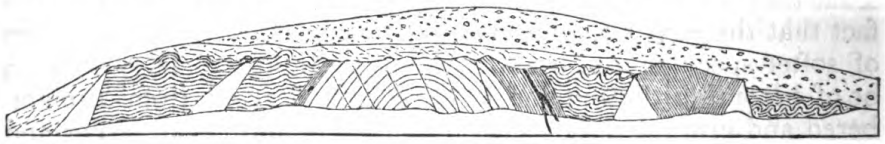
The position of the beds of specular iron ore has already been stated to be at the top of the Huronian series as developed in the Marquette region, and it has also been remarked that they are interstratified with talcose and argillaceous schists. Sharing the plications of the entire series, these specular schists, as they may properly be called, are accordingly folded into synclinal basins and anticlinal crests—of which the axes in the case of the former lie below drainage, in the bottom of the valleys, and in the case of the latter are commonly obliterated through the erosion of elevated outcrops. Hence their outliers have been chiefly preserved along the flanks of the ridges where they have been the least exposed to the agencies of denudation, protected as they were against a drift current coursing not with the valleys but obliquely across them, by the elevated outcrops of their underlying rocks from the summits of which however they have been abraded. The denudation has been most extensive upon the purer specular schists and the earthy red hematites, while the only exceptions to these conditions of erosion and preservation as already given, occur with specular schists which acquire from their composition a refractory structure, or from their mode of deposit, the property of resisting the effects of a sweeping denudation.

The bosses of specular-iron—the iron-knobs or mountains, as they are called—are the most striking examples of exception to the general effects of denudation already noticed. They are instances of the preservation of the anticlinal crest, and owe their permanence to the fact that they are made up, not of pure and soft specular schists, but of specular iron ore closely interlaminated with quartz or jasper—a structure capable of resisting denuding action to a far greater degree than the homogeneous schists. The laminæ of jasper alternating with pure specular

iron ore, vary in thickness from a hair's breadth to an inch, producing a banded appearance. The fact that the entire mass is highly contorted into minute as well as sizeable folds, furnishes an explanation of a power of resistance to vicissitudes of erosion, superadded to the same property which it derives from the mere presence of intermingled jaspery bands. The conglomeratic beds of specular iron ore and jasper, are generally preserved in elevated outliers, acquiring a refractory character from their coarse and tenacious structure, but I have met with no instance of the preservation of the anticlinal crest in beds of this description.

In Section 26, T. 47 N., R. 27 W., an example of a preserved anticlinal crest occurs in a ridge of half a mile in length and 170 feet in height. The specular iron-ore at the surface displays a laminated and contorted structure into which enter thin laminæ of quartz, and although the mass in itself is not so substantial as the exterior of the Cleveland Knob, its attending conditions of deposit impart to it similar refractory properties. In this region, as elsewhere, denudation may be noticed to have had less effect upon those stratified rocks which present to its agencies their planes of stratification, than upon those which are highly inclined. Hence it wiil be observed that the more extensive the sweep of the undulations the more complete is the preservation of the specular iron-ore which in the form of schists enters into them.

The most conclusive indications of the stratigraphical conditions which prevail among the hematite schists in Marquette county up to this time exhibited, are disclosed by the cutting of the Peninsula Railroad in the N.E. quarter of Section 8, T. 47 N., R. 26 W., to which allusion was made in the first part of this article. The grade of the railroad has been laid in an excavation 600 feet long and sunk 25 feet below the highest point upon the surface of a hill which derives its configuration from a boss of hematite schists intercalated with argillite. Thus the cutting presents an anticlinal section, as seen in the accompanying woodcut. The lower strata penetrated are preserved in an unimpaired



anticlinal crest, while the arch of the upper, sharing the general denudation of the district, has been abraded with the preservation only of the deflected dips of these strata. The schists are alternations of earthy red hematite and chloritic argillite, or the peculiar greenish slates, already described, both becoming gradually reduced in an ascending order from comparatively heavy beds of two feet to thin laminæ of a quarter of an inch. The

power of resistance in the arch is strikingly illustrated; for while the crest itself remains perfect, the same strata have been much disrupted and displaced as they pass out of the arch and change their curvature. The upper laminated portions are exceedingly wrinkled, and the heavy beds broken and doubled. Under these circumstances of displacement but one or two of the heavier beds of argillite are exhibited above the grade of the railroad; and while these, through scales or plates retain marks of stratification so as to expose their true character, they have been sharply folded and collapsed, and by their nearly vertical position have the appearance of intrusive masses. The central portion of the cutting—that is, the undisrupted strata—is traversed by a system of parallel joints which the disrupted portions are without. Several segregated veins of quartz with limonite occupy the divisional planes of stratification.

Other sectional cuttings in the several mines or quarries display more or less completely the stratification and undulations of the hematites in common with the Huronian schists of this region, and show conclusively, if further testimony were requisite, the existence of the iron ore under the same conditions of deposit and secondary modification as the schists with which they are associated. In the Jackson mine, for instance, occur two bosses or short anticlinal folds, while in the Lake Superior mine a perfect synclinal flexure is exhibited.

Beds of specular conglomerate are of frequent occurrence throughout the Iron region of Northern Michigan, consisting of a paste of specular peroxyd of iron, through which are disseminated fragments of jasper, and rounded pebbles of specular iron ore which usually differ from the paste in texture, a difference very perceptible among ores of any one class even within narrow limits of distribution. These conglomerates not unfrequently resemble breccia in the angularity of the jasper fragments which they contain; but the pebbles of specular peroxyd, although sometimes obscure in a matrix of the same material, commonly serve to indicate the detrital origin of these beds. That they are derived from local detritus is evident from the fact that the jasper fragments are not rounded, while the particles of softer specular iron ore are worn but slightly. They seem to be of littoral formation and to have been derived from dismembered and crumbled deposits of successive laminæ of jasper and iron ore—similar to those deposits distinguishable in the bosses of the region. The specular conglomerate invariably exists under circumstances of true bedding, and is traversed by parallel joints splitting the imbedded pebbles. It occurs interstratified with talcose and argillaceous schists quite as regularly as the homogeneous ores. As would naturally be expected, the specular conglomerates, owing to their enduring composition, have re-

sisted erosion more effectually than the purer schists, and are preserved in elevated outliers, as seen particularly at the New England mine, and in the N.W. quarter of Section 21, T. 47 N., R. 27 W. A specular conglomerate uncontaminated with any considerable portion of jasper, forms the bulk of the schists at present wrought by the Lake Superior mine. These are jointed obliquely, and are cleavable at right angles, to the stratification.

Specular schist often occurs charged with detrital quartz, or sand, thus differing from the conglomerates only in its external properties, while it is analogous to them in attitude and actual composition. Schists of this description are intercalated with the conglomerates in Section 21 above noticed, and in Section 26 of the same township are found underlying the laminated beds which crown the anticlinal arch.

An estimation of the thickness of the ferriferous series is attended with no little difficulty at present owing to the absence of any entire sectional cutting. The Jackson mine presents the best data for an opinion upon this point, although its workings expose neither the base, nor uppermost members of the series. It is upon the northern dip of a steep anticlinal, and the ground of the quarry is some 500 feet wide, the excavation having been entirely within the series. At this mine the range of thickness exceeds one thousand feet. Individual thicknesses of specular schist without the intervention of other schists, are upwards of 150 feet. Some idea of the massiveness of a homogeneous bed of specular schist may be had from the fact that in the month of September last 3500 tons of workable ore were thrown down at this mine in one blast, for which 11 kegs of powder were used.

It will be observed that while the smaller plications furnish the most available and complete evidences of the stratigraphical conditions of the ferriferous schists, every exposure of them in quarries or natural outcrops, conveys the same character of evidence, but upon a scale far more extended, and generally requiring allowance for superficial vicissitudes, and a large degree of denudation. Even if space permitted, I conceive it to be unnecessary to multiply instances of this evidence. It has been shown that the iron ores of the Huronian series in Michigan are essentially schists and heavy-bedded strata in which none of the phenomena of aqueous deposits formed by precipitation from water on the one hand, or by detrital accumulation on the other, are wanting. They exhibit not only stratification, anticlinal and synclinal folds, but are invariably traversed by systems of joints, and at many points exhibit a perfect slaty cleavage.

The intimate connection between the greenstones, hornblende rocks, and aluminous and magnesian silicated schists of the ferriferous series, has already been indicated in general terms, these rocks not only alternating with, but passing into, each other—a

conjunction commonly existing wherever bedded greenstones, and isolated silicates occur together as sedimentary products of the decomposition of compound silicated rocks. The peculiar green slates which have a large development in this series, are intermediate in composition between clay slate and hornblende slate, and together with the talcose and chloritic slates, with which they are interstratified, are probably products of such a decomposition in the wet way of the same crystalline sediments which entirely or less undecomposed have gone to form those greenstones which constitute members of the same series. Effervescence with acids of some of the green slates in common with many of the greenstones of the series, together with the presence of a large amount of lime and magnesia, was pointed out by Messrs. Foster & Whitney, as indicating the characters of a pulverulent greenstone—the schalstein of German geologists.[12] The presence of carbonate of lime in many of the schistose rocks of this region must be regarded as generally due to the decomposition of silicate of lime in the silicates of the primary crystalline sediments whence they are derived, although in a few special instances it may have been derived by the subsequent conversion of greenstones into schalstones, or by infiltration from superincumbent calcareous strata. Chemical reactions in crystalline sediments resulting from the disintegration of crystalline silicated rocks, and operated upon by carbonated waters, are amply capable to have produced the lithological conditions of augitic rocks, clay-slates, schalstone, and other schists, together with the oxydized ores of iron intercalated with greenstone among the ancient crystalline rocks of North America as well as Europe—as shown by Bischof[13] in pronouncing the Neptunian characters of these rocks. From a stratigraphical point of view, while evidence is elsewhere often obscure, the Huronian greenstone, schists and iron ores of Northern Michigan, in the absence of close attention to their special chemical conditions, exhibit sedimentary and metamorphic phenomena adequate to render quite untenable, it is believed, the theory of the exotic character of any portion of them.

New York, Dec. 19th, 1864.

[12] Report of the Geology of the Lake Superior Land District, ii, 17, 93.

[13] Bischof, Elements of Chemical and Physical Geology, London, 1854–59, vol. iii, chap. 55. Volger, Studien zur Entwicklungsgeschichte der Mineralien. Zurich, 1854.

ART. XXXVII.—*Astronomical Photograyhy;* by LEWIS M. RUTHERFURD.

MY present observatory is a circular brick building of twenty feet internal diameter, with a light revolving roof supported on twelve wheels which are fixed to the stone coping of the walls.

The opening, two feet wide, extends from side to side with simple shutters, which, when elevated on the weather side, serve to prevent the wind from blowing into the observatory and shaking the telescope. Opening from the west side of the Equatorial dome is a small transit apartment with computing room attached. This observatory is in the garden of the house where I reside. The transit is 189 feet N.W. from the Second Avenue, and 76·3 feet N.E. from Eleventh street. It was erected in the summer and autumn of the year 1856. The equatorial, by Fitz, is a very substantial instrument, having circles divided on silver 18 and 20 inches in diameter.

The objective is of 11¼ inches aperture, and fourteen feet focal length, and was corrected for figure by myself after the methods and directions of Mr. Fitz. It is a fine glass, capable of showing any object which should be seen by a well corrected objective of those dimensions.

The observatory is low and therefore cannot reach any object near the horizon, but I prefer losing such observations to the tremors and expense of a high structure.

The transit room has been used on several occasions by the U. S. Coast Survey in their telegraphic operations for longitude. It is $0^h.\ 12^m.\ 15{\cdot}47^s$ E. of Washington, and in latitude 40°, 43′, 48″·53; the latitude being the result of observations with the zenith telescope upon twenty-four pairs of stars by the observers of the Coast Survey.

During the winter of 1857–58, Messrs. Alvan Clark & Sons constructed, and in the spring attached to the equatorial, a driving clock of the highest merit. It has a remontoir escapement similar to that of Bond's spring governor.

Having seen with great interest the photographic experiments conducted at the observatory of Harvard College, I determined, as soon as the clock should be in working order, to prosecute the subject of celestial photography. After many experiments it was ascertained that the best photographic focus of the objective was about $\frac{7}{10}$ of an inch outside the visual focus. I continued making photographs of the moon and such stars as could be obtained, and although when compared with what had been done by others the results gave reason for satisfaction, yet in view of what was desirable and apparently attainable, astronomical photography with me was a failure. By reducing the aper-

ture of the telescope to five inches for the full moon, I was enabled to produce negatives which would bear an enlargement to five inches or fifty diameters. An impression of a sixth magnitude star was never obtained, γ Virginis, then 3″ distant, was the closest pair the duplicity of which could be measured on the collodion plate. The ring of Saturn and the belts of Jupiter were plainly visible, but entirely unsatisfactory. An image of Jupiter could be obtained in from 5 to 10 seconds exposure, but the satellites failed to impress the plate in any length of time. This was due to the uncorrected condition of the objective which diffused the violet rays over a large space, so that in the case of the planet each point of the picture was influenced not only by the ray due to that point, but by the stray beams from adjoining portions of the object, and thus nearly the whole actinic force of the objective was gathered within the dimensions of the image. In the case of the satellite the lost rays were not replaced by the wanderers from any adjacent point.

During the summer of 1858 I combined my first stereograph of the moon, producing quite a satisfactory result with the low power of the stereoscope. I do not know when this was first done in England by Mr. De La Rue, but with me the idea was an original one.

My greatest success with an uncorrected objective was in the pictures of the sun taken with about one-fiftieth of a second exposure, with the aperture reduced to one inch. The negatives were four inches in diameter and exhibited the spots with reasonable sharpness, the manifest difference in light between the center and the edge, and under favorable circumstances the faculæ. Some of the negatives verify the observation of M. Dawes, that the faculæ are elevations.

In June, 1860, the sun's disk was remarkably rich in spots, and I combined the pictures of two days to produce a stereograph, but the result was a failure and did not give the impression of a sphere, but presented the appearance of a flat uniform disk spanned by a spherical net-work which seemed entirely detached from the disk. This is attributable to a want of sufficient detail on the surface of the sun.

During the year 1859 and for a long time I worked with combinations of lenses to be inserted in the tube between the objective and the plate with the view of correcting the photographic ray. This attempt succeeded well so far as the center of the field was concerned, but it was impossible to produce a good correction over a space equal to the area of the image of the moon, without using a corrector of inconvenient size.

In 1860 I prepared a telescope with camera and instantaneous apparatus mounted equatorially to send by the U. S. Coast Survey Expedition to Labrador for the observation of the eclipse.

The objective in this case was a fine one, by Alvan Clark of 4¼ inches aperture. A ring was placed between the crown and flint lenses of such a width that the best visual and photographic foci were united. For this purpose it was necessary to shorten the combined focus about one-twentieth of its former value.

The pictures of the sun taken with this instrument were better than those made by my large telescope, in which no attempt had been made to correct the photographic rays.

Being unable to accompany the expedition, I made a series of pictures of the eclipse at home, upon which are seen the nuclei and penumbræ of the spots, the gradation of light of the sun's disk, and the serrated edge of the moon projected upon the sun. They show, however, none of the fogging of the moon's surface, commented upon by other observers, nor a greater intensity of light at the points of contact between the sun and the moon, both these results are, when they occur, due, in my opinion, to photographic or optical causes, and not to any true astronomical phenomena of that nature.

On examining the first negative of the eclipse I was struck by the difference of sharpness between the edge of the sun and that of the moon projected upon its disk. At first I was inclined to think that it was caused by a falling off in definition near the edge of the eye-piece used. In the next picture the edge of the sun was placed near the center of the field and the moon removed to a remote part of the plate, yet still the result was the same; the sun's edge was soft and indefinite while that of the moon was hard and sharp, showing that the light from the two objects comes to us under different conditions; in one case traversing the sun's atmosphere, in the other unaffected by this disturbing cause.

In the autumn of 1861 I began to experiment with a reflecting telescope with silvered mirror, which recommended itself both by the simplicity and ease of its construction and the entire freedom from dispersion. One was mounted of thirteen inches aperture and eight feet focus, of the Cassegranean form. It was ground and approximately figured by Mr. Fitz, and in its frame, as strapped to my large tube and carried by the equatorial clock, weighed less than fifteen pounds. Many modes were tried of silvering but the best results were obtained by Liebig's process, wherein the silver is deposited from an ammonia nitrate solution by sugar of milk. After three months trial I abandoned this instrument as unfit for use in my observatory. First, the tremors of the city, quite imperceptible in the achromatic, were, by the double reflection, increased about 36 times, an insurmountable obstacle to good work. Secondly, the silver deposit is so easily attacked, both by moisture and the gases which abound in the city, as to make it necessary to re-silver the specu-

lum at least every ten days, a labor not to be contemplated with equanimity. Dr. Draper has found the silver surface very much more durable in the dry, pure air of the country. I regard the Cassegranean form as the best adapted to lunar photography, since the dimensions of the image can be varied at will, as circumstances dictate, by simply changing the small mirror, a number of which might be kept at hand.

Having thus failed in astronomical photography with an ordinary achromatic, with a correcting lens and with a reflector, I began, in the autumn of 1863, the construction of an objective, to be corrected solely with reference to the photographic rays.

In a former communication to this Journal, Jan. 1863, I drew attention to the peculiar adaptation of the spectroscope as a means of examining the achromatic condition of an objective, and since it was principally by the aid of this instrument that I have been enabled to procure a fine photographic correction, I may be pardoned for touching again upon this application.

The image of a star at the focus of a perfectly corrected objective would be a point, the apex of all conceivable cones having the object glass, or parts of it, as the bases. This point falling upon a prism would be converted in a line red at one end and violet at the other with the intermediate colors in their proper places. If, however, the different colored rays are not all brought to the same focus, the spectrum will no longer be a line, but in the uncorrected colors will be expanded to a brush the width of which will be the diameter of the cone where intercepted by the prism. It will thus be seen that a simple glance at a star spectrum will indicate at once what parts of the spectrum are bounded by parallel lines and consequently converged to one focal point, and what parts do not conform to this condition, and also the amount of divergence.

On applying this test I found that an objective of flint and crown in which the visual was united with the photographic focus, (in other words, where the instrument could be focalized on a plate of ground glass by the eye, as in ordinary cameras, and in the heliographs constructed by Dalmayer for the Kew observatory and for the Russian government), is a mere compromise to convenience in which both the visual and actinic qualities are sacrificed.

In order to bring the actinic portion of the spectrum between parallel borders, i. e. to one focus, it is necessary that a given crown lens should be combined with a flint which will produce a combined focal length about one-tenth shorter than would be required to satisfy the conditions of achromatism for the eye, and in this condition the objective is entirely worthless for vision.

Having obtained the achromatic correction, I had a most delecate task to produce the correction for figure, since the judgment of the eye was useless unless entirely protected from the influence of all but the actinic rays. A cell of glass inclosing a sufficient thickness of the cupro-sulphate of ammonia, held between the eye and the eye-piece, enabled me to work for coarse corrections upon α Lyræ and Sirius, but so darkened the expanded disk of a star in and out of focus that all the final corrections were made upon tests by photography, which gave permanent record of all the irregularities of surface to be combated. Still, however the process was long and tedious, dependent upon but three stars as tests, and they too often obscured by bad weather. My mode of correction was almost entirely of a local nature, such as practiced by the late Mr. Fitz and Mr. Clark for many years.

This objective was completed about the first of December last; it has the same aperture, $11\frac{1}{4}$ inches, as the achromatic, with a few inches shorter focal length, and can be substituted for it in the tube with great ease. The corrections of this objective are such that I think it capable of picturing any object as seen, provided there be sufficient light and no atmospheric obstacles.

As respects the light, I have obtained images of stars designated by Smythe as of the $8\frac{1}{2}$ magnitude, and other stars on the same plate of full a magnitude lower. In the cluster Præsepe, within the space of one degree square twenty-three stars are taken, many of which are of the ninth magnitude, with an exposure of three minutes. An exposure of one second gives a strong impression of Castor, and the smaller star is quite visible with half a second. With the achromatic objective it was necessary to expose Castor ten seconds to obtain a satisfactory result.

The great obstacle which prevents the results of photography from realizing the achievements of vision is atmospheric disturbance. In looking at an object the impression is formed from the revelations of the best moments, and it is often the case that the eye can clearly detect the duplicity of a star, although the whole object is dancing and oscillating over a space greater than its distance. The photograph possesses no such power of accommodation, and the image is a mean of all the conditions during exposure. It is, therefore, only on rare nights in our climate that the picture will approach the revelations of the eye.

Since the completion of the photographic objective, but one night has occurred, (the 6th of March), with a fine atmosphere, and on that occasion the instrument was occupied with the moon; so that as yet I have not tested its powers upon the close double stars, 2″ being the nearest pair it has been tried upon. This distance is quite manageable provided the stars are of nearly equal magnitude. The power to obtain images of the 9th mag-

nitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens, and in some measure to realize the hopes which have so long been deferred and disappointed.

It would not be difficult to arrange a camera box capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved, and more sensitive methods devised.

On the 6th of March, the negatives of the moon were remarkably fine, being superior in sharpness to any I have yet seen. The exposure for that phase, three days after the first quarter, is from two to three seconds, and for the full moon about one-quarter of a second.

The success of this telescopic objective has encouraged me to hope that an almost equal improvement may be made for photography in the microscope, which instrument is more favorably situated for definition than the telescope, since it is independent of atmospheric conditions. Its achromatic status is easily examined by the spectroscope, using as a star the solar image reflected from a minute globule of mercury. Mr. Wales is now constructing for me a one-tenth objective, which, upon his new plan, is to be provided with a tube so arranged as to admit of the removal of the rear combination, and, in place of the one ordinarily used, one is to be substituted at will which shall bring to one focus the actinic rays.

ART. XXXVIII.—*Notes on Coal and Iron Ore in the State of Guerero, Mexico;* by the late N. S. MANROSS. (From an unpublished Report dated May, 1857).

THE region here described is situated near the Pacific coast, just west of the meridian of the city of Mexico, extending 170 miles west of this line, and about 75 miles from the coast.

We have not succeeded in proving that true coal exists in this part of Mexico. Of the reported localities which we examined, the *first*, that of Chilpaucingo, proved to be a bed of soft and peaty brown coal, which might answer perhaps for local purposes, but would not bear transportation. The *second*, near Huetamo, was an irregular vein of asphaltum resembling the "Albert Coal" of Nova Scotia. The slates here bore a strong resemblance to those of the Coal-measures. Other rumored deposits in Tempantitlan and Los Nuevos turned out to be in one case pitchstone, and in the other black tourmaline. The people

of the country have so little idea of what coal really is, that they are liable to mistake any black mineral for it. Their information is therefore very unreliable, and it is always best to conduct any examinations for coal by sections without regard to their reports. I am still of the opinion that true coal may be found in this country. The Carboniferous or Mountain limestone is largely developed here; and we have seen in several places shales and sandstones which in their lithological character strongly resemble those of the Coal-measures in other countries. What is still lacking is the evidence of fossils, to prove that a coal vegetation has once existed in these latitudes.

Our examinations for the metals have been highly successful. Iron ores of the very best quality, and inexhaustible in quantity, have been found in six different localities. The *first* is situated at a distance of four leagues from the town of Mescala. It is a vein 25 feet wide, and nearly vertical, and consists of solid magnetic ore. We have specimens from other similar veins in the vicinity, which we had not time to visit. There is no doubt of an abundant supply of ore in this region, and it could be readily transported to the river. It derives importance from the fact that it is upon the present direct road from Mexico to Acapulco.

The *second* great deposit is the Cerro Yman, or Magnet mountain, of Las Anonas Grandes, near Coyuca. This is also about four leagues from the river. It is a mountain of iron ore, fully equal to the celebrated ore of Missouri. It is about 300 feet high, half a mile long, and fully one-third of its bulk is pure magnetic ore. It also contains a bed of limestone in its summit. It is owned by a company who have some small furnaces near it, and are producing small quantities both of iron and steel from it.

Two other deposits, those of San Francisco and Singungao [Singangeo?], situated near each other, may be classed together as a *third* great locality. They are about two miles apart and about the same distance from the Mescala river. That of San Francisco is a conical hill about 150 feet high, consisting principally of pure magnetic ore. Its surface is covered with loose blocks of the same, from one to five feet in diameter. That of Singungao covers several acres near the summit of a mountain six or eight hundred feet above the river. Besides paving the ground over this space with large blocks of ore, the vein throws up several piles of ore in masses of ten or twelve feet diameter. Taken together or singly, these deposits are capable of yielding an unlimited supply of the very best ore. They are not worked at all at present. A *fourth* great deposit was met with near Villadero, within a day's ride of Zacatula. Here a bed of ore four hundred yards long and one hundred wide occupies the side and crosses the summit of a low hill. The surface is made up of blocks of pure ore from three to five feet in diameter.

The country around is well wooded and nearly level from this place to the Pacific.

The *fifth*, and, all things considered, the finest deposit we have seen, is situated near Chutla, on the road from Zacatula to Acapulco. It is not more than three miles in a direct line from the sea, and not more than six from a bay which affords a good and safe anchorage and landing. This deposit extends along the side of a mountain for more than a quarter of a mile, is several hundred feet wide and affords masses of ore of all sizes from ledges down to pebbles. The highest point of the bed is about 150 feet above the adjoining plain. On the upper side the deposit is bounded by a thick bed of soft limestone, affording an easy supply of flux. The ore is pure magnetic, and is the finest we have seen. The country around for many leagues has a fertile soil and is covered with dense forests of hard woods, which would produce an unfailing supply of the best charcoal. This location has special advantages in the quality and quantity of the ores, the convenience of fluxing materials and fuel, and the nearness of a good landing, with the whole of the Pacific coast for a market. I see no reason why the best qualities of charcoal iron could not be produced here as cheaply as elsewhere, with the advantage of freight and duties over any foreign competition in the supply of the Pacific coast of Mexico, and to a considerable extent also of the interior.

One other large deposit of iron was passed over, on the shore between Petatlan and Coyuguilla. It is a hill of large blocks of ore, situated immediately upon the shore, and beaten by a heavy surf. The ore was not seen in place, but the quantity was sufficient to show that a very large bed of it exists in the hill. The beach for several miles was blackened by sand derived from the disintegration of the ores.

We have information of another large deposit of iron ore, near the Hacienda of General Alvarez, which we may perhaps see on our way to Mexico.

The occurrence of so many and so rich deposits of iron, so widely distributed over the country, is interesting, from the assurance it gives that, whenever coal shall be discovered, there will be no want of iron ore within accessible distance of it.

Next to iron, copper appears to be the most abundant metal in this portion of Mexico. I shall defer to another occasion a description of the many localities of this metal which we have seen. One mine, that of Inguaran, has been worked since the time of the conquest, and is still producing well, though worked without the aid of machinery. Others in the same neighborhood, said to be equally rich, are entirely idle. The specimens and information which we have collected are sufficient to convince me that this portion of Mexico is one of the richest

copper districts in the world. Silver and lead ores occur in abundance, but I will not undertake to describe here the various localities of them which we have seen, or the specimens which we have collected.

I am fully convinced that this portion of Mexico, when properly opened by the application of intelligent enterprise, will prove one of the richest mining regions of America.

Art. XXXIX.—*Numerical Relations of Gravity and Magnetism;* by Pliny Earle Chase, M.A., S.P.A.S.[1]

In the fifth century before the Christian era, Leucippus and his disciple Democritus taught that heat is the soul of the world, the principle of life and intelligence, and that space is an infinite plenum, pervaded by material atoms too minute to be perceptible to the senses, which, by their constant motions, unions, and separations, form the beginnings and ends of things. In this theory, which is said to have been borrowed from the priests of Isis and Osiris, we may trace the origin of the modern belief in a universal kinetic æther, and of the attempts to resolve all forces into "modes of motion," which were practically inaugurated by our own countryman, Benjamin Thompson, Count Rumford, and which have been so successfully prosecuted by Carnot, Seguin, Mayer, Colding, Joule, Grove, and their collaborators.

The mutual convertibility of Light, Heat, Electricity, Magnetism, Chemical Affinity, and Vital Energy, may be now regarded as one of the most probable physical hypotheses. Faraday has endeavored also to connect gravitation and magnetism or electric action by experimental results, but in vain. Still, the conviction of such a connection is almost irresistible, and various physicists have given us incidental pointings in that direction. Ampère discovered the magnetic effect of electric currents circulating around iron bars; Arago, whose experiments were repeated and extended by Babbage, Herschel, Barlow, Christie, and others, showed that simple rotation produces magnetic disturbances which are governed by fixed laws; the distribution of induced magnetism in masses of iron, as determined by Barlow and Lecount, is the same as would follow from the relative centrifugal motions of different portions of the earth, provided the magnetic axis corresponded with the axis of rotation;[2] Hansteen suspected, and Sabine practically demon-

[1] From the Proceedings of the American Philosophical Society, Dec. 16, 1864. The Magellanic Gold Medal was awarded the author for this memoir.

[2] This fact was first announced by me, at the Society's meeting, April 15, 1864. See Proc. A. P. S., ix, 367.

strated, the influence of the sun upon terrestrial magnetism; Secchi ascertained that "the diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on a horary angle, and the second depends, besides, on the sun's declination,"[3] and that "all the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing that the sun acts upon the earth as a very powerful magnet at a great distance."[4]

This hypothesis has been objected to, on the ground that it is difficult to understand how any conceivable intensity of solar magnetism, by its simple induction, could produce so great a disturbance as is daily observed. Therefore it will probably follow the fate of the earlier ones, which attributed terrestrial magnetism to one or more powerful magnets lying nearly in the line of the earth's axis, while Barlow's idea that the magnetism is superficial, and in some manner induced,[5] will still remain in the ascendant. Secchi's conclusions are, however, none the less interesting, and from the fact that magnetism is, like gravity, a central force, varying inversely as the square of the distance, they lend encouragement to those who are endeavoring to find new evidences of the unity of force.

My own experiments and researches have led me to the belief that all magnetism is a simple reaction against a force which disturbs molecular equilibrium, that the numerical equivalent of the magnetic force is therefore equal and opposite to that of the disturbing force, ($\pm M = \mp D$), and that all the phenomena of terrestrial magnetism result from tidal and thermal changes in terrestrial gravitation.

Sullivan[6] and Reinsch[7] have pointed out the effect of musical vibrations upon the magnetic needle, and I have shown the controlling influence of a purely mechanical polarity.[8] A careful examination of the polarizing thermal and rotation currents,[9] will show that the spirals, which they have a tendency to produce, are quasi horizontal cyclones, one set flowing in a nearly constant direction along the magnetic meridian, and the other toward the momentarily shifting solar meridian. From an investigation of these currents and a comparison of various observations, I have deduced the following theses:

I. The daily magnetic variations, though subject to great disturbances, at different hours, show an average approximation to the differences of the gravitation-tidal currents.

Hours from Mean, - - - -	1h.	2h.	3h.
Means of Theoretical Ratios, - - -	·500	·866	1
" " Observed " - - -	·563	·865	1

[3] Phil. Mag. [4], viii, 396.
[4] Ibid. ix, 452.
[5] Phil. Trans., 1831.
[6] See De la Rive's Electricity, ii, 635.
[7] Phil. Mag. [4], xiii, 222.
[8] Proc. A. P. S., ix, 359.
[9] Ibid., p. 367 seq.

II. Marked indications of an accelerating force are discoverable in the magnetic fluctuations, especially during the hours when the sun is above the horizon.

Hours from Mean,	1h.	2h.	3h.
Mean Ratios of Hourly Tidal Differences,	100	73	27
" " " Squares of Hourly Magnetic Differences,	100	74	26

See also Thesis V.

III. There are lunar-monthly barometric and magnetic tides, which may be explained by differences of weight or momentum,[10] occasioned by the combined influences of solar and lunar attraction, and terrestrial rotation.

IV. The solar diurnal variations of magnetism between noon and midnight are nearly identical in amount with the variations of weight produced by solar attraction at the same hours.

The ratio of the solar to the terrestrial attraction for any particle at the earth's surface, being directly as the mass, and inversely as the square of the distance ($M \div R^2 = 354{,}936 \div 23{,}000^2$), is ·00067. The weight of any particle is therefore increased by this proportionate amount at midnight, and diminished in the same proportion at noon, making a total half-daily variation of ·00134 in the atmospheric weight, and consequently, according to my theory, in the terrestrial magnetism.

Theoretical variation, ·00134. Observed variation, ·00138.

V. The magnetic variations at intermediate hours, between noon and midnight, indicate the influences of an accelerating force, like that of gravity, modified by fluctuations of temperature, and by atmospheric or ætherial currents.

Every particle of air may be regarded as a planet revolving about the sun, in an orbit that is disturbed by terrestrial attraction and other causes. In consequence of these disturbances, there is an alternate half-daily fall toward the sun, and rise from the sun. By the laws of uniformly accelerated and retarded motions, the mean fall and the consequent mean magnetic disturbances should occur at $12^h \div \sqrt{2} = 8^h\,29'$ from midnight.

Theoretical mean, $8^h\,29'$. Observed mean, $8^h\,31'$.

VI. Some of the magnetic influences appear to be transmitted instantaneously, through the rapid pulsations of the kinetic æther; others gradually, through the comparatively sluggish vibrations of the air.

VII. The comparative barometric disturbances of the sun and moon exhibit an approximate mean proportionality between their comparative differential-tidal and magnetic disturbances.

Let the solar differential-tidal force be represented by A′, and

[10] I believe there can be no weight without some degree of momentum. See Proc. A. P. S., ix, 357.

the lunar by A″, the respective barometric disturbances by B′ and B″, and the magnetic disturbances by M′ and M″. If M′ and B″ are required, we have

	A′÷A″	B′	B″	M′	M″
Theoretical values,			·00012	·00144	
Observed · "	2·55	·00057	·00013	·00140	·0000255

VIII. The theoretical gravitation-variation of magnetism (Prop. IV) is slightly less, while the theoretical barometric variation (Prop. VII) is slightly greater, than the corresponding observed variation. The excess in one case exactly counterbalances the deficiency in the other, the sum of the theoretical being precisely equal to the sum of the observed variations.

IX. The total daily magnetic variations, like the barometric, can be resolved into a variety of special tides, which may be severally explained by well-known constant or variable current-producing and weight-disturbing forces.

Hours from Midnight.	A Theoretical Gravitation Tide.	B Theoretical Differential Solar Tide.	A+B Theoretical Mean Tide.	Observed Mean Tide.
0	−·00067	+·00024	−·00043	−·00043
6	·00000	−·00024	−·00024	−·00023½
12	+·00067	+·00024	+·00091	+·00095

The hours are counted from midnight, in each half-day.

Column A contains the hourly differences from mean weight, attributable to solar gravitation, with changed signs; diminution of weight being accompanied by increase of magnetism, and *vice versa*.

The form of the tide in column B is evidently such as should be determined by solar action. The magnitude of the tide is estimated by comparing the relative amounts of motion down the diagonal and down the arc of a quadrant ($·00067\times[1-(\frac{\pi}{4}-\frac{1}{2})]$ $=·00048$). The mean-tidal difference $[(·00067-·00048)\div 2]$ is very nearly equivalent to the average theoretical inertia-disturbance of weight. The atmospheric inertia at St. Helena, (regarding the fluctuations as uniform between successive hourly observations,) produces retardations of 59′, 85′, 26′, and 31′, at 0^h, 6^h, 12^h, and 18^h, respectively. The mean retardation is 50′, or $\frac{5}{7\frac{1}{2}}$ of a half-day. The theoretical daily gravity-variation being ·00134, the average variation in $\frac{5}{7\frac{1}{2}}$ of a half-day is $·00009\frac{11}{36}$, the mean tidal difference being $·00009\frac{1}{2}$.

The consideration of the moon's disturbance of the atmospheric gravitation is complicated by the magnitude of its differential attraction, the position of the center of gravity of the terrestrial system, the varying centrifugal force, and other circumstances involved in the lunar theory. Still there are indications, in the following synopsis, of the influence of gravity, sufficiently

striking to encourage a hope that our knowledge of the moon's perturbations may be improved by a thorough comparative study of the lunar astronomical, atmospheric, and magnetic tables.

Lunar-daily disturbances of Magnetic Force at St. Helena, in millionths of the total force.

Hours.	0	1	2	3	4	5	6	7	8	9	10	11	12
Before Lunar M.	+5	−1	+4	−2	−5	−5	−6	−3	−2	− 1	+14	+15	+16
After " "	+5	−1	−5	−6	−7	−6	+1	+1	−2	+18	+25	+22	+16
Mean,	+5	−1	−0·5	−4	−6	−5·5	−2·5	−1	−2	+ 8·5	+19 5	+18 5	+16
Rotation-Tide.	0	0	±4·5	±2	±1	±5	∓3·5	∓2	0	∓ 9·5	∓ 5·5	∓ 3·5	0

The above table shows, that

1. The moon's attractive force ($M \div R^2 = \cdot016 \div 60^2 = \cdot000004$) multiplied by the coëfficient of its differential attraction (2·55) gives ·0000113, which is nearly the same as the mean meridional magnetic disturbance [(·000005+·000016)÷2=·0000105].

2. The increase of magnetism at 12^h is nearly equivalent to the attractive force, multiplied by the square of the distance from the center of gravity of the system, and divided by the square of the earth's radius ($\cdot000004 \times 7707^2 \div 3963^2 = \cdot0000168$).

3. There is a tendency to equality of disturbances on each side of the meridian at 1^h and 8^h, as in the solar magnetic tide.

4. The greatest disturbance occurs at the hours of 10^h and 11^h P. M., both in the solar and in the lunar tide.

5. There are some indications of an increase of gravity, and decrease of magnetic force, when the tidal flow is toward the center of gravity of the terrestrial system, and *vice versa*.

6. The rotation-tide has the customary quarter-daily phases of alternate increase and diminution.

X. The phenomena of magnetic storms indicate the existence of controlling laws, analogous to those which regulate the normal fluctuations. See Proceedings Amer. Phil. Soc., Oct. 21, 1864.

The foregoing comparisons have been based on Gen. Sabine's discussions of the St. Helena records. It would be desirable, if it were possible, to confirm them by observations at other stations near the equator; but the need of such confirmation is in great measure obviated, by the variety of ways in which I have shown the probable connection of gravity and magnetism. At extra-tropical stations, the rotation tide becomes so preponderating that it is difficult to trace the diminished gravitation- and differential-tides, still I shall look confidently to a fuller development of the theory of tidal action, for future additional support to my views.

ART. XL.—*On the Origin and Formation of Prairies;* by LEO LESQUEREUX.

THIS paper is intended as a review of Prof. Winchell's new theory on the origin of the prairies of the Mississippi Valley,[1] and as a defense of my own views on the same subject.[2] I shall therefore omit all details relative to the surface of prairies, their conformation and appearance, and their geological and geographical distribution, since these details do not directly concern the elucidation of the question.[3]

From the brief mention made of my opinion concerning the formation of prairies, it appears to have been misunderstood by those who have quoted it, or rather it was entirely unknown to them. For this reason it is advisable to first expose, as they were originally given, the essential points of a simple explanation, which does not merit to be spoken of as a theory. I merely translate from my letter to Professor Desor:

"You well know that prairies are at our time in process of formation along the shores of the Lakes:—Lake Michigan, Lake Erie, etc. and also along the Mississippi and some of its affluents, especially the Minnesota river. The formation of these recent prairies, whose extent is not comparable to that of the primitive ones, is peculiar, and has a great analogy to that of peat bogs. Where the waves of the lakes, or their currents, strike the shores or the low grounds, and heap materials, such as sand, pebbles, mud, etc., they build up more or less elevated dams or islands, which become covered with trees as soon as they are raised above water. These dams are not always built upon the shores, and do not always even follow their outlines, but often enclose wide shallow basins, whose water is thus sheltered against any movement. There the aquatic plants, sedges, rushes, grasses, soon appear, and these basins become swamps, as may be seen near the borders of Lake Michigan. Though the forests may surround them, the trees do not invade them, even when the swamps become drained by some natural or artificial cause.

"Along the Mississippi and Minnesota rivers, the same phenomenon is observable, with a difference only in the process of operation. During a flood, the heaviest particles of mud are deposited on both sides of the principal current, along the line of slack water, and by repeated deposits, dams are slowly formed

[1] This Journal [2], xxxviii, 332.

[2] Letter to Prof. Desor in Bull. de la Soc. des Sci. Nat. de Neuchatel, Dec. 1856.

[3] Prof. Whitney in the 1st Chap. of the Report of the Geological Survey of the State of Iowa has given a clear and very accurate descriptive account of the prairies.

and upraised above the general surface of the bottom land. Thus, after a time, the water thrown on the bottoms by a flood, is, at its subsidence, shut out from the river, and both sides of it are converted into swamps, sometimes of great extent. Seen from the high bluffs bordering its bottom lands, the bed of the Minnesota river, in the spring, is marked for miles by two narrow strips of timbered land bordering the true channel of the river and emerging like fringe in the middle of a long continuous lake. In the summer, and viewed from the same point, the same bottoms appear transformed into a green plain, whose undulating surface resembles immense fields of unripe wheat. In reality, they are impassable swamps covered with sedges, rushes, and coarse grasses. By successive inundations, they become, by and by, elevated above the level of the river. They then dry up in summer, mostly by infiltration and evaporation, and when out of the reach of the floods, they become at first wet, and afterward dry, prairies.[4] In that way, admirable locations for river towns have been made. On the Mississippi, Prairie du Chien, Prairie la Fourche, Prairie la Crosse, etc., are, as indicated by their names, towns located on formations of this kind.

"These splendid patches of praries, though of a far more recent origin than the immense plains above them, are nevertheless true prairies. Although bordered on one side by the high-timbered banks of the bottoms, and still separated by a fringe of trees from the actual bed of the river, yet the trees do not enter them. This peculiarity of formation explains first the peculiar nature of the soil of the prairies. It is neither peat nor humus. It is a black, soft mould, impregnated with a large proportion of ulmic acid, produced by the slow decomposition, mostly under water, of aquatic plants, and thus partaking as much of the nature of peat as of that of true humus. In all the depressions of the prairies, where water is permanent and unmixed with particles of mineral matter, the ground is true peat.

"It is easy to understand why trees cannot grow on such kind of ground. The germination of the seeds of arborescent plants needs the free access of oxygen; and the trees, especially when they are young, absorb by the roots a great amount of air, and demand a solid point of attachment to fix themselves. Moreover, the acid of this kind of soil, by its particularly antiseptic property, promotes the vegetation of a peculiar group of plants mostly herbaceous. Of all the trees, the tamarac is the only species which, in our northern climate, can grow on a peaty ground, and this even happens only under rare and favorable circumstances,

[4] The lowest part of these fluvial prairies is of course farthest from the rivers along the bluffs. Here, generally, the percolation of water through the banks forms springs and deeper swamps, which are often transformed into peat bogs.

that is, when stagnant water, remaining at a constant level, has been invaded by the *Sphagnum*. By their absorbing power, their continuous growth and the rapid accumulation of their remains, these mosses slowly raise the surface of the bogs above water, and it is then, in this loose ground, constantly humid but accessible to atmospheric action, that the tamarac appears.[6]

"Now, let us examine the prairies according to this idea of their formation, and see if, from the first trace of their origin to their perfect completeness, there is any thing in their local or general appearance which is not explained by it or does not agree with it.

"The bay of Sandusky is now in process of transformation into prairies, and is already sheltered against the violent action of the lake by a chain of low islands and sand-banks, most of them covered with trees since a long time, at least, judging from the size of the trees. All these islands are built up with the same kind of material, that is, with lacustrine deposits, either moulded into low ridges under water, or brought up and heaped by waves and currents. Around the bay, especially to the southwest, there are extensive plains covered with shallow water. The bottom, in the depression toward the lake and where the aquatic vegetation is only at its origin, is sandy clay. But, in the more shallow places, the clay is already muddy and blackened at its surface by the detritus of the herbaceous vegetation which has grown upon it. Farther toward the borders, and in proportion to the shallowness of the water, the detritus thickens, and still farther, we have wet prairies, with exactly the same vegetation as that of the lake swamps, and a black soil with a substratum of clay, the same materials also as those of both the deeper and more shallow swamps of the lakes. In receding from the borders of the lake toward the high prairies, the transition from wet to dry prairies is by so insensible degrees that it would be impossible to fix a point of separation between them. All the surface appearances are the same. Vegetation is here and there modified by the presence of some peculiar species of herbaceous plants, but nothing else. The homogeneousness of the soil is still more striking. There is the same kind of clay as subsoil, and this is overlaid by the same kind of black spongy mould. And if here and there you see knolls covered with trees, the cuts of the railroad show that the materials of which they are formed are a different compound from the ground of the prairies, even if they are scarcely elevated above the general level, and that they are of the same nature and of the same formation with those of the low wooded islands of the lake.

On the borders of the numerous lakes which dot the high

[6] In my letter to Prof. Desor, this question is discussed at length. I have omitted here the details, as they are unnecessary for the elucidation of the subject.

rolling prairies, especially in Minnesota, the process of formation of the prairies is repeated in the very same way. These lakes are of every size, sometimes small and circular, true ponds; sometimes thirty to forty miles in circumference, and in this case shaping the outlines of their shores according to the undulations of the prairies; divided into innumerable shallow branches, mere swamps, covered with water plants, and emptying themselves from one to another; passing thus by slow degrees toward rivers, not by well marked channels, but by a succession of extensive swamps. These are the sloughs which separate the knolls of the prairies—so to speak, the low grounds of the rolling prairies. They are nearly dry in summer, but in the spring are covered by one to three feet of water. Their vegetation is merely sedges and coarse grasses. I have never seen fishes in these sloughs, but plenty of crawfishes and a great quantity of freshwater shells.[6] Wherever the borders of the lakes are well shaped, not confounded with or passing into swamps, they rise from five to six feet above the level of the water and are timbered, mostly with oak and hickory. This elevated margin is more generally marked on the eastern sides of the lakes, a record of the action of the waves under the dominant winds. Or it is shaped as a low range of hills surrounding the lakes, and is due then to original irregularities of the surface. The materials are the same as those of the dams, or low islands, of the great lakes; indeed, the same as those of the under bottoms of the swamps, or those over which the prairies have been formed. But they have been removed from the influence of stagnant water: this is the only difference."

"From all these remarks, what other conclusion can we deduce but that all the prairies of the Mississippi valley have been formed by the slow recession of sheets of water of various extent, first transformed into swamps, and, by and by, drained and dried. The high and rolling prairies, the prairies around the lakes, those of the bottoms along the rivers, are the results of the same cause, and form a whole, an indivisible system."[7]

"To this assertion you will object, I suppose, and say: How is it that the prairies are not everywhere perfectly horizontal; and as there is some unevenness of surface, have these undulations not been formed like the low islands or high borders of the lakes, and why then are they not timbered?"—"I believe that, although the surface of the prairies may be now undulated, it was originally horizontal enough to form shallow lakes, and

[6] Especially *Planorbis trivolvis, P. lentus, Lymnea appressa, L. emarginata, L. decidiosa* Say, etc. The lakes have the same species, with many bivalves, and a great abundance of fishes, especially catfishes (*Pimelodes*).

[7] Atwater, this Journal, i, 116, 1819, and Bourne, ibid., ii, 30, 1820, have both considered the prairies as originating from swamps, without, however, giving an explanation of the phenomenon.

then swamps, like those which at the present time cover some parts along the shores of Lake Erie, Lake Michigan, etc. Where this horizontality has disappeared, it is only by very slow degrees, under the eroding action of water, which, in its slow movements, tends to follow every change of level, seek an outlet, and so establish channels of drainage. I have followed for whole days the sloughs of the prairies, and have seen them constantly passing to lower and well marked channels, or to the beds of the rivers, by the most tortuous circuits, in a manner comparable to the meanderings of some creeks in nearly horizontal valleys. Indeed, the only difference is, that, in the high prairies, there is not a definite bed, but a series of swamps, extending, narrowing, and bending in many ways. The explanation appears to me so natural, that I cannot understand how high prairies could ever be perfectly horizontal. Along the lakes and in their vicinity, the horizontality is a necessary consequence of the primitive evenness of the bottom and of the proximity to water. The level of the low prairies being scarcely above that of the lakes, their surface, after an overflow, becomes dry, rather by percolation and evaporation than by true drainage. But wherever the rivers have cut deeper channels,—as is the case in the north part of the Mississippi basin, where they run sometimes from one to three hundred feet lower than the surface of the high prairies—the drainage has constantly taken place toward those deep channels, and the water, though its movements may be very slow, furrows the surface in its tortuous meanderings. From this, results that irregular conformation of surface generally and appropriately called *rolling*. In Indiana and Illinois, in the vicinity of the Wabash river, for example, there are some high prairies whose surface is apparently horizontal. But these prairies, as at Terre Haute, are surrounded by a margin of low wooded hills, and have originally been shallow lakes of difficult and slow drainage. Moreover, their horizontality is rather apparent than absolute; some parts of them are already dry enough to be cultivated and ploughed in the spring; some parts are used as wet meadows, and still others are covered with water and inaccessible. This apparent horizontality results from the great width of what we may call already channels of drainage. These will, by and by, contract and deepen, and thus the prairie become undulating. This opinion is in contradiction to what you say in your excellent paper on the drift of Lake Superior: that the irregularities of surface of the prairies have been caused by currents at the time when they were under water. If the ground of the high rolling prairies had been prepared in advance, as you suppose, the prominent parts or the knolls would have been timbered like the low islands of the lakes."

Next follows, in my letter, an examination of the evidence that the prairies have been covered with water to their highest points; that the water has not been drained by any violent action but by slow upheaval of the surface; that the deepening of the great channels, and the formation of low prairies, around the lakes and along the rivers, are merely a continuation of the same phenomenon or a result of the slow and continuous upheaval of the great Mississippi basin.

The letter ends in refuting opinions, admitted by some authors, that the absence of trees on the prairies is caused by atmospheric dryness, and by others that it results from fires, etc.

After reading this exposition of my views, Prof. Desor remarked that what he calls my theory has the merit of combining in the same explanation two phenomena, the formation of the peat bogs, and the formation of the prairies, whose relation to one another is too natural to be without foundation. But he objects to some of my ideas, and especially thinks that my explanation of the formation of the *rolling* of the high prairies cannot be admitted, because the spaces between the knolls are not deep and narrow sloughs or simple trenches, but broad depressions, broader than the knolls themselves; that this could not be the case, if they had been formed by the erosion of water.

This objection, I think, is groundless. As we have seen, in considering the surface of the low and of the flat prairies, wherever the drainage is insensible, water can scarcely have any action in digging trenches. In the spring, or after heavy rains, its slow movements extend over the whole breadth of the low grounds, scarcely displacing or carrying away the finest material. This can hardly be considered as *erosion*. Nevertheless, it is certain that all the sloughs of the rolling prairies find their way to lower and deeper channels, where they definitively shed their water. It is certain, also, that in reaching the Mississippi, or the Minnesota, etc., these sloughs are deeper and all nearly perpendicular to the direction of the rivers. For this reason the *rolling* of the prairies along the great rivers resembles a succession of fronting abutments. Prof. Whitney, in the first chapter of the geological report of Iowa, makes a corresponding remark, when he says, (p. 17,) "The streams of the prairies usually take their rise in small depressions of the high prairie, scarcely to be noticed as being below the general level of the region. As their course continues, the beds generally sink, etc." Prof. Desor holds that the knolls of the prairies were formed under water, and in deep water, and, to confirm his assertion, he says that the bottoms of our great lakes, and of the ocean also along the shores, are marked by swells and deep furrows. In fact, the knolls of the prairies are of quite a different form from the long continuous parallel undulations of the bottom of the seas. Moreover, there

are such traces of marine action in Iowa and Minnesota in the timbered *Coteaux*, as the Coteau des Bois, Les Bois-rouges, etc., which cut the nakedness of the prairies and resemble those narrow strips of land bordering the ocean and soon becoming long peninsulas covered with a luxuriant arborescent vegetation. The first origin of these undulations may have been in deep water, or they may have resulted merely from the action of the waves. In any case, they cannot be compared with the barren knolls of the prairies.

Prof. Whitney,[8] admitting that the prairies have been covered with water, even to their highest point, considers the absence of trees as caused by the fineness of the soil, which he attributes in part to the nature of the rocks underlying it, and in part to *accumulation in the bottom of immense lakes of a sediment of almost impalpable fineness, under conditions which will be discussed hereafter.*[9] This unfinished explanation cannot fully satisfy the mind. Prairies cover every kind of geological formation, even granitic rocks, as in Minnesota, between St. Peter and Fort Ridgely. Most generally they overlie the Drift. It is evident that the black soil of their surface, as well as the clayey subsoil, though the thickness of these strata may be great, has been formed, in place, by the agency and growth of a peculiar vegetation. In stagnant water, whenever water is low enough to admit the transmission of light and air in sufficient quantity to sustain vegetable life, the bottom is first invaded by Confervas, especially by *Characeæ* and a *peculiar* kind of floating moss (*Hypnum aduncum* Hedw.). These plants contain in their tissue a great proportion of silica, lime, and even oxyd of iron.[10] Moreover, they feed a prodigious quantity of small mollusks, whose shells add to the detritus of the plants, and the final result of the decomposition of the matter is that fine clay of the subsoil of the prairies, truly impalpable when dried and pulverized. This formation has been observed and described long time ago, especially by Chrome, who attributes to the decomposition of Confervas, Characeæ, etc., the clay over which peat bogs generally rest. I have seen it in process of prodigious activity in a large pond of the King's Garden of Fredericksburg in Denmark, which every year is filled with about one foot of clayey matter, by the decomposition of Characeæ, small mollusks and infusoria. This kind of formation is so general that it can be observed in nearly every open swamp. In the lakes of the high prairies it has sometimes a peculiar character. At the depth of from one to three feet, the plants

[8] Geological Survey of Iowa, chapter i.

[9] Geological Report of Iowa, i, 25.

[10] DeCandolle *Physiologie Vegetale*, p. 183 and 188. When exposed to atmospheric influence, the Charas become covered with an efflorescence of scarcely carbonated or pure lime.

above named, Mosses, Confervas, and Charas, form a thick carpet, which hardens, becomes consistent like a kind of felt, and, floating about six inches from the bottom, is nearly strong enough to sustain the weight of a man. The carpet is pierced with holes, where fishes pass in and out, and the bottom under it is that fine impalpable clay, evidently a residue of the decomposition of its plants. At the depth of three and a half to four feet, this vegetation suddenly ceases and the bottom of the lakes is pure sand and pebbles with shells. Nearer to the borders, on the contrary, at the depth of one foot, the carpet of mosses, etc., begins to be intermixed with some plants of sedges, becoming more and more abundant in proportion as the depth decreases. As soon as the leaves of these plants rise out of the water, they absorb and decompose carbonic acid, transform it into woody matter, under atmospheric influence, and their detritus is at first clayey mould, and then pure black mould, the upper soil of the prairies. Of course, near the borders of the rivers, or under peculiar circumstances, the formation is somewhat modified by the addition of transported matter, or of foreign elements. The clay may thus take a different color, and have a somewhat different composition; but the process of formation does not materially change.

Considering the whole explanation of the formations of the prairies, as it is exposed in this paper, I think that it covers the whole ground and can apply to most of the cases (if not to all) where the ground is naturally naked or without trees. It gives the reason of the formation of the prairies from the base of the Rocky Mountains to the borders of the Mississippi River; of the prairies around the lakes, and of those of the broad flat bottoms of our southern rivers; of the Platas of the Madeyra river; of those of the Paraguay; of the pampas of Brazil; and even of the desert plains of our western Salt Lakes. For this formation is produced, in the same manner, in the salt marshes of the sea, as in the freshwater swamps of our lakes. And if, passing to other continents, we examine, in Europe, the natural meadows of Holland, the barrens or heaths of Oldenbourg, all the plains on the shores of the North and of the Baltic Sea, and, in Asia, the vast steppes of the Caspian, etc., etc., we find everywhere the same appearances, and the same results of a general identical action, modified only by local and mostly climatic circumstances.

The glades on the slopes of some mountains of Arkansas, of the Alleghanies, etc., have been quoted as a phenomenon contradicting the theory of formation of prairies by water.[11] I have carefully examined those glades in connection with the geologi-

[11] R. W. Wells, this Journal, i, 334, 1819.

cal survey of Arkansas, and found them always the result of water. When the springs descending the slopes are stopped by some obstacle, the waters slowly overflow the surrounding surface, and generally favor the growth of a thin stratum of peat, where only grasses and prairie flowers vegetate. Sometimes the peat of these glades is one to two feet thick. They are generally dry in the fall, but always true swamps or wet prairies in the spring.[12]

Prof. Winchell observes that *a theory* [for the absence of trees] *often urged is the considerable humidity of the soil of certain prairies and especially the wetness of the subsoil*, &c., and refutes it by this mere assertion: *it is singular that such an opinion could be entertained when it is so well known that there is no situation so wet but certain trees will flourish on it. The willow, cottonwood, tupelo, water oak, tamarac, American arbor vitæ*, etc.[13]. And, when considering Prof. Whitney's supposition that the extreme fineness of the prairie soil is the cause of the absence of trees, he sets it aside, in the same way, by another assertion: that *the fatal objection to this theory, and all theories which look to the physical or chemical condition of the soil for an explanation of the treeless character of the prairies, is discovered in the fact that trees will grow on them when once introduced.*[14]

As it is not proper to refute an assertion by a contrary one, let us examine under what circumstances trees may grow in some swamps, and what the highest scientific authorities have to say on the subject.

It is a well known fact of botanical physiology that trees absorb by their roots a certain amount of oxygen necessary to their life. It is in accordance with this principle that trees, to thrive well, ought not to be planted too deep; that most species of trees perish when their roots are buried in a stratum of clay, impermeable to air; that whenever the water of a creek is dammed, to make a pond, all the trees are killed on the whole flooded space. Running water furnishes a sufficient amount of air and oxygen for their life to certain species of trees (most of the species quoted by Prof. Winchell) whose roots, when immersed, have the property of dividing themselves into innumerable filaments. Hence, such trees grow, indeed, in those swamps inundated by the water of adjacent rivers, or periodically in-

[12] The relation of glades to peat bogs is beautifully exemplified on the slopes of Mt. Marcy and other peaks of the Adirondac Mts. of New York. Here, in the middle of deep and nearly impenetrable woods, such openings, half prairies, half bogs, are suddenly entered without transition whatever, and their surfaces entirely barren of trees, appear like clearings and meadows produced by human agency. They are of every size, cover slopes of various degrees, and ascend as high as to 5000 feet above the sea.

[13] A. Winchell on the prairies. This Journal, [2], xxxviii, 343.

[14] A. Winchell, ibid., 344.

vaded by the tides. The water of such swamps is not permanent, and its whole mass is subjected to some kind of movement. It is thus that in the south the bald-cypress and the tupelo grow even in the middle of creeks and bayous. But look anywhere else, along the rivers, or on the shores of seas or lakes, and wherever a sheet of shallow water is sheltered against the waves, the tides or the currents, you find invariably treeless swamps, passing to prairies.[15] The only fact, to my knowledge, which could be mentioned as sustaining the assertion of Prof. Winchell is, that certain kinds of shrubs, as the button bush, the swamp rose, etc., grow and form thickets around some true swamps of our forests. But, examining the process of germination of the seeds, it is easy to observe that the germs are not developed in water, but in dried decayed vegetable matter of rotten prostrated trees. In summer, and only when the margins of the swamps are dry, the roots penetrate the ground. Thus the swamp is surrounded by a belt of shrubs, but its central part is open and occupied by herbaceous plants only.

Now, what says DeCandolle in his classical book on Vegetable Physiology? He remarks that the constant irrigation required for the rice culture in Lombardy is a serious inconvenience, because the water penetrates the ground of the neighboring estates and *kills the trees*:—That water, left stagnant for a time on the ground, rots the trees at the collum, prevents the access of oxygen to the roots, *and kills them*:—That in the low grounds of Holland they dig, for planting trees, deep holes, and fill the bottoms with bundles of bushes, as a kind of drainage for surplus water as long as the tree is young enough to be *killed by humidity*:—That true swamps and marshes have no trees, and cannot have any, because *stagnant water always kills them.*[16] Authorities to the same effect could be quoted by volumes.

The second assertion, *that trees will grow on the prairies when once introduced* (or planted, I suppose), is certainly true. But we should take care to make a distinction between the results of an artificial process and those of a natural one. When trees are planted on the prairies, the soil is conveniently prepared. The clayey subsoil mixed with the black mould forms a compound which combines density of certain parts with lightness of others, and contains a great proportion of nutritive elements. If the clay of the subsoil is not too thick to be impermeable to water, and thus retain it around the roots, this prepared or artificial ground is indeed very appropriate to the growth of trees. But has any one ever seen oak or hickory, or any other kind of trees, grow on the prairies from a handfull, say even from a

[15] Species of trees like the magnolia grow over the southern peat bogs for the same reason that tamaracks grow in the peat bogs of the north.

[16] Physiologie Vegetale, pp. 1206–1212.

bushel, of acorns or nuts thrown upon their surface? Why, then, if trees *will grow* on prairies, do not isolated or widely separated clusters of trees insensibly cover a wider area? Some of these trees have lived there for ages; their trunks are strong and thick; their branches widely expanded, and their fruits are swept far away by the impetuosity of autumnal storms; nevertheless, their domain is restricted, by the nature of the ground, to their own narrow limits; these they never pass.

[*To be continued.*]

ART. XLI.—*On a Process of Fractional Condensation: applicable to the Separation of Bodies having small differences between their Boiling-points;* by C. M. WARREN.[1]

IT is well known that the process in general use for the proximate analysis of mixtures of volatile liquids,—viz: that of simple fractional distillation, either from a tubulated retort or from a flask with bulbs, as proposed by Wurtz,[2]—affords but very imperfect and unsatisfactory results, and not unfrequently leads to gross errors and misconceptions, except in those cases in which the boiling-points of the constituents are widely different, or in which some auxiliary method can be advantageously employed.

The want of a more efficient process for effecting such separations has long been recognized. There are numerous natural and artificial products, of the highest scientific interest,—such as petroleums, essential oils, tars, and other mixtures of oils obtained by the distillation, under varied circumstances, of bituminous, vegetable, and animal substances,—of which it may at least be said that we have but very imperfect knowledge,—I might almost say no knowledge, except such as could be derived from the study of very impure materials,—still mixtures of different bodies,—with which, instead of the pure substances sought for, chemists have felt compelled to content themselves, as the best results which they were able to obtain by the means at their command.

In repeated instances, apparently after persevering and protracted efforts, investigators have been forced to assert either the impossibility, or their inability, to obtain, from such mixtures, bodies of constant boiling-point,—a property which is generally received as a test of purity for liquid bodies.

I may here specify a few recent instances of this kind.

[1] From the Journal of the Acad. Arts and Sciences, Boston, May 10th, 1864.

[2] Annales de Chimie et de Physique, [3], xlii, 132.

1. Warren de la Rue and Hugo Müller,[3] in their paper entitled "Chemical Examination of Burmese Naphtha and Rangoon Tar," after detailing the preliminary treatment by distillation in a current of steam, add that "A further separation of the various products was effected by repeated fractional distillations; but no absolutely constant boiling-points could be obtained, notwithstanding the great number of distillations and the large quantity of material at command. It is true that considerable portions of distillates could be collected between certain ranges of temperature, tending to indicate a constant boiling-point; nevertheless, it soon became evident that distillation alone could not effect the separations of the various constituents, and that recourse must be had to other processes." The other processes resorted to were, treatment with sulphuric and nitric acids, either separately or mixed; but still with very imperfect results. This acid treatment, which was first proposed by De la Rue, and subsequently employed by C. Greville Williams,[4] Schorlemmer, and others, will be further noticed below.

2. Frankland,[5] in speaking of a mixture of the hydrocarbons of the formulæ C_nH_n and C_nH_{n+1} (now generally considered as C_nH_{n+2}), which have a difference of 6° to 7° C. between their boiling-points, says, "The separation of two such bodies by distillation alone is impossible;" and suggests that the employment of anhydrous sulphuric acid may accomplish the object by dissolving out the body of the formula C_nH_n.

3. And so recently as 1862, Schorlemmer,[6] in his first paper "On the Hydrids of the Alcohol-Radicals existing in the Products of the Destructive Distillation of Cannel Coal," remarks that "it was, however, found impossible to obtain a product of constant boiling-point by repeated fractional distillations;" and he also had recourse to the acid-treatment above referred to.

4. Pebal,[7] after an elaborate research on the petroleum from Galicia, in which Wurtz's bulbs were employed, and also Eisenstuck,[8] who made an extended investigation of the petroleum from Sehnde, near Hannover, also with the use of Wurtz's bulbs, both assert in the most positive manner the impossibility

[3] Proceedings of the Royal Society, viii, 221.

[4] Philosophical Transactions, 1857, 447.

[5] Quarterly Journal of the Chemical Society, 1851, 3, 43.

[6] Journal of the Chemical Society, xv, 419.

[7] Annalen der Chemie und Pharmacie, cxv, 20, asserts the "Unmöglichkeit, das Gemenge durch fractionirte Destillationen zu entwirren."

[8] Annalen der Chemie und Pharmacie, cxiii, 169, says as follows: "Mit den 5° zu 5° aufgesammelten Destillaten wurde die fractionirte Destillation wieder von Neuem vorgenommen, aber nachdem diese Operation sieben Wochen mit etwas 50 Pfund Steinöl fortgesetzt worden war, erhielt ich doch kein Product von irgend constantem Siedepunkt. Nach diesen Versuchen halte ich es für Unmöglich. das Steinöl durch fractionirte Destillationen allein in Producte mit constantem Siedepunkt, zu scheiden."

of separating from petroleum, by fractional distillation, products of constant boiling-point.

Such is the general character of the results obtained in the attempts which have been made to separate the constituents of such mixtures by fractional distillation.

The treatment with strong acids, etc., as an auxiliary to the common method of fractional distillation, which is claimed to have given good results in some cases, is open to serious objections in its application to mixtures of unknown substances, as must be readily apparent. The further consideration of this subject is reserved for another occasion, when I shall submit the results which I have obtained by my process in the study of mixtures almost identical with some of those in the investigation of which the acid process has been employed. I shall then be able to show that the results obtained by that process are, to a considerable extent, inaccurate and by no means exhaustive; and that it is still of the highest importance to have a process which shall be generally applicable in all such cases, without resort to any harsh or uncertain treatment.

With regard to the value of constancy of boiling-point above referred to, as a test of purity of a liquid substance, I may here say that, without scarcely lessening the importance of obtaining constancy of boiling-point, before resorting to harsher treatment, in the study of mixtures of unknown substances, I think I shall be able to show, on another occasion, that this property is not necessarily indicative of so high a degree of purity as has generally been supposed; and that a body may have a constant boiling-point, and yet contain enough of a foreign substance to appreciably—and, in delicate cases, seriously—affect the determination of its constitution and of some of its other properties. But in no such case have I yet found that the removal of the impurity by chemical means has essentially changed the boiling-point,—i. e., never to the extent of 1° C. of temperature. I propose, at a future time, to study this question synthetically, operating with pure liquid substances, with the view to determine, in a few cases, how much of a foreign substance may be present,—which would probably be variable in different cases,—without sensibly affecting the boiling-point. A solution of this question would, I think, be of considerable practical value in some instances.[9]

Of the New Process.—The chief distinctive feature of my process, as compared with the common one, consists in this,—that the operator has complete and easy control of the temperature of the vapors given off in distillation; and consequently can

[9] Since this was prepared for the press I notice that late experiments by Berthelot go to show the correctness of my conception of the value of constancy of boiling-point, as above stated.

readily cool these vapors to the lowest limit of temperature which the most volatile portion, under the circumstances, is able to bear and retain its vaporous condition. It will be seen at a glance that, under these conditions, the operator has it in his power to secure in any case the very largest possible amount of condensation of the heavier from the lighter vapors. The liquids resulting from the condensation of the less volatile portions of course fall back into the retort, while the vapors of the more volatile parts continue to go forward to a cold condenser, descending in the opposite direction, from which the condensed product falls into a special receiver. In this manner he is able to obtain, in each successive operation, a series of products which shall contain the minimum quantity of the less volatile constituents, which a single distillation is capable of affording.

Of the common process, on the contrary, nearly the reverse of all this is true: the operator having no control whatever; being forced to receive the vapors at the temperature which they naturally acquire in passing from the retort, and laden with such proportion of the less volatile bodies as may be carried forward with them.[10]

[10] The only apparatus, of which I have any knowledge, which can be regarded as bearing any analogy to my own, is that employed in the rectification of alcoholic spirits, on a manufacturing scale. In one of the older forms of this apparatus, that of Solimani, to which my attention was first called by a friend, after my process had been in use more than a twelvementh, the temperature of a dephlegmator is kept within such limits as to give alcohol of any required strength more readily than by the common methods. The mode of construction of this apparatus is, however, only adapted to manufacturing purposes, and it could not be utilized in the more exact experiments required in scientific research. Either on account of its complication, or some other cause, the apparatus of Solimani has, I believe, long since been abandoned.

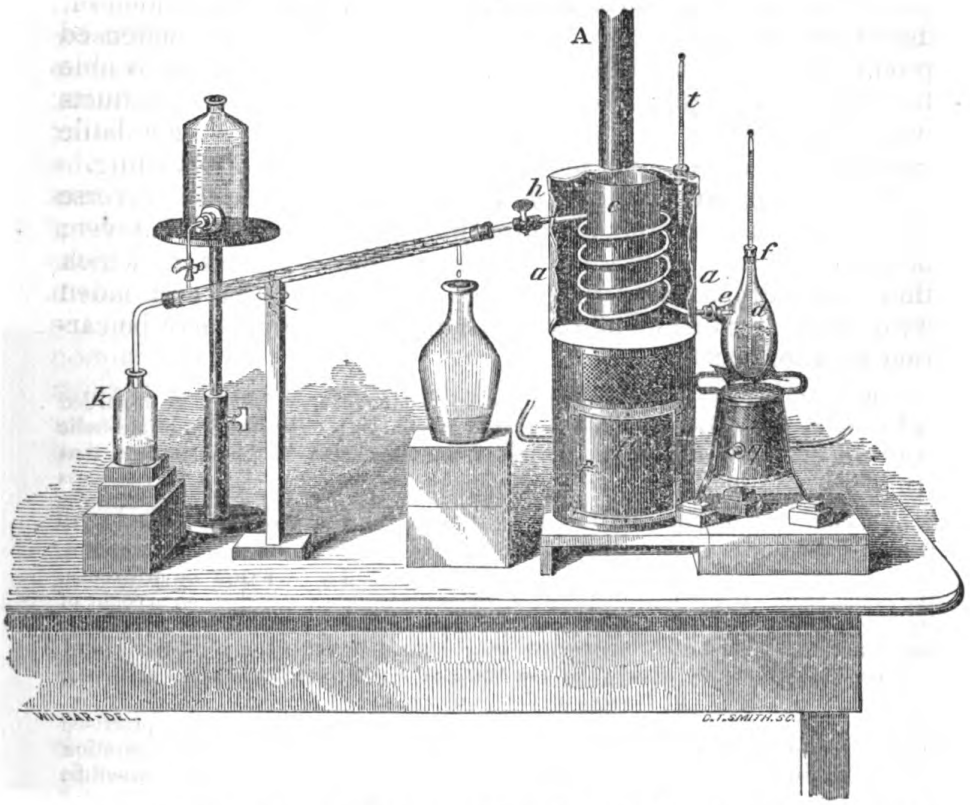
Mansfield (*Quarterly Journal of the Chemical Society*, 1849, i, 264), observing that "the boiling-point of benzole is the same as that of alcohol of sp. gr. 0·825," remarks that "any of the summary processes of rectification which are practised by distillers in the manufacture of alcoholic spirits, are applicable to the separation of benzole from the less volatile fluids of naphtha;" and, appended to his scientific treatise on coal-tar, under the title "*Of a Practical Mode of Preparing Benzole*," goes on to describe a process for that purpose, which, I believe, he had previously patented. It appears that Mansfield did not employ this process in his research, but obtained his benzole, as well as the other less volatile hydrocarbons, in the usual manner,—by simple distillation.

In the belief that no process of fractioning at all analogous to mine has ever been employed in scientific research, and that I am not in any way directly indebted to any of the devices of my predecessors, I have taken no special pains to consider these devices in much detail. I may say, however, that I have found no record of any one's ever having employed the oil bath and a separate fire to regulate a heated condenser, this being the essential feature on which the superiority of my process is based; adapting it at once to both high and low temperatures, and for the most delicate work.

The employment of bulbs, above referred to, as proposed by Wurtz, is simply a modification of the old process. The bulb apparatus furnishes the same, or, at most, but slightly better results than a simple retort; being no more than equivalent to increasing the height of the sides of the retort itself, without introducing any control over the accuracy of the results; the only advantage gained being, that these results are obtained somewhat more quickly.

In the new process, perfect control of the temperature of the vapors is secured by simply conducting these vapors upward through a worm contained in a bath, *aa*, figs. 1 and 2, the temperature of which is regulated by means of a separate lamp, *b*, fig. 2, or by a safety-furnace, *p*, as shown in fig. 1. The bath may be of oil or water, or of metal for very high temperatures, as the case may require, and is furnished with a thermometer, *t*.

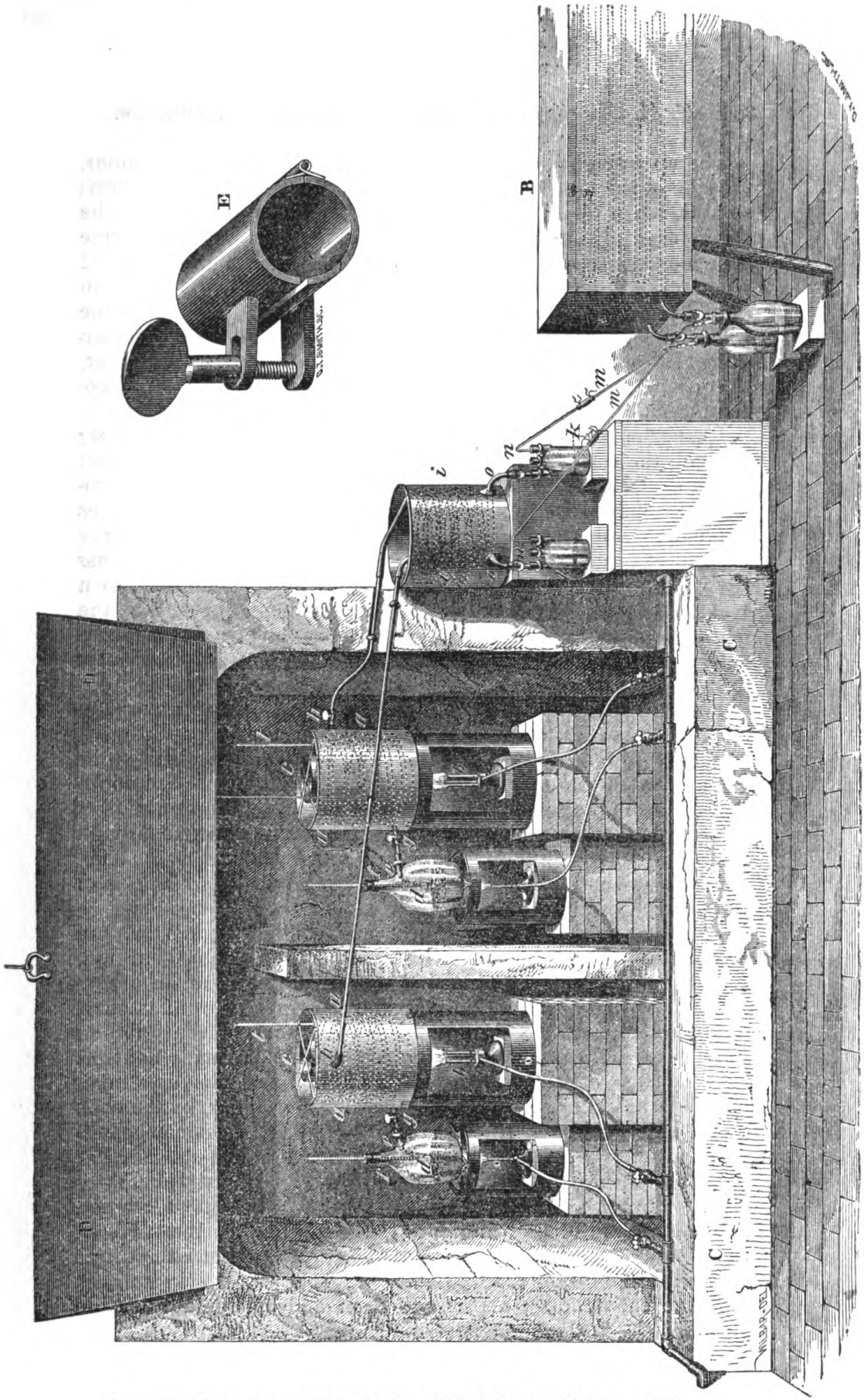
1.



That this bath may be equally adapted for the separation of liquids boiling below the common temperature, an empty vessel, *c*, figs. 1 and 2, is permanently secured in the interior of the bath by means of straps of metal across the top, to serve as a convenient receptacle for ice or iced water, by means of which a low temperature may be steadily maintained. The interior vessel also serves a good purpose in economizing time, and fuel in heating the bath, as it diminishes the quantity of oil required to cover the worm. It is made to extend to within about three inches of the bottom of the bath, and large enough to fill the greater part of the space in the center of the coil. The bath and interior vessel are both made of sheet-copper, with joints brazed so that they will bear a high temperature. I generally

use, also, copper worms, especially in the earlier distillations, the quantities then operated upon being larger, as such worms are conveniently procured, and not liable to break. In the larger-sized apparatus, the tube of which the worm is made measures ten feet in length and half an inch in diameter. I have tried several lengths of worm and several diameters of tube, but not, as yet, with any special view of determining the precise proportions, in relation to the size of the retort, which would be best adapted to the purpose. There appears, however, to be nothing gained by increasing the length of the worm beyond what is required to reduce the temperature of the vapors to that of the bath. I have in use three sizes of apparatus: the largest has a copper worm 10 feet long and $\frac{1}{2}$ inch bore; the medium size, a worm 5 feet long and $\frac{3}{8}$ inch bore; and the smallest size, for very small quantities, a worm 1 foot 6 inches long and $\frac{1}{4}$ inch bore. Each of these has been found to answer a good purpose. The distillation may be conducted in a glass flask, or more conveniently in a glass retort of the form shown at *d*, figs. 1 and 2. The body of this retort, as appears in the figure, is of the form of the corresponding part of the common retort; but which, in place of a long neck, has only a short tubulure, *e*, in the side, for escape of the vapors, and another tubulure, *f*, in the top, which contains the thermometer, and through which the retort is charged.

In the larger apparatus the retort is connected with the lower end of the elevated worm by means of a glass tube of about the same diameter as the end of the worm. One end of this tube enters the retort at the lateral tubulure through a perforated cork, and the other end is joined to the end of the worm, either by being firmly bound with a strip of cloth thickly covered with vulcanized caoutchouc,—such as is found in commerce,—or by means of a perforated cork, which is made to fit the ends of both tubes as snugly as possible, and then tightly pressed together upon the joint by means of an iron clamp, as shown at *g*, fig. 2. This clamp is figured on a larger scale at E. As it is highly important that all joints in the apparatus should be perfectly tight, inasmuch as the least leakage, when continued a long time, would cause, in the aggregate, a serious loss of material, I would call special attention to the clamp joint as the best which I have tried. Before falling upon this device I had used exclusively the vulcanized caoutchouc joints, which were found to answer a good purpose, in most cases, except that they required too frequent renewal. I have found the cloth covered with vulcanized caoutchouc preferable to the common caoutchouc tubing. In the smaller sizes of apparatus I have the end of the worm itself project far enough from the bath to connect directly with the retort by means of a perforated cork, without the use of an additional connecting tube.


E
B
i
n
k
m
m
C
D
WILBAR. DEL.

The upper end, *h*, of the elevated worm is brought out through the side of the bath at a point about three inches below the top; so that, when working with a low temperature of the bath, the worm may still be completely covered with oil, and also give sufficient space above the worm for the expansion of the oil when higher temperatures are employed. To avoid contaminating the atmosphere of the laboratory with the disagreeable fumes which are given off, in large quantity, from such a mass of heated oil, the top of the bath is tightly closed with a sheet-iron cover, from which a small funnel, A, fig. 1, conducts these fumes to a chimney.

In the larger apparatus, the vapors which succeed in passing through the heated worm are conducted downward into a cooled worm contained in a bath of water, *ii*, fig. 2, and the liquid product is collected in the receiver, *k*. The cold bath, *ii*, contains two condensing worms,—one for each apparatus,—and is large enough to condense for both without the necessity of renewing the water. I have represented two apparatuses combined, as it will be found more economical of time to operate with two at once. In the smaller apparatus, for the table, a Liebig condenser may be conveniently substituted for the cold worm, as shown in fig. 1.

For collecting liquids which boil below the common temperature, when such are present, I attach a refrigerator, B, fig. 2, which is provided with two block-tin condensing-tubes,—one for each apparatus. These are bent in a zigzag form, and attached to the inner sides of the refrigerator. The lower ends of the tubes extend through the end of the refrigerator far enough to form a convenient connection with the second receiver, *l*, fig. 2, which communicates with the first receiver, *k*, by means of the glass tube, *m*.

In order to successfully collect and condense the vapors of such extremely volatile liquids as are now under consideration, it is of course indispensable that the apparatus should be constructed with very tight joints; and for greater convenience, but more especially to prevent breakage, such of the joints as require to be frequently taken apart should be made flexible. A very convenient and perfectly tight joint of this kind may be made as follows:—the short stationary tube, *n*, in the cork of the receiver, *k*, fig. 2, is made with the opening somewhat divergent upward; the end, *o*, of the worm is enough smaller than the inside diameter of the upper end of the tube, *n*, to leave room for a piece of caoutchouc tube to be drawn over it, and still admit of its being inserted in the end of the tube, *n*; the flexible tube is drawn on far enough to prevent the drops which form on the end of the worm from coming in contact with the caoutchouc; a perfectly tight and convenient flexible

joint is now made by pressing the tube, *n*, over the caoutchouc covering of the end of the worm, *o*. The joints of the receivers, *ll*, are made in the same manner.

The vapors which escape condensation in *ii* pass through the receivers, *kk* and *ll*, to the refrigerator, B, which contains ice, or a mixture of ice and salt, are there condensed and fall back into the receivers, *ll*; which should stand in a wooden vessel also containing ice or a freezing mixture. The refrigerator, B, is made with double bottom and sides, with an inch space between, which is filled with pulverized charcoal. Being tightly covered, a charge of ice and salt will serve for a long day's operations without renewal. In this manner I have been able to collect, in considerable quantity, bodies boiling nearly at 0° C., and this from mixtures in which such bodies had been quite overlooked by previous investigators.

It will be observed, on reference to fig. 2, that the larger distilling apparatus is represented as standing in a brick fire-place, with brick-work, CC, a few inches high, built up in front; and a sheet-iron apron, DD, folded above. This is for security against fire in case of accident, either to the retort or hot bath of oil. As arranged, the contents of either or both of these could run out and burn without danger to the operator or the premises, as the brick-work in front would prevent the liquid from spreading beyond the fire-place, and the dropping of the sheet-iron apron would cause an additional draft, and thus insure the passage of the flames into the chimney. Instead of placing the apparatus in a fire-place, where that is not convenient, equal security against accidents may be attained by the use of my safety heating-lamp,[11] *q*, fig. 1, to heat the retort, and safety-furnace, *p*, containing a Bunsen's burner, for heating the bath. The bottom of this furnace, and also a large part of the sides, is formed of wire gauze, such as described for the safety-lamp.[12] The gauze upon the bottom need not be permanently attached to the furnace, but may be simply laid over an opening cut in the stool or board on which the furnace is to be placed; if the furnace be then set upon it, taking care that the joint shall be tight around the edge, nothing more will be required. A strip of vulcanized caoutchouc, about an eighth of an inch in thickness, is riveted around the edge of the opening for the door; against this the door tightly closes, so that no ignition can take place through the cracks which would otherwise remain under the edges of the door.

For an apparatus to stand upon the table, the safety-lamp and furnace are especially desirable. I have also used them for the larger apparatus, placed upon the floor of the laboratory. As

[11] This Journal, 1862, [2], xxxiii, 275. [12] Loc. cit.

a practical test of the security which they afford, I may relate an incident which happened to myself. I had left the laboratory for a short time, with such an apparatus in full operation; the retort containing nearly a quart of light petroleum boiling below 100° C. Having been detained longer than I expected, on returning I found the laboratory filled with the vapors of hydrocarbons; and, on approaching the retort, found that the caoutchouc joint, connecting the retort with the elevated worm, had failed, and that the larger portion of the liquid had distilled into the room, having been mainly condensed in the upper worm, and conducted thence down the outside of the retort into the safety-lamp. This process was still going on, the lamp being highly-heated from the excess of fuel thus added to it, but no ignition took place outside the lamp. Although this experiment was rather injudicious, it furnishes a valuable test of the efficiency of the safety-lamp and furnace.

Having described the apparatus, I now proceed to give such details of the method of conducting the separations as have been found, in my experience, most efficient and economical of time. In commencing with a crude mixture of unknown liquids, I deem it advisable to operate at once on a tolerably large quantity of material, especially if the constituents are supposed to be numerous, and to omit chemical treatment till after the separations have so far progressed as to indicate the number and species of bodies present, and, approximately, their several boiling-points.

Notwithstanding the precautions taken to avoid loss from evaporation and leakage, I have at times been surprised at the large waste of material which has been made apparent after a long series of operations. When it is considered, however, that the time required to make a complete separation of a very complex mixture of liquids must necessarily be very protracted, during which more or less of evaporation is constantly taking place, it will be a matter of no surprise that the loss is so considerable. The quantity of material required must depend also on the proportions in which the various constituents are contained in the crude mixture, and upon their degree of volatility; but as these cannot be known *a priori*, it may suffice to make a single preliminary distillation of a portion of the mixture, from a tubulated retort, to ascertain the range of temperature within which it distills, noting at the same time the proportions which come over between certain temperatures; as, for example, below 50° C.; between 50° and 100°, etc.; from these data one may judge pretty nearly of the quantity which it will be advisable to take. It is evident that, when very volatile bodies are present, even in considerable proportion, a much larger quantity

would be required than if the material were but slightly volatile; as the waste in the former case, from evaporation, would be much greater.

But in many cases it will be found that highly volatile bodies are present only in very small proportion,—e. g., in viscid petroleum like Rangoon tar, and in the products of distillation of some species of asphalt. In such cases, the requisite quantity to be operated upon, to obtain the most volatile constituents in sufficient quantity for anything like a complete study of their chemical relations, would be extremely large,—too large to be conducted in the laboratory,—and one would have to resort to the manufactory for the first distillation. I have dwelt at some length on this point, having experienced the disappointment which one feels, after months of labor, on finding the products insufficient for his requirements, when the expenditure of a little more time, comparatively, might have given double the quantities obtained.

In the first series of fractioning, I generally operate on successive portions, of about one gallon each, of the crude material, and take off a fraction for every 20° C. rise of temperature of the retort. These fractions are preserved in well-stoppered bottles, and each carefully labelled with the temperatures between which it was obtained. The fractions for each fresh portion of the crude material, being collected between the same limits of temperature, are added to the corresponding products from the preceding operations, till enough of the crude material has been taken to insure, ultimately, a sufficiency of the pure products.

In the commencement, not only of this but of all subsequent fractionings, when the temperature to which the bath should be raised is unknown, I first bring the liquid in the retort into full ebullition, so that a steady stream of liquid shall flow back from the end of the worm into the retort. I then carefully raise the temperature of the bath until the vapors from the retort pass through the heated worm so freely that the liquid, in condensing from them, shall drop with tolerable rapidity into the cold receiver. In order that this dropping may be continuous, it is necessary that the temperature of the bath should rise *very gradually* as the more volatile constituents of the mixture are taken off; this is easily effected by carefully regulating the flame under the bath.

It is advisable to boil the retort as rapidly as possible without choking the lower end of the heated worm with the returning liquid. As this choking would give rise to additional pressure in the retort, and consequently occasion abnormal elevation of the temperature, and possibly a rush of liquid into the receiver, and thus introduce irregularities in the work, excessive heat under the retort should be avoided. The first indication of

choking of the worm is a partial or entire stoppage of the stream of liquid which normally flows steadily from the end of the worm into the retort. Any interruption or unsteadiness of this flow would indicate too rapid ebullition.

As a rule, other things being equal, the greater the difference between the temperature of the bath and that of the retort, the slower the products will come off, and the more effectual will be the separation. I think it possible, however, that the earlier fractionings may be conducted so slowly that the loss of time would more than counterbalance what might be gained by more thorough separation, and that equally good results may be more economically obtained by more frequent operations, somewhat more rapidly conducted.

A striking illustration of the advantage to be gained by this process is presented by the fact that, during the first fractioning of a crude mixture, such as American petroleum or coal-tar naphtha, for example, the difference between the temperature of the bath and that of the retort may sometimes be as much as 35° C., or even more. While, as the products become purer, this difference between the temperatures of the bath and retort proportionally decreases, till finally, in operating on a pure product, the temperature of the bath must be brought to within a few degrees of that of the retort, in order to bring the vapors through. But the amount of this difference is variable for different bodies of equal purity.

The first fractionings must necessarily be quite arbitrary; for, as a general rule, when operating on such mixtures as those just mentioned, neither the thermometer nor the quantities obtained for any given range of temperature will indicate any decided preponderance of any one substance. On the contrary, the temperature rises uniformly, and about the same quantity is generally obtained for the same number of degrees of temperature throughout the operation. In other mixtures, in which certain bodies may seem to be present in much larger proportion than others, or in which there may be a greater difference between the boiling-points of the constituents than in the cases referred to,—facts which would be indicated by the thermometer of the retort, and by the relative quantities of the products obtained,—there might be something gained by exercising discretion in taking off fractions according to these indications.

In the second series of fractioning, the first or lowest fraction of the preceding series, which is large enough to operate upon by itself, is transferred to the retort, and brought into ebullition. The temperature of the bath is then adjusted as above described, and the distillation continued, the fractions obtained being placed in their appropriate bottles until the temperature of the retort shall have risen to, or somewhat above, the point at which the

second or next succeeding fraction of the first series may be supposed, or has been found by experiment, to boil. This fraction is then added to the residue in the retort, and the distillation is continued as before. In the same manner, I proceed with the remaining fractions of the first series.

All subsequent fractionings are similarly conducted. As the work progresses, however, the fractions are taken for a gradually decreasing number of degrees of temperature, until finally it becomes necessary, for the attainment of absolute constancy of boiling-point, to take off a fraction of every degree, centigrade; and to continue thus to operate on these fractions, each representing one degree of temperature, until the desired end is attained.

The operator will observe that, in each series of fractions, in which each fraction has been taken for the same range of temperature, the difference between the boiling-points of any two contiguous fractions is nearly the same as the difference between any other two contiguous fractions,—in other words, that the difference referred to approximates to a common difference throughout the same series. Once ascertained, this difference serves as a valuable guide in determining with sufficient accuracy when to add the next fraction to the retort. By observing this systematic course, irregularities, from the improper mixture of products, may be avoided, and time thus economized.

After a few series of fractionings,—sometimes after two or three, variable in number, according to the nature or complication of the mixture,—it will be found that some of the fractions are considerably larger than others for the same range of temperature, indicating approximately the boiling-points of the several constituents. But fractions of constant boiling-point, or those, the boiling-points of which cannot be sensibly changed by further fractional condensation, are not obtained, as already mentioned, till after repeated careful fractioning of every degree of temperature. When fractioning of every degree, it is important to use every precaution to protect the thermometer from external influences, and to carefully apply the corrections for variations in the atmospheric pressure. This may even be desirable earlier; but it is of so much importance in the case specified, that, if omitted, the operator would be liable one day to mix products which he had separated the day previous.

In this way, certain larger fractions are obtained, which are not susceptible of further alteration in their boiling-points; but there are yet considerable quantities of liquid in the intermediate fractions, which still continue to change more or less in each succeeding operation. When the fractions of constant boiling-point have once been obtained, if it were not important to test for other bodies in the intermediate fractions, the operation

might here be suspended, provided the pure products already obtained should be large enough for the purposes required.

But, in my investigations, I have undertaken to prove the negative as well as the positive. I have attempted to carry the process of separation so far, that I might assert the absence of other bodies, as well as the presence of those obtained; and this clearing up of the intermediate fractions has generally been the most tedious part of the work. I have continued to operate upon these by themselves, until they also have become distributed in regular course—no new bodies appearing—among the fractions of constant boiling point, or to such an extent that the intermediate quantities have become too small to admit of further continuance of the process.

This process has been in constant use in my laboratory during the last three years. In this time it has been applied in the study of petroleums, coal oils, the more volatile parts of coal- and wood-tars, the essential oil of cumin, commercial fusel oil, from corn whiskey, and even to mixtures more complex than either of these. As the result of this long experience, I can say that, as regards bodies not decomposed by heat in distillation, I have not yet found a mixture so complex that it may not be resolved by this process into its proximate constituents so completely, that these shall have almost absolutely constant boiling-points. In repeated instances, even from petroleums, I have obtained these constituents so pure, that the contents of an ordinary tubulated retort charged with one of them has been completely distilled off without any essential change of temperature; i. e., not to the amount of $\frac{1}{2}^{\circ}$ C., the thermometer frequently remaining absolutely constant for more than half an hour, a constancy of boiling-point not exceeded by that of distilled water. This state of purity, I think I may safely assert, has never before been attained from such mixtures, by any system of fractional distillation.

As I shall soon be prepared to present to the Academy detailed results of the investigations above referred to, I may omit further allusion to them on this occasion.

I would remark, in conclusion, that it seems to me not improbable that this process may ultimately prove to be of great value in the arts. It is not too much to anticipate that, whenever the various constituents of the mixtures referred to shall have been separately and thoroughly studied in a pure state, some of them may be found to possess properties which will give to them great commercial value, sufficient to justify the expenditure necessary to separate them in large quantities.

ART. XLII.—*Examination of Petroleum from California;* by B. SILLIMAN.

THE specimen of petroleum here described came from a natural well or spring on the Simi estate in Santa Barbara county, California. It issues from rocks of the Tertiary Age. The discharge from this spring is sufficient to flow nearly two miles down the dry bed of the ravine in which it occurs.

It is of a dark brown color, dichroic, thin and mobile as water, and of a faint naphtha odor, quite without offensive smell. In a thin tube it has a yellowish brown color by transmitted light, and obviously owes its dark color to its holding in solution a portion of the asphaltum with which it is associated.

Its density is 0·861 at 15° C. or about 34·30° B. It burns, in its crude state, in a double current lamp, without smoke, with quite a bright flame and strong light for a few moments. After eight or ten minutes the wick commences to coal, and after about fifteen minutes it smokes and finally dies out.

1000 c. c. of the crude substance were subjected to fractional distillation in a tubulated retort to which was adapted a thermometer and condenser. A condensible vapor appeared at 60° C., the liquid simmered at 90° and boiled at 123°.

Of the 1000 c. c. of crude oil there were distilled from glass up to the boiling point of

Mercury, - - - - - - -	500 c. c.
In iron, - - - - - - -	496 "
Loss and coke, - - - - -	4 "
	1000 "

This method of practical distillation gives, as is well known, very unsatisfactory results, as compared with the method of Warren.[1] As the experiment was, however, very carefully conducted by Mr. Peter Collier, of the Sheffield Laboratory, under my directions, it furnishes a good illustration of the differences arising solely out of an imperfect method of analysis, when practiced on the same raw material. I therefore append the results in detail.

The temperatures were noted by a mercurial thermometer the bulb of which was continually in the boiling liquid.

20	c. c.	had	distilled	at	153°	C.	260	c. c.	had distilled at	234° C.
40	"	"	"	"	170	"	300	"	" " "	250 "
100	"	"	"	"	177	"	360	"	" " "	266 "
140	"	"	"	"	190	"	400	"	" " "	280 "
160	"	"	"	"	200	"	450	"	" " "	297 "
200	"	"	"	"	213	"	485	"	" " "	320 "
240	"	"	"	"	223	"	500	"	" " "	370 "

[1] See this vol., p. 327.

When about 485 c. c. had passed over, the temperature rose very suddenly from 320° to 370° C.

The remaining 460 c. c. were distilled without noting temperature, in an iron retort, leaving a light carbonaceous residue.

The successive fractions gave the following sp. gr. and explosive tests, the latter being the commercial standard as marking the temperature at which the liquid exhales an inflammable vapor.

	Sp. gr.	Beaumé.	Expl. test.
1st, 100 c. c.	755	55·77	58° F.
2nd, 100 "	775	51·80	70° "
3rd, 100 "	793	47·62	100° "
4th, 100 "	815	42·70	156° "
5th, 100 "	838	37·83	196° "
6th, 100 "	867	32·08	190° "
7th, 50 "	872	31·13	190° "
8th, 100 "	890	27·79	181° "
9th, 100 "	900	26·00	171° "
10th, 100 "	900	26·00	140° "

The color of these products varies from Nos. 1 and 2, which are quite colorless, through 3 to 7, which, commencing in 3 as a faint pink, become red in 6 and 7, while 8, 9 and 10 can hardly be distinguished in color from much of the better quality of crude oil flowing from the best wells, having the same change of color (dichroism) by reflected and transmitted light. The unrectified products would arrange themselves thus by color—

I.	1st,	2nd,	3rd,		fractions.
II.	4th,	5th,	6th,	7th,	"
III.	8th,	9th,	10th,		"

But in the actual conduct of the manufacture of this oil, I presume the distiller would make three divisions:

1st,	Light oil, including the			1st	fractions, about	53·5°	commercial.
2d,	Burning oil, "	" 3	to	7	"	"	38·5°	"
3d,	Lubricating oil,	" 8	to	10	"	"	28·0°	"

By mingling a portion of the light oil with the heavier, it is probable that a product of about 60 per cent would be obtained suitable for illumination.

The light oil and the burning oil are perfectly decolorized by the usual treatment with sulphuric acid and carbonate of soda. The light oil has a highly agreeable smell, and none of the other products possess any of the heavy and disagreeable odors common in the petroleum products. The burning oil has a very high illuminating power, and fails to char or encrust the lamp-wick after an hour's burning. The explosive test is 155° F.

The densities of various specimens of crude California petroleum which I have determined range from 0·859 to 0·972, the

difference being apparently due to the greater or less degree of evaporation and oxydation of the products consequent on exposure to the warm air of a very dry climate. The specimens thus far examined from California are all from surface springs. When the oil is drawn from artesian borings the same change of density will undoubtedly be observed as in Pennsylvania. I found the surface oil of Oil Creek, in 1855, having a density of ·880 to ·885, thick in warm weather as thin molasses, and in cold weather quite viscid. The flow of wells in the same region is now quite thin, and has a density of ·800 to ·850.

Appended are the results of Mr. Warren's examination of the same California oil now under consideration.

"To Prof. SILLIMAN, New Haven.

"*Dear Sir*—The results of my examination of the crude California petroleum which you sent me through Messrs. Spear, Burke & Co., of this city, and which bore the seal of Messrs. Wyeth & Bro., are as follows:

"Specific gravity at 17 c. 0·864=33° Beaumé.

1250 c. c. of the crude substance subjected to my process of fractional condensation, gave—

41 c. c.	of light oil	between 93° and 100° C.	Specific gravity not taken.[2]
43 "	" " "	100° and 140° C.	
525 "	Burning oil "	140° and 310° C.	sp. gr.=44° B.

From this point the distillation was conducted in the ordinary manner from a common retort, no thermometer being used.

270 "	lubricating oil	of sp. gr.	29½° Beaumé.
293 "	" "	"	28° "

1172 c. c. = 93·8 per cent of total product.

"The residue left in the retort was dry coke. After treatment with sulphuric acid and alkali the light oil and the burning oil were nearly or quite colorless. The lubricating oil had a yellowish color. The odor of the burning oil was extremely agreeable, fully equal in this respect to the best kerosene or refined Pennsylvania petroleum. The other products were also entirely free from disagreeable odor, and indeed the same may be said of the crude oil itself. In this respect it is readily distinguishable from the Pennsylvania petroleum.

"The burning properties of the illuminating oil are not surpassed by any oil which I have seen.

"What I have called *light oil* is not very volatile, and would not, I think, rank in the market as naphtha, at least not that taken between 100° and 140° C.; and it is my opinion that the burning oil would take the whole of the light oil, and still bear the commercial fire test. In that case some of the lighter of the lubricating oil probably might be run into the burning oil, so that the yield of the latter would be over 50 per cent."

Boston, March 31, 1865."

[2] The specific gravity of the mixture determined by me is 0·753=58° Beaumé.—S.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the determination of lime.*—FRITZSCHE has made the very simple yet important discovery that oxalate of lime may be easily and completely converted into caustic lime free from every trace of carbonate by simple ignition over a blast lamp. The tedious processes recommended in works on analytical chemistry, such as ignition with carbonate of ammonia, conversion into sulphate of lime, etc., become unnecessary, and will hardly be employed in future. The author used in his experiments a platinum crucible 33mm. in height and 27mm. in width at the top: the ignition took place over a gas lamp consisting of six Bunsen's burners, (the ordinary Bunsen's blast lamp would doubtless answer even better). In four experiments Fritzsche obtained from known weights of oxalate of lime 99·9, 100·08, 100·08 and 99·98 per cent of the calculated quantity of lime. In all these experiments the ignition was not continued longer than 15 minutes. In a large platinum crucible, quantities of the oxalate not exceeding one gram could be rendered perfectly caustic in 15 minutes, but 30 minutes were necessary for a quantity of more than 3 grams. Precipitated carbonate of lime was also completely converted into caustic lime by ignition, and the method, as giving results which are more accurate and more easily obtained than those of the older processes, will doubtless come at once into general use.—*Journal für prakt. Chemie*, xciii, 335. W. G.

2. *On the sulphur compounds of uranium.*—REMELÉ has found that the brown precipitate which sulphid of ammonium produces in a solution of nitrate of uranium is neither sulphid of uranium nor a mixture of protoxyd of uranium and sulphur as was formerly supposed. The precipitate changes rapidly when formed in an aqueous solution and becomes orange and then yellow. When formed in an alcoholic solution, the precipitate is permanent and has the formula U_2O_2S, so that it is analogous to the well known chlorid U_2O_2Cl. By boiling in presence of sulphid of ammonium the compound is easily decomposed into sulphur and hydrate of protoxyd of uranium.—*Ann. der Chemie und Pharmacie*, III *Supplementband*, p. 196. W. G.

3. *Note on the regenerative gas-furnaces of the brothers Siemens.*—The regenerative gas-furnaces of the Messrs. Siemens are so interesting and suggestive that we make no apology for again calling attention to them, supposing of course that their general features are sufficiently familiar from the description given by Dr. Faraday, and quoted in the last number of this Journal. Two points which appear to us of special importance are, first, the extremely high temperature which can be obtained and which in fact is limited only by the nature of the materials employed in the construction of the furnace, and secondly, the possibility of employing at will either an oxydizing or a reducing atmosphere. The brothers Siemens have already applied their furnaces to puddling and reheating, but we venture to predict for them a very extensive use in metallurgical processes. Thus, it is well worth while to determine by direct experiment

upon a large scale whether the rich iron ores of Lake Champlain, Lake Superior and Missouri, cannot be directly reduced to the metallic state by heating them to a sufficiently high temperature in the chamber of a Siemens furnace, and then changing the gaseous mixture in the furnace to a reducing condition. This would in fact be blooming upon a large scale, and would not improbably avoid the inconvenience and expense of blooming in the small way, which in spite of the superior quality of the iron produced has been almost wholly superseded by the cheaper process of puddling. Experiment only can determine whether fluxes can be used with advantage in blooming in this manner, when poorer ores are employed. Ores of copper could doubtless be roasted and reduced in furnaces of this construction, and with some additions to the original plan, the sulphurous acid formed during the roasting might be directly converted into sulphuric acid in leaden chambers. But it is for the metallurgy of iron that the new furnaces will probably be found most advantageous. As the temperature attainable is extremely high, it may even be found practicable to melt the malleable iron formed by the direct reduction of the ore, the walls of the fire-chamber being lined with lime as more refractory than any fire-clay. But even if this rather bold suggestion should not be realized, it is at least probable that the earthy impurities of the ore would be reduced to a peculiarly fluid condition, so that the blooms could be easily treated under the hammer and brought into the form of malleable iron. For the rest, there is hardly a branch of manufacture in which heat is employed upon the large scale in which furnaces on the regenerative principle would not find an application. We suggest that small gas furnaces could be made upon the same principle for laboratory use and for various processes in the arts, using the ordinary city gas, however, as fuel instead of gas produced by a special furnace. The high temperature obtained in Gore's gas-furnaces appears to be due to the heating of the air and gas before they mix in combustion. W. G.

4. *On some aluminum compounds.*—Buckton and Odling have studied the action of aluminum upon ethyl-mercury and methyl-mercury, and have thus obtained ethyl-aluminum and methyl-aluminum without difficulty and in considerable quantity. Ethyl-mercury, or, as the authors term it, mercuric ethid, with an excess of clippings of aluminum, was heated for some hours in sealed tubes in a water bath, when the mercury was found to be completely displaced by the aluminum. After distillation the aluminic ethid was obtained as a colorless mobile liquid which boiled at 194° C. and did not solidify at −18° C. It evolved dense white fumes on exposure to the air, and when in thin layers took fire spontaneously burning with a bluish-red edged flame, and producing an abundant smoke of alumina. Analysis gave nearly the formula $Al_2(C_4H_5)_3$ or $Al_4(C_4H_5)_6$. The vapor-density at 234° C. was found to be 4·5, the theoretical number for $Al_2(C_4H_5)_3$ being 3·9, whence it would appear that the true molecular formula is $Al_2(C_4H_5)_3$ and not $Al_4(C_4H_5)_6$. Water decomposes aluminic ethid with explosive violence. Iodine forms iodo-derivatives and iodid of ethyl. Oxygen gives a body apparently analogous to boric di-oxyethid. Aluminic methid was obtained as a colorless mobile liquid boiling at 130° C. and solidifying a little above 0° to a beautiful transparent crystalline mass. The liquid takes

fire spontaneously in the air. The formula of this body was found to be $Al_2(C_2H_3)_3$, with which the vapor-density 2·80 closely agrees, the theoretical number being 2·5. But the vapor-density was found to increase very rapidly as the temperature diminished, so that it is probable that at a temperature not far removed from the boiling point the molecular formula would be $Al_4(C_2H_3)_6$. The authors ask whether the observed vapor-density of aluminic chlorid may not correspond to the high density of aluminic methid, so that both may be anomalous and untrustworthy for determining the general formulæ of aluminum compounds. —*Chemical News*, No. 271, p. 61.

5. *On the density of the vapor of salammoniac.*—H. St. Claire Deville has repeated his important experiments on the density of the vapor of sal-ammoniac, and has replied to the objections made by various chemists to the conclusions drawn from his first researches. The author shows conclusively, by an ingenious piece of apparatus, that chlorhydric acid and ammonia, each heated to a temperature of 360° C., combine with a sensible evolution of heat, so that at this temperature the vapor the density of which is taken cannot be regarded as a mixture of HCl and NH_3, but must be admitted to be NH_4Cl, the vapor-density of which corresponds to 8 volumes (H=2) and not to 4 volumes as in the case of other organic compounds. Deville suggests that the proof that no simple or compound substance corresponds for one equivalent to 1 or to 8 volumes rests with those chemists who make this assertion, and that until that proof is given the assertion amounts to a pure hypothesis. Among other difficulties, the following is suggested to the partisans of the so-called anomalous densities.

(1.) Sulphydric acid forms with ammonia two distinct crystalline and volatile compounds, the formulæ of which are respectively NH_4S, and NH_4S, HS. Sulphid of ammonium represents 4 volumes of vapor; its condensation is $\frac{1}{3}$: sulphydric acid and ammonia combine and remain combined at the temperature (for example 100° C.) at which we determine the density of the vapor. Sulphydrate of sulphid of ammonium represents 8 volumes of vapor; its condensation is nothing. If we suppose its elements separated at the temperature at which we take the vapor-density (100° for example), we must admit that it separates into ammonia and sulphydric acid, NH_3 and 2HS, each giving four volumes and having 8 volumes as their sum. Now at this temperature the elements can only separate into sulphid of ammonium and sulphydric acid, giving the one 4 volumes and the other 2 volumes, the sum of which is 6. Hence, if the sulphydrate were decomposed at the temperatnre at which the vapor-density is taken, it ought to furnish 6 volumes, and as it furnishes in reality 8 volumes, it follows that it is not decomposed.

(2.) In the case of bodies, simple and compound, which present vapor-densities variable with the temperature, it cannot be said that these densities would become normal if the temperatures could be sufficiently increased, because, as Deville and Troost have shewn, as the temperatures increase the vapor-densities diminish toward a constant limit, so that the coëfficients of expansion must also become constant and differ insensibly from 0·00367. To admit that the coëfficient of expansion of arsenic and phosphorus vapors may be different from this number, in order to show

that these vapors do not correspond to one volume (H=2) is to make an assumption which is inadmissible in the present state of science.—*Comptes Rendus*, lix, 1057.

6. *On the dispersion of light produced by the rotation of the plane of polarization in quartz.*—STEFAN has given a beautiful method of forming a spectrum by means of the dispersion of the planes of polarization produced by plates of quartz. A bundle of parallel plane-polarized rays of white light is allowed to fall upon a glass cone, the sides of which make with the axis an angle equal to the angle of polarization. The reflected rays are received upon a screen perpendicular to the axis of the cone and produce a circular sector in which the maximum intensity of light corresponds to the rays, the plane of reflection of which is parallel to the plane of polarization. If we introduce a plate of quartz cut perpendicular to the axis into the path of the bundle of incident parallel rays, the position of the maximum is different for differently colored rays, and the sector then exhibits the different colors of the spectrum. In this manner the dispersion of the planes of polarization is rendered visible. In the same manner, if we receive the polarized bundle of rays transmitted by a plate of quartz upon a plate of spar cut perpendicular to the axis which gives in the analyzer a system of rings traversed by a black cross, each of the branches of this cross will be replaced by a sort of fan exhibiting the different colors of the spectrum.

Measurements the details of which are not yet published have led Stefan to represent the rotations due to a plate of quartz 1^{mm}. in thickness by the formula $$\varrho = \frac{8{\cdot}1088}{\lambda^2} - 1{\cdot}697$$ in which the wave length λ is expressed in thousandths of a millimeter. From this formula we should deduce the remarkable consequence that the rotation would be zero for rays of a wave length equal to $0{\cdot}002186^{mm}$, and would change its sign when this limit is exceeded.—*Ann. de Chimie et de Physique*, [4], iii, 501. W. G.

7. *On combustion by invisible rays.*—At the Newcastle meeting of the British Association in 1863, Dr. C. R. AKIN proposed three experiments by means of which, if successful, rays of heat could be converted into rays of light. One of these experiments consisted in converging the sun's rays by means of a concave mirror, then cutting off the light by "proper absorbents," and then igniting platinum foil in the focus of the invisible rays. Want of means appears to have prevented the execution of the proposed experiments; but Dr. Tyndall, who had devised experiments of a similar character, and who had discovered a liquid possessing precisely the required properties, has recently succeeded in the conversion of rays of heat into rays of light in a very striking and instructive manner. Tyndall employed an electric lamp of Duboscq and a linear thermo-electric pile, the spectrum being formed by lenses and prisms of rock salt. As in the case of the solar spectrum, the heat was found to increase in intensity from the violet to the red, rising to a maximum beyond the red at a distance about equal to the distance of the green on the more refrangible side. The curve of intensities rises beyond the red into a steep and massive peak, quite dwarfing the radiation of the luminous part of the spectrum. Water interposed cuts away the peak in part; the vapor

of water, according to Tyndall, would do the same, and this explains the difference between the heat-spectra of the electric and solar light, the peak being much higher relatively in the former than in the latter case. After several trials the author found that a solution of iodine in bisulphid of carbon, so opake as to cut off the light of the midday sun, was, within the limits of experiment, absolutely transparent to invisible radiant heat. When the rays from the electric lamp were concentrated into a cone by means of a small glass mirror silvered in front, a cell containing the solution of iodine absorbed the light completely, while the invisible rays of heat being brought to a focus set fire to paper, wood, charcoal and other substances. With a sufficient battery power a plate of platinized platinum is rendered white-hot at the focus of the invisible rays, and then yields a complete and brilliant spectrum. The image of the carbon points of the electric lamp formed upon the platinum plate becomes visible as the plate becomes red-hot, and a species of thermograph is obtained, as is well shown by drawing the points apart or causing them to approach each other, the red-hot image following the motion. In this case the invisible rays of heat have their refrangibility raised in becoming visible, while in the case of fluorescence the invisible rays of light have their refrangibility lowered. Tyndall proposes to apply the term *calorescence* to the phenomena of heat in question, a term which is certainly preferable to the word "calcescence" employed by Akin. We regret to see that a bitter personal controversy has been carried on in a prominent English scientific journal between Dr. Tyndall and Dr. Akin as to the proprietary rights of each in the beautiful experiment above described. Into the merits of this controversy we cannot enter; we may, however, be allowed to express the opinion that the experiment in question does not involve much originality of conception, but that it is a very simple and natural deduction from the experiments and results of Melloni upon smoked rock salt and upon the production of light without sensible heat. —*L. & E. Phil. Mag.*, March, 1865, p. 241. W. G.

8. *On the Electrical properties of Pyroxiline-paper and Gun-cotton;* by Prof. J. JOHNSTON. (In a letter to Prof. SILLIMAN, dated Middletown, Conn., January 25, 1865.)—You did me the honor, a year ago, to publish in the Journal a note of mine on the electrical properties of *pyroxiline paper* and *gun-cotton*, adding an approving note of your own. Having lately had opportunity to make some further experiments with the same substances, I herewith send you the results.

It is proper to say that both the paper and the cotton were prepared nearly a year ago, and may have undergone some change, though nothing of the kind was apparent to the eye, except that one sheet of the paper used seemed at one place a little discolored.

The substances experimented with were *amber*, *sealing wax*, *sulphur*, *gum lac*, *pitch*, *rosin*, *caoutchouc* (native rubber), *hard rubber* (stick obtained of Messrs. J. F. Luhme & Co. of New York), *common vulcanized rubber* (as used in forming gas bags), *gutta percha*, and various crystallized mineral substances. But these last, becoming always positive, will not be further alluded to. The sulphur by friction with the gun-cotton always became positive, and also by friction with different sheets of the paper, except in a single instance, when using the paper which was

slightly discolored, it appeared to be feebly negative. Rosin, pitch, gum lac, and amber, both with the paper and the cotton, became always positive, as did also the native rubber, by which I mean the rubber as it is imported. Sealing-wax with the cotton became always positive, but with the paper occasionally negative. Vulcanized rubber (the kind used in making gas bags) would sometimes become positive and sometimes negative, and the same was true of gutta percha, two different specimens being used. The hard rubber (from Luhme & Co.) became always negative, both with the paper and the cotton. This being contrary to the results obtained by yourself with this substance, I made very many trials, but always with the same result.

In all cases after friction with other substances, whether the latter became positive or negative, the paper and cotton would be found invariably negative. Sheets of the paper, when handled, especially in cold, dry weather, often become highly excited,—always negatively, so far as has been determined; and my son informs me that sometimes, when handling considerable quantities of the recently prepared paper, he was even fearful that it might become ignited by the sparks produced!

I will just remark in closing, that in making experiments like these, great care is required in order that the results arrived at may be satisfactory. This is particularly the case when it is necessary to rub a substance, as a roll of sulphur or a stick of sealing-wax, successively with different substances, some of which give the positive and others the negative electricity. Occasionally it will be found that the substance will be positive at some points and negative at others; and in such cases the only way is to lay the particular specimens aside until they shall have returned to their natural condition at all points. Sometimes a substance when first rubbed, after having remained undisturbed twenty-four hours or more, will take on one electricity, but, by continuing the friction a very little time, it will take on the other. Thus, a stick of sealing-wax in its natural state, when gently rubbed, *one* or *two strokes*, with a silk handkerchief, will often be found decidedly positive, but by a few strokes more it will become as decidedly negative; and it cannot be made positive again by friction with silk until allowed by repose first to return to its natural state.

II. MINERALOGY AND GEOLOGY.

1. *On Tin Ore at Durango in Mexico;* by Prof. C. F. CHANDLER.—I have recently examined a sample of 1450 grams of tin ore from Durango, and find it to be a handsome "wood tin" in pebbles and fibrous crusts, some of which are an inch in their longest diameter. The color varies from a very light brown to black.

Associated with the cassiterite, there are brilliant crystals of topaz, some of which are half an inch long. They vary from transparent to opaque, and from colorless to deep brown. I have not been able to examine them very closely, but have noticed the planes O, I, $i\bar{2}$, and 2. Owing to the development of the planes $i\bar{2}$ and 2, and their rich brown color, some of the crystals might easily be mistaken for cassiterite.

The sample examined gave—

Tin (by crucible assay),	50·90
Topaz crystals easily separated by the process,	3·10
Other topaz crystals, too small to be easily separated,	1·00 (?)
Oxygen and impurities (by difference),	45·00
	100·00

The material used for the assay was very carefully averaged, by pulverizing about 500 grams of the ore, after the topaz crystals had been separated. A correction was subsequently made in the result of the assay corresponding to the quantity of topaz removed.

School of Mines, Columbia College, N. Y., Dec. 23.

2. *Note by F. B. Meek and A. H. Worthen in relation to a genus of Crinoids from the Coal-measures of Illinois and Nebraska, proposed by them on p.* 174 *of the last number of this Journal.*—At the time we proposed the name *Erisocrinus* for this genus, we were not aware that M. Koninck had described it under the name *Philocrinus* in 1863, among some Carboniferous fossils from India (see *Memoire sur Foss. Palæozoiques Recueillis dans L'Inde*, p. 21. *Liege*, 1863). Owing to the fact that Mr. Koninck's figure, (probably from the wearing of the stone from which the impressions were taken,) does not show the basal pieces, nor distinctly their connection with the subradials, we had overlooked the generic identity of the American and Indian species. On comparison with his description, however, it becomes evident that our Crinoid agrees exactly in its generic characters with his. This being the case, we cheerfully withdraw our name, and arrange our species, which differ specifically from that described by him, under his name. In doing this it also becomes necessary to change one of our specific names (*E. typus*), since it is not the typical species of Koninck's genus. Hence our species may be called *Philocrinus pelvis* and *P. Nebrascensis.*

It is worthy of note that Mr. Koninck's figure of the arms and secondary radial pieces of this type, together with its other characters, show it to be closely allied to the still older genus *Bursacrinus*, Meek & Worthen, 1861, which only differs in the possession of an anal piece, and the consequent truncation of one of the subradials.

3. *On the Oil Formation in Michigan and elsewhere;* by Prof. A. WINCHELL, of the Michigan University. From a report on the "Baker Tract," near Lakeport, St. Clair Co., Michigan. 8 pp. 8vo. Detroit, 1864. —In most of the Northwestern States occurs a formation which has been known among American geologists as the *Black Slate* or *Black Shale.* It is well known in Ohio, Indiana, and Kentucky, and extends also into the surrounding States. It was formerly supposed by Prof. Hall, to be the western representative of the "Marcellus Shale" of New York; but the researches of western geologists tend rather to prove its equivalency with the "Genesee Shale" of New York. The color of the rock is due to the presence of a large amount of carbonaceous or bituminous matter, believed to be derived from the vegetation of the period in which the shales were deposited.

Similar black shales were brought to light by Dr. Houghton, many years ago, in the lower peninsula of Michigan; and the opinion was expressed by him and by Mr. Hubbard, his assistant, that these shales were

of the same geological age as the black slate of Ohio and Indiana. The same formation was subsequently described by Mr. Murray, of the Canada geological survey, as making its appearance at various points in Canada West; and the geologists of that survey speak of the formation as constituting the principal part of the "Hamilton group" as known in Canada. In the prosecution of the geological survey of Michigan, I enjoyed the opportunity to establish, by direct comparisons, the identity between the Black Slate of Michigan and that of Ohio and Indiana, and, by proving that it lies stratigraphically above the limestones of the Hamilton group, have established its distinction from the Marcellus shale, and co-ordinated it with the Genesee shale of New York. It is locally known as the *Huron Group.*

The geographical distribution of this formation in Michigan, will be seen by a reference to my Report for 1860. The strata along the eastern slope of the lower peninsula of Michigan dip westward beneath the Carboniferous rocks of the interior. This dip, however, is extremely slight, and the Huron group does not terminate at the waters of Lake Huron and the river St. Clair. It prolongs itself eastward, and overlaps the angle of Canada West, finally thinning out along a line extending from the mouth of Maitland river to a point on the Great Western Railway, a few miles east of Bothwell.

The lithological characters of the formation are but little varied. Throughout its entire thicknesss of 600 or 700 feet, it is argillaceous and generally bituminous. The most bituminous and darkest colored portions seem to be situated near the bottom of the group. We know, at least, that they are not near the upper part of the group. Other portions of the series consist of bluish, argillaceous shales and plastic clays, ranging in color from bluish to whitish. Numerous bands of sandstone, and of arenaceous magnesian limestone, from four to eighteen inches in thickness, occur irregularly in the middle and upper portions of the group. Bands of iron pyrites, of local extent, but sometimes from one to six inches through, are found imbedded in the shales and clays. The "black slate" proper, of the series, is freely combustible; and in some cases, where fire has been communicated by accident to outcropping masses, the combustion has survived for months. By distillation, this shale affords petroleum and all the related compounds. Its appearance at the surface is generally regarded as an indication of coal, and many serious disappointments have resulted from ignorance of the fact that the "black slate" is several hundred feet beneath the coal measures.

The bituminous shales of this group of rocks are generally considered to be the source of most of the native petroleum of the Northwestern States. It is the opinion of Dr. T. S. Hunt, of the Geological Commission of Canada, that the product issues from a bituminous limestone—the Corniferous limestone—next underlying these shales; but though this limestone may unite its exhalations with those of the Huron group, it seems more probable that the latter supplies the principal amount of the bituminous flow. It is also asserted that, at least in a few cases, petroleum is eliminated in quantities of commercial importance from the rocks embraced in the coal measures.

Wherever the oil-producing shales are exposed to the air, or covered

by a porous medium, the product of the slow spontaneous distillation going on escapes into the atmosphere, and is lost. Where the shales are covered by an impervious layer, as of shale or plastic clay, the oil and gas elaborated are retained in the rocks, filling cavities by driving the water out by elastic pressure, and saturating porous strata embraced in the formation, or intervening between it and the impervious cover above. In the oil region of Ohio and Pennsylvania, an overlying sandstone becomes the reservoir in which these products are accumulated; and the oil wells of that region consequently terminate in that sandstone without penetrating to the original source of the oil. Throughout the oil-producing region of Canada and Michigan, this sandstone is wanting, but the destruction of the outcropping shales by geological agencies has created an immense deposit of argillaceous drift material, which forms an impervious cover for the petroleum beds. In Enniskillen the lower portion of this drift deposit is rendered somewhat porous by the presence of gravel, and this, in consequence, has become saturated with oil in the same manner as the overlying sandstone of Ohio and Pennsylvania. By boring into the rock, however, a lighter and purer oil is found surcharging the beds of sandstone and arenaceous limestone by which the group is intersected. It is found that the quality of the oil improves with the depth from which it is obtained. That drawn from the drift gravel lying upon the surface of the rock, is dark, thick, and impure, and commands but a moderate price for purposes of distillation. It is now principally employed for lubrication.

Though it is not yet actually demonstrated that rock oil exists in Michigan in quantities of economical importance, we possess every favorable indication which has ever been discovered in the prosecution of this new branch of industry. In the first place, the topography of the oil region of Michigan is identical with that of Enniskillen. The surface is level, rather low, and inclined to dampness. The soil is thin, argillaceous and indifferent for agricultural purposes. The forest growth, nevertheless, is dense and luxuriant, consisting of Beech, Hard Maple, Swamp Elm, and Black Ash, with occasional trees of White Ash, White Oak, Hickory, Sycamore, &c., interspersed. The water in the vicinity of the oil manifestations is of a dark color. Black river of Michigan seems to be the counterpart of Black creek of Enniskillen. It would seem evident that the color of the water in both cases must be due to some constituent of the crude oil; and yet a chemical examination of the water of Black river taken at Port Huron, did not sustain my expectations. It should be tested six or eight miles further north.

When we compare the more direct indications, the resemblance is equally complete. The oil region of Michigan abounds in "gum beds," or masses of inspissated oil, some of which have hardened to the character of asphalt. The oil is still issuing, and it shows itself on the surface of standing water everywhere throughout the Baker tract. Moreover, inflammable gas, the almost infallible criterion of an oil district, exists in very great abundance, and, in several instances, has been known to escape with noise and violence from the bottom of wells. In one instance the jet, when lighted, illuminated the country over a radius of four miles, and the sound was like that of steam escaping from a high-pressure engine. Many similar cases have occurred.

Whatever differences exist between the geological indications observed in the two regions are rather in degree than kind; and the advantages are undoubtedly on the side of the Michigan region. Canada West is located upon the thinning outskirts of the formation; Michigan embraces the great mass of it. And, in the abundance of gas emitted from the underlying rocks, the Enniskillen region bears a very unequal comparison.

It is quite possible that no porous stratum will be found in Michigan reposing upon the surface of the rock. Its occurrence is accidental anywhere. Should it be found wanting, and "surface oil" consequently wanting, this fact would not have the slightest bearing on the probability of oil in the rocks below.

I have stated that the Huron group, or Oil formation, extends continuously from the peninsula of Michigan into the peninsula of Canada West. To embrace both regions under one designation, they might be spoken of as the *Peninsula Oil Region.* It is not to be imagined that oil will certainly be obtained at all points within this region. It may be, but probably will not. Slight undulations are liable to exist in all strata so nearly horizontal as those of the two peninsulas. The volatile emanations from the strata would therefore accumulate under the highest arches. Possibly one of these arches runs nearly north and south through the Canadian region, from Bothwell to Petrolea, and beyond, while another underlies the district of petroleum manifestations on the Michigan side of the river. For the present, it would be safest to confine large expenditures to the immediate vicinity of this belt of indications.

In the transverse section across the strata, already referred to, an attempt has been made to show in what way the oil-bearing rocks may rise in two or more gentle undulations, thus inducing the oil and gas to accumulate along geological axes, and become restricted to belts of country of limited width.

4. *Account of some new or little known species of Fossils from rocks of the age of the Niagara Group.* From the 18th Report on the New York State Cabinet; printed in advance of the Report, and issued January 1865, by Prof. JAMES HALL. 48 pp. 8vo, with many illustrations.—These pages contain descriptions of a number of new species of Cystideans, with figures of seven of them, and of several Crinideans, Trilobites, and various Mollusks. The following important remarks on the age of the Galt or Guelph limestone of Canada, and Leclaire beds of Iowa, and their relations to the New York Niagara group, form an introduction to the paper:

In the study of the fossils of the Niagara group and associated strata, previous to the publication of the final report on the Fourth Geological District,[1] and also to the publication of the second volume of the Paleontology of New York, I separated a few species found in the impure drab-colored limestone from Wayne county, New-York.

The limestone containing these fossils became exposed only in the bed of the canal during its excavation; and the low country, or deeply drift-covered surface in the vicinity did not admit of any exposure of the

[1] The same reference of species had in fact been made during the Geological Survey; and they were thus published in the Report on the Fourth Geological District in 1843.

rock in place. The materials were thrown out of the excavation in connexion with the soft marls of the Onondaga-salt group, and the specimens of rock containing the fossils preserved the peculiar celluliferous structure and characteristic color of the argillaceous limestone of that formation. Differing so especially from any known beds in the Niagara group, I did not hesitate to refer them to the Onondaga-salt group, since there was no evidence of any other formation in the neighborhood.

Throughout the State of New York, the country along the junction of the Niagara and Onondaga-salt formations is low and level, or covered by drift accumulations; and no opportunity offered of discovering any exposure of similar beds along the course of the outcrop. In some places in Monroe county, we have been able to trace the two formations to within a few feet of their contact with each other; but no fossiliferous beds, similar to those of Wayne county, have been found. At a later period,[2] my attention was called to some peculiar fossils collected at Galt, in Canada West; and in visiting the locality, I discovered some pieces identical with those before known, from beds which I had regarded as of the Onondaga-salt group in New York. As this limestone at Galt [and Guelph] was clearly above the great Niagara limestone of the Falls, and contained an almost entirely different set of fossils, I very naturally inferred that it belonged to the next higher formation, or the Onondaga-salt group; and that the Wayne county locality was a feeble representation of the limestone of Galt.[3] For these reasons the two were treated as identical, and referred to the age of the Onondaga-salt group; an opinion at that time sustained by the members of the Canadian Geological survey.

At a later period, during the Geological Survey of Iowa, I recognized, at the Leclaire rapids on the Mississippi river, a limestone holding the same relative position, having the same lithological character, and containing some identical and many similar fossils with the limestone of Galt or Guelph in Western Canada; and I thus announced its apparent relations in the Report on the Geology of Iowa.

"Should the identity of the limestone of these two distant localities be proved, it will afford sufficient ground for separating these beds from the Onondaga-salt group, and for establishing a distinct group. It seems quite probable that the limestones of this period have their eastern extremity in Central New York, where, from their small development, as well as from similarity of lithological character, there seemed no sufficient ground for separating them from the non-fossiliferous beds of the Onondaga-salt group.[4] Since, however, in Canada, these beds attain considerable importance, and (admitting the conclusions above given) acquire a still greater thickness and more distinctive character on the Mississippi river, it seems necessary to elevate them to the same rank as the other groups of the series." *Geology of Iowa*, vol. i, p. 75, 1857.

[2] After a considerable part of vol. ii had been printed.

[3] The name *Galt* being considered objectionable, on account of a similar term already in use, and the same rock occurring also at Guelph, it has been called the "Guelph formation," in the nomenclature of the Geological Survey of Canada.

[4] My views regarding the presence of the Onondaga-salt group proper in Wisconsin and Iowa have somewhere been called in question, and I have only to remark in this place that I have seen no reasons on my own part, nor facts adduced on the part of others, to change my opinion in reference to the occurrence of this formation in the localities I have heretofore cited.

Some two or three years later, I explored the geology of the central and eastern portions of Wisconsin and the adjacent parts of Illinois. I here found the limestone of Racine, and a part of Waukesha and some other localities, resembling in all respects that of Leclaire, and holding many of the same fossils. It is likewise underlaid by the even-bedded darker-colored limestone, bearing *Halysites catenulatus*, *Pentamerus oblongus*, and many large Orthoceratites, which are everywhere regarded as evidence of the Niagara age. I could not hesitate, therefore, to parallelize the succeeding beds with the limestone of Leclaire, though we had failed to trace it across the country in a continuous outcrop. At the same time, on critical examination of the collection of fossils made at Racine and at some other points, I detected many species known as characteristic of the Niagara formation in the State of New York, requiring its recognition as a member of that group (rather than of the Onondaga-salt group), and uniting with it, as identical in position, the Leclaire limestone.[5]

At the same time, we have recognized, from Racine and adjacent localities, including Leclaire in Iowa and a single locality in Illinois, the following species which are identical with, or are very closely allied to, those from Galt in Canada West: *Pentamerus occidentalis*, an *Obolus*-like fossil, a *Favosites* and a species of *Amplexus* which are identical in several localities, *Cyclonema sulcata*, *Murchisonia Logani*, *Murchisonia* identical with or closely allied to *M. mylitta* (Billings), an undescribed *Murchisonia* from Racine identical with one from Galt, *Subulites ventricosa*, *Pleurotomaria solaroides? Loxonema longispira*, besides other forms which are closely allied to species of the Guelph limestone.

An examination of several localities in Wisconsin shows that this peculiar fossiliferous limestone is very unequally distributed. At Racine it has a very considerable thickness;[6] while in other places, either from denudation or other causes, it is very thin, or even absent. In some places in the vicinity of Milwaukee and Waukesha, there are indications of beds of passage from the regularly bedded limestones below to the unequally bedded rock above. There appears, indeed, very good evidence of the irregular or unequal accumulation of this higher rock in many of the localities along a considerable portion of the outcrop; and where the lower part of the formation comes to the surface, the upper rock does not appear to be developed. I am therefore induced to believe that this limestone at Racine, the mass at Leclaire and extending thence into Iowa, as well as the Guelph formation in Canada and the feeble representation of the same in New York, are really lenticular masses of greater or less extent, which have accumulated upon the unequal surface of the ocean bed in a shallow sea during the latter part of the Niagara period. These isolated masses of limestone have close relations with each other, while their relations with the Onondaga-salt group, though very intimate in the single locality in Central New York, become less and less conspicuous in a westerly direction.

[5] Report on the Geology of Wisconsin, p. 67, 1861.

[6] I am inclined to believe that I have overestimated the thickness of the limestone at Leclaire from the presence of lines of false bedding, but I have had no opportunity for a re-examination of the locality.

5. *Observations on the Geology of Southern New Brunswick*, made principally during the summer of 1864 by Prof. L. W. BAILEY, and Messrs. G. F. MATTHEW and C. F. HARTT, prepared and arranged, with a geological map, by L. W. BAILEY, A.M., Prof. Chem. in the Univ. of New Brunswick, &c. Printed by order of the House of Assembly. 160 pp., 8vo. Fredericton, 1865.—In this Report, the distribution of the formations is given, with many details as to structure, fossils, geological relations, and economical products. The formations included are the Paleozoic, with the Azoic below these, and the Triassic Red Sandstone above, together with Post-tertiary beds. The Report contains a large colored geological map, showing the limits of the out-cropping rocks. The outcrops are nearly parallel with the coast, or about N.E. by E. in direction. The Azoic is in the region of Portland; and at St. Johns, between it and the Bay of Fundy, occur Primordial rocks, affording, according to determinations by Mr. Hartt, *Paradoxides* and other characteristic fossils, besides some Brachiopods. Mr. Hartt observes upon these fossils:—

"Representatives of four genera of Trilobites have been obtained thus far from the Saint John rocks, viz:—*Paradoxides*, *Conocephalites*, *Agnostus*, and *a new genus?* allied to *Conocephalites*.

The number of species in each genus has not yet been satisfactorily made out; but of *Paradoxides* there are at least five, of *Conocephalites* seven, and of *Agnostus* and the *new genus* each one.

All the species appear to be new. One of the *Paradoxides* bears a close resemblance to *P. rugulosus*, Corda, from the *Etage* C of Barrande, in Bohemia, and one of the *Conocephalites* is allied to *C. coronatus*, Barrande, from the same fauna and horizon, though neither is identical with the European species.

There are six species of Brachiopoda, belonging to the genera *Orthisina*, *Discina*, *Obolella*, and *Lingula*. I have not been able to identify any of the forms with described species.

Though all the species from the Saint John group are apparently new, yet the occurrence of *Paradoxides* and *Conocephalites*, genera confined entirely to the so-called *Primordial fauna* of Barrande, and everywhere characteristic of it, together with the strong likeness borne by the Saint John species, in their facies, to those of the same genera of the faunæ of the "Primordial" in Europe and America, enable us unhesitatingly to assign to the Saint John group, or at least to that lower part of it which has afforded Trilobites, a geological position equivalent to Barrande's Etage C, or to the Potsdam proper of America.

The lower part of the Saint John Group, at Coldbrook, has been divided by Mr. Matthew, on lithological grounds, into three Bands, viz:

No. 1. The lower or arenaceous band, with no determinable fossils, and constituting passage beds from the Coldbrook Group.

No. 2. Argillaceous shales, rich in fossils, *Paradoxides*, *Orthisina?*, *Conocephalites*, *Obolella*.

No. 3. Carbonaceous shales, full of fossils, *Paradoxides*, *Conocephalites*, *Orthisina*, *Discina*, &c., all much distorted."

Prof. Bailey, speaking of the *Albertite*, states that in his opinion and that of Mr. Matthew, "it is neither coal nor jet, but an oxydized oil, derived from the decomposition of fish remains, and subsequently changed

by chemical action," giving as reasons the position and constitution of the material, the nature of the adjoining rocks, and the fact that "springs containing oil are not uncommon throughout the district in which the Albertite is found."

6. *On Devonian Insects from New Brunswick;* by S. H. SCUDDER. (From a communication addressed by Mr. Scudder to Dr. C. F. Hartt, of New Brunswick, dated, Boston Society of Natural History, January 11, 1865.)—I have made as careful an examination as my present circumstances will permit of your most interesting collection of the fossil remains of insect-wings from Lancaster. There are ten specimens in all, eight of which are reverses of one another, thus reducing the number to six individuals; of these, one, a mere fragment, belongs, I think, to the same species as another of which the more important parts of the wing are preserved, so that we have five species represented among these Devonian insects, and these remains are all, I suspect, composed of portions of the anterior wing alone. The data being thus fragmentary, the conclusions cannot be quite so satisfactorily determined as we could wish, but we can still discover enough to prove that they are of unwonted interest. Besides the peculiar interest which attaches to them as the *earliest known traces of insect life on the globe*, there is very much in themselves to attract and merit our closest attention.

One of them is a gigantic representative of the family of *Ephemerina* among Neuroptera, some three or four times the size of the largest species now living with which I am acquainted.

Another borrows some striking points of the peculiar wing-structure of the Neuropterous family *Odonata*, and combines with them those of families remote from that, and even belonging to a distinct section of the Neuroptera, exhibiting to our view a synthetic type which combines in one the Pseudoneuroptera and the Neuroptera, and represents a family distinct from any hitherto known.

Other fossil insects, found in Carboniferous concretions in Illinois, and described in this Journal ([2], xxxvii, 34), which Professor Dana has kindly allowed me to evamine,[1] also belong to hitherto unrecognized families, exhibiting similar relations to sections of Neuropterous insects in our day disconnected; and your third species is a member of still another family of Neuroptera, which finds its natural relations between the two described by Professor Dana.

A fourth, of which only an unimportant fragment was found, would seem to belong to the Neuroptera; but by some peculiarities of the minuter cross-veins, thrown off in the middle of the outer edge of the wing in a most irregular and unusual manner, suggests no intimate relations with any known family, but must have belonged to a group of large and weak-winged insects.

The fifth and last to be mentioned is of very striking interest, because, while it exhibits the peculiar venation which forms the well known tympanum or stridulating apparatus of the male in the Orthopterous family *Locustariæ* (though differing somewhat from that), it also most resembles the Neuroptera in all, or nearly all, the other peculiarities of its structure, and suggests the presence in the insect-fauna of those ancient times

[1] The results of this examination will shortly be communicated to this Journal.

of a synthetic type, which united the characteristics of the Orthoptera and Neuroptera, in themselves closely allied; this point however requires patient and severe investigation, and only my earliest impressions are here recorded, made, however, immediately after a close examination into the relations of other fossil insects.

7. *Note on the Azoic age and metamorphic origin of the Iron Ore of Mexico, described by N. S. Manross, at page* 309 *of this volume;* by J. D. DANA.—The great thickness and extent of the magnetic and specular iron masses of Guerrero leave little doubt that the rocks belong, like those of Northern New York, Michigan and Canada, to Azoic or præsilurian time, and thus they indicate the existence of an Azoic area in this part of the continent. The metamorphic nature of the iron ore is proved by the alternation of the beds with conformable layers or strata of granular or metamorphic limestone, and syenite or granite. In a section of the Chutla "iron mountain," figured by Mr. Manross, the great iron ore bed is represented as lying beneath "beds of limestone," and over granite. In another, of the iron mountain of Las Anonas, the largest bed of iron ore is divided along the middle by a bed of limestone, and on either side there are alternate beds of iron ore and syenite with epidote. In another, the Sochipala region, the iron bed has beds of granular limestone above it; and below, syenite, then another iron ore bed, 30 feet thick, and then syenite again. Thus the evidence is the same in kind as in the Azoic region of the north.

8. *Report on the Geology of Illinois;* by A. H. WORTHEN.—The Legislature of Illinois has recently appropriated $20,000 for the publication of 3000 copies of the Geological Report of Illinois, made by Mr. Worthen under the authority of the State. The plates will be numerous, and, as we know from an examination of the figures, excellent. In working up the Paleontology, Mr. Worthen has had the assistance of Mr. F. B. Meek for Zoology, and L. Lesquereux for Botany. The Report will yield to none hitherto published in the country in the value of its contributions to science. The State is especially rich in fossil shells, fishes and crustaceans of the Carboniferous age, and affords many new species of coal plants.

9. *Check List of the Invertebrate Fossils of North America: Miocene;* by F. B. MEEK. Smithsonian Miscellaneous Collections. 32 pp. 8vo. Washington, Nov. 1864.—This list of Invertebrate Miocene Fossils of North America is complete to the time of publication, both as regards paleontological discovery, and zoological science. Critical remarks are appended to the list.

10. *Ichthyosaurian Skin.*—A specimen of the *Ichthyosaurus tenuirostris*, recently obtained at Barrow-on-Soar, shows a large extension of the dermal covering upon the surface of the slab, seeming to indicate that the animal had a prominent ridge along the dorsal surface similar in appearance to that which the males of the pond-newt, *Triton cristatus*, present in spring.—*Reader, Dec.* 3.

11. *Murchison on the Drift.*—The last annual address of the President of the Royal Geographical Society, Sir R. I. Murchison, contains a discussion at length of the question of the European drift, in which the author sustains the Iceberg theory. Only the want of space prevents our citing his views in this Journal.

12. *Annual Report of the Geological Survey of New Jersey, for the year* 1864, by Prof. GEO. H. COOK, State Geologist. 24 pp. 8vo. Trenton, N. J., 1865.—Prof. Cook presents, in his Report, a brief statement of the progress of his survey, dwelling mostly on economical results. A colored map of the formations is included.

III. BOTANY AND ZOOLOGY.

1. DECANDOLLE: *Prodromus Syst. Nat. Regni Vegetabilis.* Pars 16, Sect. posterior, Fasc. I, *sistens Cupuliferas*, etc. Nov., 1864. pp. 160.—In pursuance of the plan adopted for bringing out the concluding volumes of the Prodromus in parts, as soon as the respective orders are elaborated, we have here the *Cupuliferæ*, upon which Mr. DeCandolle himself has bestowed so much labor,—the leading genus, *Quercus*, extending to 281 species, *Castanopsis* (to which belongs *C. chrysophylla* of California), 14 species, *Castanea*, only two well determined species, and *Fagus*, 15; the *Corylaceæ*, admitted as an order, after Hartig and Doll; also the *Juglandaceæ*, the earliest botanical work of Casimir DeCandolle, whose separate memoir upon the subject we have already noticed with approbation; the *Myricaceæ* by the same promising hand (38 species of *Myrica*, including *Comptonia*, and Chapman's *Leitneria* of Florida); the *Platanaceæ*, and, in a note, *Liquidambar*, by Alphonse DeCandolle. The *Betulaceæ* and *Salicineæ* will follow in this volume, we believe at no distant date. A. G.

2. J. D. HOOKER: *Handbook of the New Zealand Flora.* Part I. London: Lovell Reeve & Co. 1864. pp. lxxviii, 392, 8vo.—This is a new member of the series of British Colonial Floras. The present part includes all the known Phænogamous plants and Ferns, not only of New Zealand, but of the outlying islands which may be regarded as of the New Zealand group, such as the Chatham and Kermadec islands on the one hand, and the Lord Auckland and Campbell's islands on the other. The Cryptogamous plants are to follow. The basis of the present work, of course, is Dr. Hooker's Flora of New Zealand and the Flora of Lord Auckland and Campbell's islands, making three large illustrated volumes of the Botany of the Antarctic Voyage under Capt. Ross,—volumes much too bulky and costly to subserve the main purpose of the Handbook, i. e., to be freely used by the colonists themselves. Moreover, large discoveries have been made since the Flora Novæ-Zelandiæ appeared, mainly in the middle island, adding fully one-third to the number of flowering plants previously known. The genera are here brought up to 303, the species to 935,—still exemplifying the insular paucity. Of these species 677 are peculiar to these islands; 222 are Australian, and 111 American. There are, besides, 51 Australian representative species, and 32 American representative species. Among the American identical species are *Myosurus aristatus*, of the Pacific United States and Chili, and our *Elatine Americana*, which has a wide range in the southern hemisphere, viz: in S. America, New Zealand, Feejee Islands, Australia, and Tasmania. Dr. Hooker remarks that few if any plants appear to have been collected in the Lord Auckland's Group by Commodore Wilkes's expedition. A single officer, Lieut. Holmes we think, passed a few hours on land, collecting, however, among other plants, a single specimen of an apparently new *Ranuneulus* (*R. Aucklandicus*), which has been accidentally overlooked in the present work. A. G.

3. Martius: *Flora Brasiliensis;* fasc. 36, 37, 38. fol. Dec. 1864.—The *Gesneraceæ*, by Dr. Hanstein, the curator (succeeding the late Dr. Klotszch) of the Berlin Herbarium; with eleven plates. The *Salsolaceæ* (*Chenopodiaceæ*), by Frof. Fenzl, of small importance in Brazil; with five plates, one of them illustrating *Chenopodium anthelminticum*, and one *Roubieva multifida*. *Magnoliaceæ*, *Winteraceæ* (retained, as likewise *Schizandreæ*, as ordinally distinct), *Ranunculaceæ*, *Menispermaceæ*, and *Berberideæ*, by the acute Dr. Eichler; twenty-six plates, one of them devoted to the anatomy of the wood of *Drimys Winteri*, and two to that of Menispermaceous stems, which the author appears to have discussed with much ability. A. G.

4. *The Journal of the Linnæan Society*, No. 31 (Dec. 1864), is especially rich in articles upon Dimorphism and even Trimorphism in plants, and upon the agency of insects in their fertilization. There is 1. *Notes on the Fecundation of Orchids, and their Morphology*, by the late Dr. Cruger, Director of the Botanical Garden, Trinidad; *Catasetum* and *Stanhopea* being the principal subjects, and the conclusions of Mr. Darwin being fully confirmed. 2. *Dimorphism in the Flowers of Monochoria vaginalis*, by Dr. Kirk. The additional kind of flower would seem to be somewhat after the fashion of that of *Utricularia clandestina*, and arranged for self-fertilization. 3. *On the Individual Sterility and Cross-Impregnation of certain species of Oncidium*, by Mr. John Scott. He shows by experiment "that the male element of *O. microchilum* will fertilize the female element of the two distinct species, *O. ornithorhynchum* and *O. divaricatum cupreum*, and yet be completely impotent upon its own female element; nevertheless, the susceptibility of the latter (female element) to fertilization is shown by its fertile unions with another individual of the same species, and likewise by a fertile union with an individual of a distinct species;" and the same is true of *O. microchilum*. 4. *Notes on the Sterility and Hybridization of certain species of Passiflora, Disemma, and Tacsonia*, by the same author. Species which, in cultivation, are perfectly sterile upon the application of the pollen to the pistil of the same individual, are readily fertilizable by the pollen of other individuals of the same or of an allied species, and their pollen is likewise potent upon such individuals; although in hybridization the influence frequently is not reciprocal. For instance, *Passiflora racemosa* may be fertilized by the pollen of *Tacsonia mollissima*, upon the ovules of which, conversely, the pollen of the *Passiflora* is utterly impotent. There are two Passionflowers, totally impotent when self fertilization is attempted, the pollen of one of which effects the development of the ovaries of the other, but never the seeds, while conversely even the ovary fails to develop. And there are two species of *Tacsonia*, the pollen of one of which causes the ovary and even the seed-coats of the other to develop, but never the embryo, while conversely the effect is sometimes the same, but generally nothing at all. Although general conclusions should be hesitatingly drawn from limited experiments upon cultivated plants, yet the known facts conspire to show that no sharp line is drawn in nature between fertility and sterility in crosses. 5. *On the Sexual Relations of the three forms of Lythrum Salicaria*, by Charles Darwin. Here we have the results of an investigation which Mr. Darwin has before

referred to. A curious case it is, and treated with the wonted sagacity and point of this prince of biological inquirers.

"In *Lythrum Salicaria* three plainly different forms occur: each of these is an hermaphrodite; each is distinct in its female organs from the other two forms; and each is furnished with two sets of stamens or males, differing from each other in appearance and function. Altogether, there are three females and three sets of males, all as distinct from each other as if they belonged to different species; and, if smaller functional differences are considered, there are five distinct sets of males. Two of the three hermaphrodites must co-exist, and the pollen be carried by insects reciprocally from one to the other, in order that either of the two should be fully fertile: but, unless all three forms co-exist, there will be waste of two sets of stamens, and the organization of the species as a whole will be imperfect. On the other hand, when all three hermaphrodites co-exist, and the pollen is carried from the one to the other, the scheme is perfect; there is no waste of pollen and no false co-adaptation. In short, Nature has ordained a most complex marriage-arrangement, namely, a triple union between three hermaphrodites,—each hermaphrodite being in its female organ quite distinct from the other two hermaphrodites, and partially distinct in its male organs, and each furnished with two sets of males."

One must study this instructive paper to see how neatly it is shown, "that only the longest stamens fully fertilize the longest pistil, the middle stamens the middle pistil, and the shortest stamens the shortest pistil. And now we can comprehend the meaning of the almost exact correspondence in length between the pistil of each form and the two half-dozen sets of stamens borne by the two other forms; for the stigma of each form is thus rubbed against the same spot of the insect's body which becomes most charged with the proper pollen." For the use which Mr. Darwin makes of this case, and the theoretical deductions drawn from a genus which presents trimorphic, dimorphic, and monomorphic species, the illustration of the advantage of trimorphism, and of the now established fact that sexual differences,—"thought to be the very touchstone of specific distinction,"—may characterise and keep separate the coexisting individuals of the same species in the same manner as they do those groups of individuals which we denominate species, we must refer to the memoir itself, not having space for a full abstract. Mr. Darwin, on raising from seed some individuals of our *Nesæa verticillata*, ascertained that this plant is also trimorphic. We commend it to the particular attention of any who may be disposed to prosecute farther such investigations, which, though requiring genius to originate, are easy to follow up, and almost inexhaustible in interest.

Of the remaining papers in this number of the Linnæan Journal, one is *On a Peloria and Semi-double Flower of Ophrys arenifera*, by Dr. Masters, to which a note is added, mentioning a nearly similar monstrosity of *Pogonia ophioglossoides*, (known to him from a description by Prof. Gray), collected by Rev. J. A. Paine, in a bog near Utica, N. Y. Several flowers were detected, last year, all sharing more or less of the peculiarity, viz: having three *labella* and the column resolved into small petaloid organs; the two accessory labella and a small petaloid body on the

other side of the flower answering to the three suppressed stamens of the outer series, while two little filaments answer in position to the suppressed lateral stamens of the inner series. It is hoped that other specimens may be detected the ensuing summer, and preserved in spirit for more searching examination. In another paper, Dr. Hooker identifies *Pinus Peuce*, Griesb., of the mountains of Macedonia, with *P. excelsa* of the Himalayas. Mr. Mitten describes new *Musci* and *Hepaticæ* of Japan and the coast of China, and Prof. Oliver, new genera of plants of Western Tropical Africa.

The longest paper in No. 32 (February, 1865) is one by Dr. Masters, *On the Morphology and Anatomy of the Genus Restio*, Linn., *together with an Enumeration of the South African Species.* There is also an extract from a letter of Prof. Brewer to Sir Wm. Hooker, *On the Forests of Sequoia* (*Wellingtonia*) *gigantea of California*, correcting the popular impression that this magnificent tree is extremely local and in danger of extinction. A. G.

5. *Further remarks on larve budding;* by Dr. W. C. Minor.—In a preceding number of this Journal (January, 1865) some notice was taken of a newly observed form of insect larve budding. The first number of the *Zeits. f. wiss. Zoölogie* for this year has a further account by Dr. Wagner, the original observer, which, beyond the interest of a detailed reply to questions from Dr. Siebold, contains some generalizations upon the phenomena of budding, which we give in brief. Dr. Wagner offers the following table of the grades between the simplest asexual and the most perfect sexual multiplication in the articulates.

I. Asexual spontaneous multiplication of the larve-nurse (Amme), with sexual generation of the developed animal. The germ is metamorphosed out of the fat or granular substance of the larve-nurse, and the animal has three or four transformations, in the forms of protoscolex, deuteroscolex, strobila and proglottis.—*Cestodes* and *Trematodes.*

II. Larves with sexual organs. The germs developed in these organs become individuals within the body of the larve-nurse, and are born alive, forming thus two metamorphoses.—*Aphides.*

III. Multiplication only in the perfect sexual animal:—*a.* In both males and females, but without sexual influence.—*Daphnidæ. b.* In one sex only, without sexual influence.—*Bees*, and some *Butterflies. c.* In one sex only, under the influence of fructification.

The author admits that nothing more than one point of view is attempted here, as deeper embryological studies will, by bringing out homologies and complications of structure, probably separate the higher and lower groups here put together. The case he has discovered in the Diptera seems to him to form a transition from the first to the second group. He refers to Leydig's comparison of the Aphis buds with the summer eggs of Daphnia, and of the true Aphis eggs with the winter eggs or ephippia of Daphnia, and extends the comparison to the larve buds and eggs here.

Upon the distinction between Parthenogenesis and Alternate genesis, he urges, that the former is a germination of buds in *special sexual organs*, though without fructification; while the latter is a *self-transformation*, also unfructified, of tissue into germs or buds, without any special

organ for the transformation, and in a degree independent of the nurse, and hence spoken of as *spontaneous*. The first of the groups given above comes under the head of alternate genesis—Generationswecksel—while the second, and in part the third, are instances of parthenogenesis. From this point of view, also, is the present case intermediate between the first and second groups.

He calls attention to the compensation, by the budding of the larve, for the limited egg-bearing capacity of the mature fly, and regards this compensational balance as usual in these cases. Also, considering the fat or granular substance of the larve-nurse to be directly transformed into these larves—a view confirmed, it will be remembered, by Dr. Meinert—he points to the hypertrophied eggs as a store of material for its production. That this granular substance is of a nutritive nature, is made altogether probable by its transference into the stomach through the Malphigian vessel, in cases of prolonged abstinence, and also by the presence of sugar in it; and he observes that the imperfect tracheal system and sluggish movements, for a time at least, of the larve-nurse tend to leave this store intact for the sole use of the buds.

Both in this, and the first paper of Dr. Wagner's, which we have now received, are numerous points of anatomical and physiological interest; such as the condensation in the mature fly of the separate ganglia of the larve, the valves at the posterior end of the dorsal heart—also figured by Dr. Pagenstecher—and which, as well as the glands along its side, supposed from analogy—in leeches—to form the blood corpuscles, we have ourselves seen.

It is perhaps not premature to state here that the writer has found a number of large oval germs in some minute larves observed lately, and he would urge the attention of American observers, who may be better situated than himself, to this inquiry; since, to judge from the difference in shape of the larve's head, these were not of the same genus.

6. *On the Hymenoptera of Cuba*, by E. T. Cresson. (From the Proceedings of the Entomological Society, Philadelphia, Jan. 1865. 200 pp. 8vo.)—The Entomological Society of Philadelphia has shown great activity since its formation, having issued many long and thoroughly labored memoirs. Mr. Cresson's work is based principally on the collection of Prof. F. Poey of Havana, purchased by Dr. T. B. Wilson of Philadelphia and presented to the Society, and also on specimens in the collection of Dr. J. Gundlach of Cuba. Most of the Chalcididæ, Prototrupidæ and Formicidæ are reserved for future papers.

7. *Synopsis of the Bombycidæ of the United States*, by A. S. Packard, Jr. Part II. 66 pp. 8vo. From the Proceedings of the Entomological Society, Philadelphia. Nov. 1864.

IV. ASTRONOMY AND METEOROLOGY.

1. *Correction of D. Trowbridge's article on the Nebular Hypothesis;* by the Author.—In my article on the Nebular Hypothesis, Art. xlvi, and note 41 (this Jour., [2], xxxix, 31, 32), I have committed an error in my reference to Prof. Ferrel's article on the retarding effect of the sun and moon on the earth's rotation, in Gould's Astronomical Journal. One part was owing to a careless misconception, and the other to a

misprint in his article. The retarding effect is given in his article in angular velocity, and not in absolute velocity as I conceived (though it was so given that one might easily make the mistake). The effect of the earth on the moon is 5625 times as great, and not 562½ times, as printed. My general reasoning, however, as applied to the nebular hypothesis, is not affected by the error which I made. My error was in numbers (I read Prof. F.'s article several years ago when it was published) and not in principles.

Hector, N. Y., March 23d, 1865.

2. *Cambridge Observatory in* 1864. Extract from the Annual Report of the Observatory Committee to the Board of Overseers of Harvard College, (made previous to the decease of the Director, G. P. Bond).—The labors of the year have been chiefly directed to the continuation of the zone observations, the examination of nebulæ, observations on the asteroids, on changeable stars, and on the two telescopic comets of the year. During the last winter and the early part of the spring, observations of minute inquiry were made on Orion on fifty-nine nights, resulting in the discovery of many new features, as well as corroborating characteristics heretofore published. Among the discoveries were six variable stars in the neighborhood of the Trapezium.

In the prosecution of the zone observations, Mr. Safford obtained the position of four thousand seven hundred stars by the great equatorial, besides performing the necessary reductions and computations involved. Very few persons, who have not had the privilege of examining the great mass of manuscripts already accumulated, can form any conception of the enormous labor required in this valuable undertaking.

The director repeats his great obligations to Mr. Safford for his devotedness to all the interests of the Observatory, and the skill and fidelity with which he has executed the several parts assigned him.

3. *Extracts from the Address of* W. De La Rue, *President of the Astronomical Society of London, connected with the awarding of the Society's medal to* Mr. G. P. Bond.—The announcement that the Royal Astronomical Society has awarded its gold medal to Professor Bond will be hailed with universal satisfaction by those interested in the science which he has so ably cultivated. At the anniversary meeting of the Society, held on Friday, Mr. Warren De La Rue, under whose auspices the Society has taken such a large stride in its path of usefulness, according to custom, accompanied the announcement by an address, which should add greatly to the pleasure of the medallist, so warmly does it appreciate the work done by the transatlantic astronomer, who has for so long had the finest telescope in the world to use, and has used it so well. The address is too long to be reproduced *in extenso*, but we give several extracts from it, reserving our report of the other business done at the meeting for a future occasion.

Mr. De La Rue commenced by observing, that 'when father and son have worked zealously together in the same direction, and especially in those cases where the son has succeeded to the position created and held for a number of years by the father, it is extremely difficult to draw any line of demarcation, from which to date the commencement of the son's independent career, or the cessation of the momentum imparted to his

course of activity by the father. This is especially the case with respect to the late Professor W. C. Bond and his son, Professor George Phillips Bond, to whom the council have awarded the highest mark of distinction this society can confer.

In the estimate formed of his scientific work, the professional astronomer is generally placed somewhat at a disadvantage in comparison with the amateur. In the case of the latter, the whole of his work is weighed in the balance, while, in that of the former, large deductions are made from his labors as belonging properly to the duties of his official position. An official astronomer, in this age of zealous activity and speedy publication, inaugurated I believe by the example of the present Director of our own Royal Observatory, may, consequently, produce much good and original work without his name coming with due prominence before his peers, so long as that work falls, or rather appears to fall, within the range of his official duties. . . .

Unquestionable evidence that Prof. G. P. Bond has done more for our science than even a scrupulous discharge of his duty demanded, was given by the appearance of the 'Annals of the Astronomical Observatory of Harvard College,' vol. iii, 1862. At first sight, indeed, that volume might appear to be nothing beyond a record of excellent but, nevertheless, official work; when, however, I shall presently come to speak of its details, it will, I doubt not, be conceded that it belongs in a great measure to the category of private labor independent of official duty.'

After stating that Prof. G. P. Bond had been a most active contributor to astronomical discovery and methods for a long series of years, and that, besides his great and indeed unique work on the comet of 1858, an amount of good work had been accomplished by him which fully entitled him to the distinction now conferred upon him, Mr. De La Rue remarked that 'his earlier exertions appear to have been directed to the discovery of comets and the computation of their orbits, and in the Report of the Committee of Harvard College he is mentioned as having detected independently eleven of those bodies. It is not surprising, therefore, that so zealous an observer of these strange visitors, about the nature of which so little is known, should have been one of the most assiduous observers of so splendid a comet as that of Donati. It was also a natural consequence that Professor Bond should have desired to compare his own results with those of other observers, but it was by no means a necessary sequence that he should have entailed upon himself the enormous labor of collecting, systematically arranging, and reducing, the whole of the data resulting from this comparison.

The circumstances attending the apparition of the comet of 1858 were peculiarly favorable; its early discovery by Dr. Donati, while yet a faint telescopic object, and the prediction of its great brilliancy and splendor, having had the effect of directing the attention of a great number of astronomers to this remarkable body. Hence, in the history of cometary observations, there has never been collected so large a mass of observations as in the case of Donati's comet, the absence of moonlight, the short duration of twilight, and the remarkable continuance of clear weather during the most important part of the comet's apparition, largely contributing to this result. The darkness of the sky, which

served as a background to the comet, was peculiarly favorable to the delineation of the fainter outlines and peculiar features of this splendid visitor, which was visible to the naked eye from August 19 to December 9, one hundred and twelve days. The whole period of visibility in the telescope extended from June 2, 1858 to March 4, 1859, an interval of two hundred and seventy-five days. One of the most successful workers in recording the phenomena of this comet was Professor G. P. Bond, and the sketches and drawings made at the Observatory of Harvard College form the main contribution to the splendid graphic illustrations which are a remarkable feature of this altogether remarkable production. The pains taken to insure a truthful representation of this comet of 1858, both in its eye- and telescope-features, few are better able than myself to appreciate; and I am able, from my own practical experience, to state that the success which has attended these efforts has been deservedly won by battling with greater difficulties than would probably be imagined by the uninitiated.'

'The comprehensiveness of Professor Bond's work will be at once recognized by an enumeration of the various sections into which it is divided. They are as follows:—

I. Figure and Position of the Tail; II. Observations upon the Secondary Tails; III. Reduction of Observations upon the Figure and Position of the Tail; IV. Probable Errors of Observations of the Tail; V. On the Deflection of the Tail; VI. Columnar Structure of the Tail; VII. Reduction of Observations on the Secondary Tails; VIII. The Nucleus and Envelopes; IX. On the Outline of the Head of the Comet; X. On the Branches and Central Darkness of the Tail; XI. The Nucleus; XII. The Envelopes; XIII. The outer faint Veil; XIV. On the Direction and Initial Axis of the Tail; XV. Summary of the Contents of the Volume.'

'The sections I to VII relate to the figure, dimensions, and positions of the tail, from its first appearance on August 14, 1858, seventy-three days after its first discovery, when it was seen at Copenhagen by D'Arrest and at Vienna by Homstein, until the last recorded observation at Santiago, in Chili, by Moesta, on February 7, 1859. Arranged alphabetically under the same date, are the names of the several observers, sixty-seven in number, whose statements are given verbatim in the language in which they were written; but the value of this record is greatly enhanced by the occasional remarks of the author, who draws the reader's attention to points of special interest, and thus brings under notice the changes which actually occurred, as well as those which did not take place although anticipated from previous hypotheses. The reader is informed, in the introductory chapter, that from June 2 to September 8, the earth was on the north side of the plane of the orbit, and on the latter day crossed the line of nodes, giving an opportunity for observations on the figure of the tail projected on a plane at right angles with the comet's orbit. After the middle of September the tail was presented in nearly its full-length proportions; within a day or two of the perihelion passage on September 30, the axis of the tail was brought to a position at right angles to the line of vision, and, ten or twelve days later, when the comet had reached its least distance from the earth, its profile was almost precisely that of a section in the plane of the orbit. Professor Bond remarks, that where so little is

known, *à priori*, respecting the actual figure, there is an obvious advantage to be derived from these accidental circumstances of its position, by which the influence of perspective foreshortening is, in a great measure, eliminated.'

'In order to recognize and explain any errors or ambiguities in the observations themselves, a provisional chart was constructed, showing the path of the comet and the position of the tail among the neighboring constellations. This chart naturally gave confidence and insured accuracy in the subsequent discussion of the observations, which are duly 'weighted' according to the dependence which can be placed upon them.'

'Lastly, the normal outlines of the tails are given in a series of charts which represent the final results deduced from the whole series of observations between September 16th and October 17th, referred to the common epoch for each date of 7^h mean solar time at the Observatory of Harvard College. The figure of the tail is preserved free from any considerable distortion by the method adopted; namely, by projection on a tangent cylinder, the position of which, in regard to the path of the comet, was selected with that view.'

After having given an idea of the comprehensiveness of the first seven sections of Professor Bond's work on the comet of 1858, the speaker referred to those parts which treat of the telescopic observations of the nucleus and envelopes—which are characterized by the same care, skill, and resource, though the difficulties of dealing with the discrepancies must have been enormously greater.

'Professor Bond gives in his work a plate representing the drawings or engravings of the nucleus and envelopes in the form they reached him. A cursory inspection of this collection will suffice to show how great is the diversity of portraiture of the same object, even when made at about the same period of absolute time.'

'The form of the head of the comet received considerable attention; the result was such as to show that its outline did not accord with a parabola, but that its contour was nearly that of a catenary curve. Moreover, advantage was taken by Professor Bond, of the apparition of Comet III, 1860, and more recently, of the great Comet of 1861, to test some of the hypotheses discussed in reference to the Comet of 1858, notably the phenomena of the successive throwing off of a series of envelopes from the nucleus, and the diminution of their elevation velocities as they receded from it. A recent careful revision of these phenomena has, according to the report of Professor Bond for 1864, completely confirmed the results previously announced.

Before dismissing the consideration of the volume on the comet of 1858, Mr. De La Rue added a few words in honor of Mr. Josiah Quincy, of Boston, who defrayed the expenses of the letter-press of the volume, and of the twenty-four gentlemen of Boston, Waterton, Cambridge, and Salem, who subscribed the funds necessary to cover the cost of the unrivalled engravings with which it is illustrated. It is most creditable to these gentlemen that they so fully appreciated the importance of doing ample justice to the labors of the Director of the Observatory of Harvard College, and enabled him to lay before the public such a written

and graphic record as could not fail to attract the attention and obtain the confidence of the astronomical world. It should be remembered that the Observatory in question is not an institution endowed with ample funds to meet all its requirements, but that, on the contrary, appeals for aid to patriotic citizens have frequently been necessary, and have always been generously responded to whenever any object of scientific importance could be thus advanced.'

Passing from this splendid addition to our knowledge of the physical phenomena of comets, Mr. De La Rue next referred to a very important step made by Professor Bond in connection with the theory of planetary perturbation. 'It is well known that when the excentricity and inclination of the orbit of the disturbed planet are considerable, the problem of three bodies represents difficulties which have baffled the efforts of the most consummate mathematicians since the time of Newton. Fortunately, when the question relates to the computation of the perturbations of the principal bodies of the planetary system, such difficulties are not found to exist.' 'But for comets, which revolve in orbits of every degree of excentricity, and inclined to the ecliptic at angles of all degrees of magnitude, a special method is necessary. The chief merit of improving and applying this method of computation must, in all fairness, be assigned to Professor Encke. Nevertheless, Encke and other German astronomers were to a very great extent anticipated by Professor G. P. Bond, whose suggestions were made so early as the month of May, 1849, in a paper published in vol. iv. of the 'Memoirs of the American Academy of Arts and Sciences,' whereas Encke's labors bear date from 1851.' 'It is but right, however, to add that at the time when Professor Encke communicated his researches to the Berlin Academy he was totally unaware of Professor Bond's previously published paper on the same subject.'

Mr. De La Rue next adverted to the magnificent monographs of Saturn in 'The Annals of the Astronomical Observatory of Harvard College,' vol. ii, part i. 'Although the chief merit of that elaborate treatise is due to Prof. W. C. Bond, a reference to the initials of the various observers brings out prominently the great part taken by his son, G. P. B. being affixed to 'two very important series of observations connected with the physical history of that interesting planet, namely, in the first place those which in 1848 led to his discovery of the Eighth Satellite, Hyperion, independently of and simultaneously with Mr. Lassell; and, in the second place, observations of a peculiar luminous appearance within the then so-called inner ring, associated with a dusky belt crossing the planet in such a position that it could not be mistaken for the shadow of the bright rings on the ball, which shadow was seen at the same time in its proper place. I need scarcely say that it was the dusky ring of Saturn that was then for the first time noticed, though it was only subsequently recognized as such when, on Nov. 15, 1850, Mr. Tuttle, of the Harvard Observatory, suggested that the existenee of a new ring of a dusky character would explain the phenomena. Mr. Dawes subsequently, namely, on the 25th and 29th of the same month, discovered the same ring independently, with a refractor by Merz, of 6½ inches aperture.' The address concluded as follows:—

'I believe I am right in stating that the only orbit of Hyperion yet published was computed by Professor G. P. Bond, from his own obser-

vations. He has also published an analytical investigation of the question of the stability of Saturn's rings, in which he arrived at the unexpected conclusion that they are in a fluid state.[1]

Among other titles to our esteem for him as an astronomer must be mentioned his share in the adaptation of the great Refractor of Harvard College to the observations of very small stars in zones of declination, the first-fruits of the plan being a catalogue of 5,500 stars near the Equator, forming vol. i. part ii. of the above named "Annals," the observations having been made by himself and Mr. Tuttle.

This plan, it will be remembered, consists partly in recording, by the electric method, the times of transit of stars as they cross certain vertical lines drawn on a piece of transparent mica attached to the diaphragm of the spider-line micrometer. The electric recording apparatus is that originally contrived by the late W. C. Bond and his two sons, in which the movement of the recording barrel is regulated by their well-known spring-governor. On the piece of transparent mica exhibiting the transit lines, is also drawn a series of horizontal lines, 10″ of declination apart, grouped into threes and sixes, to indicate half minutes and minutes, the former denoted by making every third line somewhat longer than the adjoining ones, and the latter by causing every sixth line to extend quite across the slip of mica. By means of a cross-light illumination the lines are made to appear white on a dark ground. "The observer, with the aid of an assistant, whose office it is to record the declinations, magnitudes, &c., has it in his power to give the elements of position and magnitude of each star at the rate of seven per minute, the average frequency of observations being only two in each minute." Since Professor G. P. Bond's appointment to the direction of Harvard Observatory in 1859, these zone observations have been diligently continued; and in his report for 1864 it is stated that the region between + 1° 00′ and 1° 10″ had been nearly completed, and that great progress had been made in the zones between 1° 10′ and 1° 20′.

It is only necessary to read the Reports of the "Committee of the Overseers" of Harvard College, and the accompanying Reports by the Director of the Observatory, to show that the same zeal animates Professor G. P. Bond that was so strongly evinced by his father. The vast amount of work accomplished in the way of observation, reduction of the results, and their publication, is truly surprising, for we must in forming an estimate always bear in mind that the Observatory of Harvard College has very small means at its disposal, in comparison with the magnitude of its undertakings.

There is one claim to recognition which I, of all persons, must not pass over without notice—namely, the first application of photography to astronomical observations; for it was my seeing in the exhibition of 1851 a lunar photograph, which emanated from this Observatory, that stimulated me to undertake experiments in that direction. The first application of photography to the delineation of our satellite and of some of the fixed stars I had attributed to Prof. W. C. Bond, in connection with Messrs. Whipple and Black, of Boston; but I am informed that it originated with G. P. Bond, to whom, therefore, in conjunction with these last-named gentlemen, the merit of this important step must be assigned.

[1] Presented to the American Academy of Arts and Sciences, April 15, 1851.

Professor Bond's latest and yet unfinished, work, is that upon the Nebula of *Orion*, which object is also engaging the attention of Lord Rosse. Professor Bond has already sent to England a proof from a steel plate engraved from his drawing; he has catalogued the stars in and around the nebula with more minuteness than the Russian astronomers; and he has been so fortunate as to discover in the nebula a grand feature which has hitherto escaped the attention of astronomers. This new feature is a great re-entering loop of nebulous matter extending around nearly the whole of the previously known portion, and enclosing, as with a nebulous wall, a large space exterior to the well-known figure. It was discovered by employing with the 22-feet refractor of 15 inches aperture, an eye-piece magnifying 90 times, with a field of 30′; by this arrangement it was seen with great distinctness. It is well known how clearly the low powers of comet-seekers bring out the faint details of comets; and the conversion, so to speak, of the Harvard Refractor into a huge comet-seeker has led to this interesting discovery.

In 1851, Prof. G. P. Bond visited Europe, and his reception at the principal astronomical establishments, and especially at the classical Observatory of Pulkowa, as detailed in the Annals of Harvard College Observatory, vol. i. part i.,[2] may perhaps be regarded as evincing something more than the ordinary courteous welcome due to a stranger. In 1863 he again visited Europe, when I had the pleasure of making his acquaintance, and I am convinced that on this, as on the occasion of his previous visit, Professor G. P. Bond not only gained many personal friends, but also the high opinion and respect of those astronomers with whom he came into contact.'—From *The Reader*, *Feb.* 11, 1865; but somewhat altered and extended after comparison with the publication of the Address in the *Monthly Notices of the Astr. Soc.*, vol. xxv, Feb. 10, 1865, p. 125, received after these pages were in type.

4. *Comet* V, 1864.—A comet was discovered by Bruhns, at Leipzic, on the 30th of Dec. 1864. The following elements were computed by Engelmann, from observations of Dec. 30th, Jan. 3d, and Jan 21st.

$T = 1864$, Dec. $27{\cdot}76616$, $\quad i = 17^\circ\ 7'\ 13''{\cdot}7$,
$\pi = 162^\circ\ 21'\ 55''{\cdot}1$, $\quad \log q = 0{\cdot}047095$,
$\Omega = 340^\circ\ 53'\ 52''{\cdot}6$, $\quad$ Motion retrograde.

5. *Duration of the flight of shooting stars.*—Dr. Jules Schmidt, director of the Observatory at Athens, has communicated to Mr. Haidinger some of the results of his observations during the last eight years upon shooting stars. He has recorded the estimated duration of flight of 1357 meteors, out of about 16,000 seen. The mean duration of those of different colors was, of 846 white shooting stars, $0^s{\cdot}709$; of 361 yellow, $0^s{\cdot}947$; of 101 red, $1^s{\cdot}787$; and of 49 green ones, $2^s{\cdot}685$. The mean of all was $0^s{\cdot}925$. This is somewhat larger than the mean duration obtained on page 203 of this volume. Dr. Schmidt having been accustomed to estimate small intervals of time in astronomical observations, his estimates deserve not a little confidence. We trust that he will classify them according to the hour of the night. His earlier observations were made mostly in the evening. The times of the observations of this series are not specified. We believe that there is a decrease in

[2] Appendix, pp. 158-60, inclusive.

the mean duration in the later hours of the night. Such decrease is distinctly shown by Wartmann's observations in 1838. H. A. N.

6. *Heights of Auroral Arches.*—Mr. B. V. MARSH has obtained data for computing the altitudes of three auroral arches. A fine arch was seen early on the evening of the 16th of January, 1865, and from observations at Germantown, Pa. and Brunswick, Me., the computed altitude is 97 miles. A second arch on the 20th of February was observed at Germantown, Newburyport, Mass., and Brunswick. The Brunswick and Newburyport observations give an altitude of 67 miles; those of Germantown and Newburyport 92 miles. The mean of the two determinations is 79½ miles. A third arch on the 21st of February, observed at Boston and Philadelphia, had the computed altitude of 57 miles. The mean height of the three arches was therefore 78 miles. The data were not very exact in either case, but taken together they are believed to furnish reliable approximations. H. A. N.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Agassiz Expedition to South America.*—On the 29th of March, Prof. Agassiz, with a large corps of assistants, sailed in the steamer Colorado for Rio Janeiro, on an exploring tour in South America. His corps consists of O. H. ST.JOHN and C. F. HARTT to collect fossils and to aid in geological exploration, J. G. ANTHONY to collect mollusks, J. A. ALLEN to collect birds and mammals, G. SCEVA to make skeletons of mammals, birds, the large reptiles and fishes, and Mr. BURKHARDT to make drawings. Prof. Agassiz will devote himself, with native and such other assistance as he may obtain, to the collection of marine invertebrates, yet will have, for his main object, the study of the embryology of some of the remarkable fishes of the Amazon, and investigations with regard to the drift phenomena, or ancient glacial action, in the Andes.

The party is accompanied also by Dr. B. E. Cotting as surgeon, with the wives of Prof. Agassiz and Dr. Cotting, a son of Mr. N. Thayer of Boston, and a son of Mr. S. G. Ward of that city.

The expedition goes first to Rio Janeiro, whence the geological assistants will journey by land north to the Amazon, while the rest of the party, after completing investigations there, will take vessel for the same river. Prof. Agassiz and party will then ascend the Amazon, to the Andes, and, finally, after explorations in the mountains, descend to Lima.

Professor Agassiz at first intended only a visit to Brazil for his health, and proposed to take along one or two assistants to aid him in making collections for the Museum of which he is Director at Cambridge. On mentioning his plan to Mr. Nathanael Thayer, this generous patron of science at once said "Agassiz, go home, find six assistants, and I will pay the bill." The Pacific Mail Steamship Co., hearing early of the projected tour of exploration, immediately tendered to Professor Agassiz and his wife free passage to Rio; and, afterward, on learning of Mr. Thayer's munificent proposition, Mr. McLane, in the name of the Director of the Company, offered to the whole party free passage in the new steamer Colorado, about sailing for Panama *via* Cape Horn. The arrangements were soon completed, and within three weeks after Mr.

Thayer's promise was made, the expedition left New York in the Colorado.

The Secretary of the Navy has given Prof. Agassiz a letter addressed to all officers of the navy whom they may meet, in order to secure for them free transportation when desired. He offered also a government vessel to take them to their place of destination in South America, but they were already provided, through the liberality above mentioned. Every facility may be looked for also from the Emperor of Brazil, who has, for some time past, manifested great interest in the welfare of the Museum of Comparative Zoology at Cambridge.

Great results may be expected from an expedition under such a leader, so ably supported and so well equipped. The explorations will be mainly inland, and therefore the richer in novelties to science.

2. *Temperature of the climate near large bodies of water.*—Mr. J. S. LIPPINCOTT, of Haddonfield, N. J., in an article published in the Second Annual Report of the West Jersey Fruit-Growers Association (1864–5, Philadelphia), attributes the milder climate near the great lakes and the ocean to the atmospheric humidity occasioned by the proximity of water, referring to the principle established by Tyndall that moisture in the air absorbs the heat received from the earth and retains it about its surface, preventing thus its escape into space, and to his views on the subject. He explains in this way why western New Jersey is much less favorable for vineyards than the vicinity of the great lakes or the lakes of New York, situated farther to the north.

3. *Carinthian Lake-habitations.*—Prof. HOCHSTETTER has found evidences of lake-habitations on piles in four of the lakes of Carinthia; namely, those of Wörd, Keutschach, Rauschelen, and Osseach. From the lake of Keutschach, the only one yet particularly investigated, numerous black potsherds, pieces of half-burnt clay, half-carbonized fragments, a whetstone, a portion of a stag's horn, are among the relics obtained.—*Reader*, *Dec.* 3.

4. *A new meteorite from Arkansas.*—Prof. J. LAWRENCE SMITH has received a portion of a new meteorite from Arkansas, consisting of mixed iron and stony matter, which he has under investigation.—*Letter to G. J. Brush*, dated *Louisville*, *Ky.*, *Apr.* 24.

5. *Cancerine.*—A fertilizer, named *Cancerine*, is made on the coast of New Jersey, out of *King-crabs*, a crustacean otherwise called "*Horse-shoe*," and in science *Limulus*. They are dried and ground; and, thus prepared, the material is said to be worth half as much as Peruvian guano.—*Prof. G. H. Cook's Report on the Geol. Survey of N. Jersey*, 1864.

6. *Large mass of Amber from India.*—Sir DAVID BREWSTER has described a mass of amber from India weighing upward of two pounds. It is traversed by veins of carbonate of lime.—*Proc. Roy. Soc. Edinb.*, Jan. 16.

7. *Earthquake at Buffalo, N. Y.*—On the 29th of January last, at 4 A. M., there was a shock of an earthquake at Buffalo.

8. *Louis Saemann.*—The Comptoir Minéralogique et Paléontologique —or establishment for the Sale of Minerals, Rocks and Fossils—of Louis Saemann, in Paris, has been removed to Rue de Mézières, No. 6, where he stands ready to supply on reasonable terms, all wishing to buy, or complete, mineral, lithological, or paleontological cabinets.

9. *Dr. A. Krantz.*—The 7th edition of the Catalogue of minerals, rocks, fossils, casts and models for sale at the large and well known house of Dr. Krantz, at Bonn, in Prussia on the Rhine, has recently been issued, and exhibits great completeness in his stock in all departments, and favorable prices for purchasers. Besides the various other collections, we observe one of 114 models of occurring crystalline forms of various minerals, in wood, well labelled, with reference to standard works, for 16 thalers; and another of 675 forms, for only 120 thalers (the thaler being equivalent to about 75 cents).

Obituary.—Thomas B. Wilson.—The scientific world has sustained a severe loss in the death of Dr. Thos. B. Wilson, the late President of the Academy of Natural Sciences, in this city, which took place on the 15th of March last, at his late residence in Newark, Delaware. Dr. Wilson, who was a native of Philadelphia, has for many years devoted himself to the encouragement and promotion of zoological science, especially in connection with the Academy of Natural Sciences, an institution which, mainly by his energy, ability and princely liberality, has been raised from comparative mediocrity to an equality with the leading kindred institutions of the Old World. The superb collection of Birds, which ranks as the third in importance in the world, and the invaluable Library of the Academy, are but a partial evidence of Dr. Wilson's unostentatious munificence. Every department of the institution bears his mark and will feel his loss. He has also contributed largely of late to the Entomological Society at Philadelphia. Although his residence had been removed to Newark, Delaware, half of his time was regularly spent in his native city and occupied with his favorite pursuits. It is difficult to estimate the value of such men to the cause of science, or the loss which a community sustains when their labors are cut short.—*Daily Evening Bulletin, Philadelphia, March* 21.

George H. Emerson.—Mr. George H. Emerson, a young chemist of much promise, originally from the city of Hartford, Ct., died at Greenfield, Mass., after a long and painful illness, on the 28th of December last, at the age of twenty-seven. Mr. Emerson is the author of a memoir "On crystals and precipitates in Blowpipe Beads," presented to the Boston Society of Natural History, an abstract of which was published in volume xxxvii of this Journal (p. 414). Having discovered that nearly all those earths, or metallic oxyds, which produce an opaque bead on flaming, give rise, with proper manipulations, to definite and characteristic crystals, he examined, with great labor and care, a large variety of substances, and carried his investigations so far as to determine and describe the appearance of the crystals or precipitates in different fluxes. His researches—which he regarded as but just begun,—promised to contribute much to the progress of the science of Blowpipe analysis. But he was compelled to discontinue them by his failing health. He had been a student of Chemistry in Prof. Cooke's laboratory at Cambridge but one year when he obtained his interesting results. But the enthusiasm and activity with which he applied himself would have severely tried a stronger constitution than his. Having sought to improve his health by a journey to Florida without avail, he died at Greenfield a few months after his return.

Mr. Emerson belonged to that rare class of men, who perpetuate their good works, and after death remain useful to humanity. By his will he gave his whole estate to the endowment of four scholarships for the relief of needy and meritorious graduates of the Lawrence Scientific School of Harvard College. Such a grand and simple life of devotion needs no drapery of euphonious words. He loved science, and gave for her advancement all he possessed, even his young life. A. H.

WILLIAM J. WALKER.—Dr. William Johnson Walker died on the 2nd of April last, at Newport, Rhode Island, in his 76th year. He had been eminently skilful in surgery and the treatment of disease, but quitted the practice of his profession about twenty years since. He was a liberal patron of science and of education, and not less munificent in his gifts than in his bequests. During his later years his gifts amount to four hundred thousand dollars, of which the Boston Natural History Society had a share. In his will he bequeaths one million of dollars (out of an estate of about one and a quarter millions) in equal parts to the Boston Society of Natural History, Amherst College, Tufts College and the Institute of Technology at Boston.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Naturalists' Directory.* From the Proceedings of the Essex Institute, Salem, Mass. F. W. PUTNAM, Editor.—Advance sheets of Part I of this Directory have been received. It forms part of the April number of the Proceedings, but has separate paging so as to be bound by itself. This first part contains the names and addresses of the naturalists of North America and the West Indies, together with their special departments of science, and other useful information. The publication of this work is to be continued quarterly until completed. The next number will contain the names of foreign naturalists, with their addresses and special branches of study, so far as they can be ascertained. In the part already printed the names have been arranged alphabetically for greater convenience, but in future numbers it is proposed to arrange those of each country according to their special departments.

The utility and convenience of a work of this kind must be obvious to every one interested in Natural History, and the first number gives assurance, by the great labor and care that has been bestowed upon it, that the entire work will, notwithstanding the inherent difficulties of the task, be rapidly and successfully completed, and thus supply a want that has long been felt. A. E. V.

2. *The Social Science Review: a Quarterly Journal of Political Economy and Statistics;* ALEXANDER DELMAR and SIMON STERN, Editors. Vol. I, No. 1, January, 1865. 96 pp. 8vo. New York.—The subjects discussed in this first number of the Social Science Review are—Government; Mr. Fessenden's Report; Herbert Spencer on Social Statics, &c.; Mr. About's Progrès; The limits of Political Economy.

3. *The Preparation and Mounting of Microscopic Objects;* by THOMAS DAVIES. 144 pp., 12mo. New York: Wm. Wood & Co., 61 Walker street.—In accordance with the author's aim, as expressed in his Preface, this little manual supplies a want long felt, and supplies it well,

giving the methods of preparing and mounting (and to some extent, also, of collecting) specimens for the microscope that are in general use, along with the results of his own experience; and with such explanations of the rudiments of the art as may be required by a beginner, as well as those details that the advanced student might need. The subjects of the chapters are: I. Apparatus; II. Preparation and mounting of objects "dry;" III. Mounting in Canada Balsam; IV. Preservative liquids; V. Sections, and how to cut them, with some remarks on dissection; VI. Injection; VII. Miscellaneous information on objects of interest for the microscope.

4. *Trübner's American and Oriental Record.*—The Record, published in London by Messrs. Trübner & Co. (60 Paternoster Row), of which the first number made its appearance on the 16th of March last, is to be "a monthly register of the most important works published in North and South America, India and China and the British Colonies, with the occasional addition of notes on German, Dutch, Danish, French, Italian, Spanish, Portuguese, and Russian books." Its object is stated to be two-fold: *first*, to form a medium of communication between American and Oriental authors and publishers and the English public; and, *secondly*, to make American and Oriental works better known in Europe. It has therefore a special interest to American authors and publishers, and to the American people generally. Coöperation from America is asked for, and especially early intimation of publications in contemplation, with a mention of such particulars in each case as the trade and the reading public demand. The house of Trübner & Co. has long dealt largely in American works, and done much toward extending their circulation through Great Britain and Europe. The first number of the Record runs to 24 pages small 4to. Price 6*d.*

5. *American Journal of Conchology;* edited by G. W. Tryon, Jr.—The first and second numbers of the American Journal of Conchology have appeared, and by their contents promise much for the progress of the science of both recent and fossil shells. The numbers of this quarterly contain 96 pages each; the first is enriched by 9 plates of shells, and the second by 8, several of them colored. The price per number is three dollars, and per year ten dollars. These numbers contain articles on Tertiary fossils by T. A. Conrad; and on recent shells by S. S. Haldeman, T. Bland, W. G. Binney, W. Stimpson, C. M. Wheatley, A. D. Brown, J. G. Anthony, and the Editor, besides "Reviews and Summary of Conchological Publications" and "Scientific Intelligence."

6. *Woodward's Country Homes;* by G. E. & F. W. Woodward, Architects. 166 pp., 12mo. New York: G. E. & F. W. Woodward, 37 Park Row, office of the Horticulturist.—This neat little volume is illustrated by numerous cuts representing dwelling houses of various tasteful styles, and plans for their construction, and is well calculated to distribute through the country, taste in domestic architecture and some knowledge of the ways of exhibiting it.

7. *The New York Medical Journal.* Vol. I, No. 1, April, 1865. 88 pp. 8vo. Miller & Mathews, New York and J. B. Lippincott & Co., Philadelphia. $5 per year.—This new monthly is to be sustained by the highest medical and surgical talent of the country, and promises to be the leading Medical Journal.

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